The field of strategy : In search of a walking stick

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After more than twenty years of commitment to the field of strategy, and with respective doctorates in business policy and decision theory, we are still wondering if the academic field of strategy exists. There have been excellent and exhaustive reviews by authors such as Rumelt, Schendel and Teece (1994), Whittington (199x); Hitts (199x), and Pettigrew, Thomas and Whittington (2001), and many others, but most take note of the extreme diversity of the research, and shy away from providing a convincing framework to clarify what the field is all about. To develop this theme further, at the 2002 Academy of Management meeting in Denver, one of us asked the same question of some of the pioneers in the "Strategy section."[1]

The answers were somewhat tentative, to the surprise of everyone attending including ourselves. Apparently, we are not even sure if we agree on the same definition of strategy. There is no lack of available definitions. Anyone with any claim to recognition in "the field" has provided one. Andrews (1987) answer has traditionally been considered one of the more complete. Yet, all these definitions remain so vague and so general that they provide little help when one is searching for an academic field. Most of the definitions are either descriptive of the process or tautological in nature, saying basically that strategy is the set of decisions that makes an organization successful or strategy is what top managers do (Bower 1980). Such a lack of clarity in the conceptual base makes

[1] . Jay Barney, Arnold C. Cooper, Henry Mintzberg, James Brian Quinn and Dan Schendel were invited. The latter was not present.

the search for meaningful research findings and cumulative research, and hence theory construction difficult (Camerer 1986.) Rumelt (1983) was not quibbling when saying that in his regression models to explain diversified firms's performance, most was explained by the residual, and he suggested that the residual was actually strategy.[2] Trying to probe further, one is faced with multiple strategies, some corporate, others business, still others functionally-related. To each author's idiosyncratic outlook, corresponds his definition. And to totally compound the problem, each of the functional fields of management has its own strategy arm. Marketing strategy, financial strategy, production strategy, etc, have now their own journals and set of probing scholars. This proliferation inevitably leads to the question: What is strategy anyway?

The more convincing definitions are also the more troubling for researchers. Paul Cook argued that Strategy is made up of "messy, unsolved and perhaps undefined problems of importance characterizing business management." Bower (1980), in his presentation to the 1980 Academy of Management meeting argued that "the charter of business policy is to focus on the life and death issues of central interest to top management… to help top management to deal with these issues effectively, profitably, and morally." He went so far as to say that anything that can be made orderly and systematic should be left to the functional areas and considered out of the field of strategy.

The question of whether there is a field of strategy is important. It is a defining issue in strategic management. In this paper we argue that there is a field of strategy, but it is still

[2] And, in his 1991, paper (Does industry matter?) he isolates strategy as the residual, with a relatively small effect on performance after firm and industry effects are taken into account.

underdeveloped despite an incredible surge of research in the last twenty years. We intend to show that most research does not really address the field defining issues. Furthermore, the ontology of the field is misunderstood, the epistemological issues are confounded with those of the more established social sciences, and the methodologies that dominate the field are not appropriate. To do that, we first go through a historical discussion of the field's knowledge development, describing in the process the approaches that have dominated research, teaching, and practice. We argue that there is a drift to the traditional social sciences that can be explained by the complexity of strategic issues. Such a drift has not helped deal with the critical strategic management issues. It has increased confusion, among practitioners and academics alike. We argue further that the real challenge is to invent a new science, a meta-science, a science of complexity,[3] with new methods, and new concepts without which the many findings that have been reported in general management journals would lack appropriate theoretical anchors. To suggest the nature of such a science, we shall go back to the issues that have dominated debates among early general management scholars.

The nature of strategic issues

About thirty years ago, the early courses in strategic management in business schools emphasized the issues of coordination among the various activities of the firm and relied upon case studies, often developed at the Harvard Business School, to supply rich descriptions of strategy- related issues. Integrating decisions across functions and levels was seen as a huge and sometimes elusive challenge. Students were socialized to the

[3] See for example, Ralph L. Stacey, Strategic Management and organisational complexity, Pearson Education, 4th edition, 2003

challenge early in the MBA program, with the study of a very simple case. The situation of Midway Foods was typical. [HBS #] Midway was a small firm, with 100 employees, located in Chicago. It produced a range of sweets, mostly chocolate bars. The A case was so simple that most students had a hard time keeping motivated. In it were described the industry, the firm history, its nature and operations, its purpose and its marketing strategy. The case ended with a short discussion with Kramer, the company president. Most students wondered what was expected of them in such a simple and general case. Midway was however peculiar, because of Kramer's concern about defining the business and being consistent with such a definition.

The students were then exposed to additional information about Midway's management. The B case brought new challenges, with the decision to acquire a competitor. Should Midway do that? The C case highlighted the different perspectives of the four functional departments. The students discovered that each department has its own mission, operating methods, and operational problems. More important, each functional manager had his own managerial and personal philosophy. The D case was a meeting showing how difficult it was for these managers to work together. Finally the E1 and E2 cases presented the president, the person and the top executive responsible for coordinating a whole that now seemed much more complex than it seemed at first, facing decisions that could make or break Midway Foods.

Most teachers of strategic management have conducted discussions of the Midway type, to expose students to the complexity of top management issues. It is not so much the

operations or the marketing or the finances or human resources or power, motivation, leadership, or many other issues, but the combination of all these that make the top management job so intractable and sometimes confusing.

An academic field of strategy should probably, at least, provide answers to the bewildered students and practitioners facing similar and, more realistically, much more complex issues than those described in Midway Foods. Some answers have been attempted and have gone from those based on the early corporate strategy concept to those based on the more sophisticated research. Let us look at the underlying conceptualizations in sequence.

Conceptualizing strategy issues

The conceptualization of strategic issues has taken two broad routes. The first could be called Holistic, and intended to stay close to the phenomena (Roethlisberger, 1977). The emphasis was on global theories to better understand the challenges of managing the organization as a whole, and on top management normative roles (Barnard, 1938; Selznick, 1957). The second route, more contemporary, could be called Analytic, has followed the traditional scientific methods and approaches, by decomposing problems and focusing on identifying relationships that can help understand the nature of strategic phenomena and predict their behaviour (Thompson, 1967). Historically, the first route dominated the field until the end of the 1970s, while the second has emerged out of Thompson's synthesis (1967) and his efforts at *ASQ*[4] to encourage the development of an

[4] .Administrative science quarterly

"Administrative science," and has taken up much of the publication space since the launch of the *SMJ*[5].

The Holistic Approach

The holistic approach can be traced back to the 1930s and 1940s heated debates around the "Scientific management" viewpoint of F.W. Taylor and the "Human relations" movement (Roethlisberger 1968; Roethlisberger and Dickson 1939). The simple and surprisingly effective formula that Taylor came up with was showing its precise limitations. At the same time Mayo's intuitions (1933) suggested that such a formula was just too simple to resolve all the problems of collective action. Mayo's interpretations of the Hawthorne results (1945) brought new light on the complex and changing nature of humans involved in collective endeavours.

Mayo's suggestions generated an unprecedented wave of multidisciplinary debates. The most representative of this flurry of intellectual activities was W.J. Henderson's faculty seminar on "practical sociology", held at Harvard university, in the early to mid-1930s. Henderson forced everyone to take into consideration collective action in its entirety. Because of his training as a physician, he succeeded in some cross-fertilisation between, on the one hand research practices in the field of medicine and in the various disciplines that study organizations, and on the other hand, broader theoretical works. Barnard's *The functions of the executive* (1938), was one of the better known outcomes.

[5] .Strategic management journal, which was launched in 1980, has tended to focus more on analytic then processual studies of strategy (Furrer et al, 2001)

Barnard's formidable attempt at reconciling scientific management and human relations re-conceptualized the problem of collective action. It proposed a new, quasi-revolutionary vision, and at the same time imposed a research framework that was more clinical than analytic (Roethlisberger, 1977). Barnard suggested that theoreticians, dominated by Taylor's views, had looked at the problem of collective action upside-down. He put it back on its feet, by proposing his theory of cooperation, which helped redefine authority and human behaviour in collective action, and proposed ways to manage this newly conceived collective action. His work is still very influential. Barnard was the first of a long tradition of scholars who have emphasized the study of the organization as a whole. The contributions of Simon (1945), Selznick (1957), Crozier (1963), and many others have announced the discovery of strategy as the main tool of executives in charge of keeping the organization in balance.

The basic hypothesis of the holistic approach, a systems view of the world (Forrester ; Morecroft ; Checkland) is that collective action cannot be understood, if it is decomposed into parts to be studied separately. As Reality is complex, it is more appropriate to study it in its totality, thus not only study the parts together, but also in their relationships, even if the result is an incomplete and imperfect understanding. The resulting theoretical development was conceived to be utilitarian, always temporary, but an essential help for those willing to face complexity (Henderson 1970).

Roethlisberger (1977) described how Henderson's conceptual scheme of a social system, inspired a whole generation of scholars and practitioners. Henderson (1970) drew on the

principles of Hippocrates. According to him, "interactions between persons" were the essence of collective human action. They involved mutual adaptation and skill and also strong sentiments. "To understand such complex phenomena, both theory and practice were necessary… and the method of Hippocrates was the only method that succeeded widely and generally:

> The first element of that method is hard, persistent, intelligent, responsible, unremitting labour in the sick room, not in the library… The second element of that method is accurate observation of things and events, selection guided by judgment born of familiarity and experience, of the salient and the recurrent phenomena, and their classification and methodical exploitation. The third element of that method is the judicious construction of a theory – not a philosophical theory, nor a grand effort of the imagination, nor a quasi-religious dogma, but a modest pedestrian affair or perhaps I had better say, a useful *walking stick* to help on the way – and the use thereof. All this may be summed in a word: *The physician must have first, intimate, habitual, intuitive familiarity with things; secondly, systematic knowledge of things; and thirdly, an effective way of thinking about things.* (p.67)

The characteristics of Henderson's scheme are thus[i]:

1. The need for a conceptual scheme for the purpose of investigation;
2. The conceptual scheme is a matter of convenience and utility, not truth or falsity;

3. It is a way of thinking to be practiced, not just talked about;
4. It is to be practiced in relation to a class of phenomena. It is not a multipurpose instrument;
5. It is to be used as long as it is useful; and
6. One should be ready to let go when a more useful way of thinking takes over.

The manager is in a situation that is similar to that of Hippocrates's physicians. His situation may at times be even more confusing. Students working on Midway, for example, realize very fast that they need an instrument to make sense of the multiple challenges involved in managing a whole organization. That is how the concept of strategy was introduced. It became the *walking stick* that helped a generation of students and managers find their way in the multiplicity of factors and confounding variables. The concept of strategy, as a walking stick, dominated our thinking for several decades. It has faced with some success scientifically-minded criticisms because of its usefulness as a powerful problem solving theory of decision (Bower, 197x). But still, scholars felt unhappy with such a primitive instrument that was not amenable to positive research. The search for a new, more science-based walking stick was in the works (Schendel and Hofer, 1978).

The Analytic Approach

The analytic approach has taken over from the holistic approach in the 1970s. It was based on the premise that the concept of strategy, as defined then, had become obsolete and should be discarded. It was argued that despite its systemic characteristics, the holistic approach did not allow the generalizations and the predictions that both

practitioners and researchers were looking for. Under pressure from the more advanced social sciences, such as economics, sociology and psychology, researchers in general management have re-examined the field's ontology.

Simon (1945), but more importantly J.D. Thompson (1967) were precursors and the source of inspiration of the analytical movement. Contingency theory, general and global in nature, was the natural vehicle to go from holistic statements about organizations to a more positivistic approach to check cause and effect relationships (Venkatraman and …, 198x). Moreover, Thompson's chapters in his *Organizations in Action* decomposed the problem of organizations and served it in the form of ready to be tested propositions. This formidable theoretical Sum was an exciting research agenda, which was communicated through *Administrative Science Quarterly* to a generation of young and bright scholars.

Thompson has succeeded beyond his dreams. Research on each of the elements described in his book has literally exploded. A look at the main general management journals shows an increasing tendency toward specialization and the testing of simple binary relations. Dery (1996) has shown that in the 1980-1990 period the *Strategic management journal* articles are increasingly quantitative and are dominated by systematic and formalized analyses inspired by industrial organization economics. Furrer et al (2002) reinforced this finding in another extensive study of research in SMJ from 1980 – 2000. In general, as a consequence, following the lead of economic statisticians, the field became and is mostly

characterized by quantitative univariate or multivariate analyses, and by the search for "proxy" variables, to explain the behaviour of organizations.

In their attempt to provide some structure to the field, Schendel & Hofer and their associates (1978) in the Strategic Management Society Founding meeting actually added momentum to the atomization process. By proposing a vision of the field that is an assemblage of theories and methodologies, they recognized and institutionalized atomization. This was reinforced and confirmed again in the 1990 Fundamental Issues in Strategy Conference in Napa, CA (Rumelt, Schendel and Teece, 1994.), as the authors start off their book with a provocative statement: "What will most benefit strategic management, we suggest, is not a unifying paradigm, but the articulation of fundamental issues underlying the field and a refocusing of research to confront them" (page 1).

Ever since Thompson, and gathering momentum with the Strategic management society, the amount of knowledge generated has been considerable. However, as results accumulated, so did inconsistencies and contradictions (Venkatraman and …, 198x; Prahalad & Hamel, 1994; Guedri, 2003). These have been topics of multiple debates, both methodology and content-based, reflected in many of the SMJ special issues (Schendel, 1994). Furthermore, it becomes more and more difficult to come up with a synthesis within any of the sub-fields or conceptual trajectories. A synthesis of the whole field seems just out of question as Schendel states in an introduction to the Summer 1994 special edition on The Search for New Paradigms: "The Guest editors report much disagreement about models, methods, assumptions, issues and challenges, and little

agreement…The state of strategy during the 1990s, can aptly be described as "the best of times and the worst of times"" (p.2). Prahalad and Hamel had also stated: "We believe this turmoil in the field, in research and practice, is a reason for optimism… Many of the assumptions that were embedded in traditional strategy models may be incomplete and/or outdated as we approach the competitive milieu. We will argue that the need for strategic thinking and behavior among managers has never been more urgent".

Yet, most scholars in the field are uncomfortable when submitting a manuscript to a strategy journal: The best contributors are also well recognized contributors to discipline-based fields such as psychology, sociology, economics and political science. This again begs the question of what is strategy. And again, dominate the idea of a crafted rather than scientifically conceived assemblage, whose complexity has increased with the development of knowledge and the reduction of their relevance to an increasingly complex reality. The old idea of a walking stick, this time to make sense of all this seemingly incoherent knowledge, suddenly comes back to the fore.

In Search of a New Walking Stick: Science in Strategic Management

Thompson asserted forcefully that:

> No useful theory can rest on the assumption that everything is unique. It is probably inevitable that the early history of a scientific endeavour will be characterized by the opposite assumption, and by the search for universals… the discovery of universal elements is necessary, but alone it provides a

> static understanding. To get leverage on a topic, we must begin to see some of the universal elements as capable of variation…
>
> The economist, sociologist, political scientist, or social psychologist will each find that I have overlooked refinements and intricacies of concepts that he knows well. I have done so deliberately, in order to achieve generality across typical categories of organizations… My focus is on the behaviour of organizations.

Thompson recognized that the questions with which, in situations of complexity, practitioners and researchers alike have to wrestle have neither the level of elegance, nor the structure that the purist hope they would. Yet the researcher cannot be content with case analyses, always situational and hardly amenable to generalizations.

Thompson did not ignore the strategy problem. In the last chapter of his book (1967), widely considered one of his most significant contributions, he provides a striking expression of the nature of strategic management. In particular he emphasizes the need for co-alignment:

> The basic function of administration appears to be co-alignment, not merely of people (in coalitions) but of institutionalized action – of technology and task

> environment into a viable domain, and of organizational design and structure appropriate to it. Administration, when it works well, keeps the organization at the nexus of the several necessary streams of action. Paradoxically, the administrative process must reduce uncertainty but at the same time search for flexibility.

Following on Thompson's steps, quantitative, more precise research, generally focused on interactions between a limited number of variables, dominates the field. There are exceptions as some conceptual developments were holistic in their approach (Chakravarthy and Doz, 1992). These could be clustered into four groups of research : (1) Chandler (1962)'s study of American firm growth and the related strategy-structure relationship, reinforced by such works as Galbraith (1977), Mintzberg (1978), Rumelt (1974) et Scott (1971); (2) Bower's study of the resources allocation process in large diversified firms, followed by many others of which Burgelman (1980); (3) Miles and Snow's (1978) study of organizational strategic adaptation; (4) finally, the contributions related to Montreal's school of configurations (Miller 1996). Although more attuned to the realities of the nature of the co-alignment that Thompson talked about, they are increasingly pushed back to serve as an exotic background or reinterpreted to be used as frameworks for content-based quantitative research.

Reality is given by a look at the Strategic Management Journal's index of topics, for the 1990s. It shows more than a thousand different topics addressed by less than a thousand

authors. Assuming that the topics as expressed by the authors are a reliable indication of what is being presented, such a variety suggests the Babel syndrome, that authors are barely talking to each other, and a closer look shows that the field is spread all over traditional disciplines or functional areas. More specifically, of all the articles examined in the ten years, about 60% can be related to specific disciplines. Economics alone represents the bulk of all these articles. Indeed, Furrer et al (2002) in their review of SMJ research over the 1980-2002 period, use very broad categories to be able to group the topics of the articles published. Arguably, each category corresponds to a concatenation of academic fields.

Many cademics, we mentioned Schendel (1994) and Prahalad & Hamel (1994), representative of many others, believe that this is acceptable, even a stimulus to research and creative practice. If that is the case, 25 years after the creation of the Strategic management society, we have no indication of its positive effects on the practice of strategic management. Prahalad and Hamel (1994) recognized that "Even well-known consulting firms, such as McKinsey and Boston Consulting Group (BCG), who built their reputations on strategy consulting, started to deemphasize their strategy focus… Academic disillusionment with the value of strategy literature and schools of thought, while not as widespread, followed quickly" (p. 5).

imagine a practitioner, concerned about managing a whole organization, confronted with the strategic management research results. What would the findings mean to her ? It is hard to say. There are almost no syntheses of these studies, or accumulation of findings from different studies researching the same topic; even if occasionally there are furious

debates on the nature of the field or on the nature of strategy (see for example the Mintzberg-Ansoff angry interactions in 1990, 1991). When it comes to teaching, academics are unable to provide a common meaning to all these findings. An examination of the textbooks used in MBA programs shows an incredible diversity. Besides the classic Harvard business school's text and cases, and a few others, which are still concerned about providing a synthesis (Johnson and Scholes, 2002, Hafsi et al, 2000), most others are specialized, if interesting, views on strategic management, some of which have been popularized by well known gurus like Gary Hamel and C. K. Prahalad, Tom Peters, Michael Porter and Peter Senge.

We are back to the situation of students trying to make sense of Midway. The problems are easy to state, yet difficult to deal with The actual behaviour of organizations (business organizations in particular) is the result of combinations that cannot be predicted by the existing science. The paradox is that we need to accept the differentiating logic of elements without losing sight of the whole, an old problem for which the old concept of strategy was much better suited than the level of sophistication of today's theories.

In our opinion, the tragedy (in the operatic sense) of the field is precisely that academics are no more able than practitioners to live with the paradoxes of reality, with the nuances that make strategic management what it is. The search for clear-cut answers destroys the essence of the field. The search for generalizable knowledge leads to the search for deep understanding of the behaviour of those elements of reality that can be isolated or modelled, knowing that this process of segregating and separating the various factors is

not even a second best, in this case could be said a “first worst,” that pushes reality away, and sometimes pushes away from the understanding of strategic management phenomena. Since little is know in strategic management, one would expect more attention to skills or clinical knowledge. Yet, the trend favors analytical knowledge, even if often irrelevant.

So Where is the Field of Strategy?

Having said all that, we are back to our first question: What is the field of strategy? Is it deep research on a limited number of variables and their effects? Is it the reporting of gross if crafted experiments by practitioners, their systematic examination, classification and search for patterns or heuristics ?

First, let’s say what the field is not. It cannot be reduced to specialized analytical research similar to those of the disciplines, for a simple common sense reason. At best, what would then be the difference between the strategy researcher and his colleague of the disciplines? At the worst, wouldn’t that be a way to sneak away from the more rigorous scrutiny of researchers in those disciplines? The specialised contributions should probably be considered contributions to the related disciplinary field.

The strategy field cannot be reduced either to the strategic approach of the practitioner. The latter is entitled to believe that academics are probably less qualified than he is to articulate such an approach, since they do not practice it, nor know well enough the reality to which it is applied. The search for knowledge in strategic management should

therefore follow a different path. A meaningful approach should reconcile the findings of research in the disciplines with practical concerns. The meaning and relevance of academic disciplinary research for the improvement of practice are not obvious. First, disciplinary research results do not apply directly, but in combination with other findings. Second, applying them requires some familiarity with the phenomena concerned.

As we said earlier, this is very similar to what happens in medicine. The medical researcher is faced with the same difficulty. In medicine, it is accepted that the researcher can belong to a discipline, while the practitioner, with the help of his association, is in charge of translating to improve his practice. And even there, in actual practice, the physician is often confused when faced with the multiplicity and sometimes contradictory findings of research. For example, are some specific foods or drugs good or bad for health, for whom, and in what circumstances ? These are daily questions to whom no one has a clear answer. What is a scientific fact becomes slowly a bureaucratic decision made by professional associations under the scrutiny of government offices and other stakeholders.

We believe that, in strategic management, the variety of relevant phenomena is even greater, because the researcher is not only concerned about the individual's health, but also by the collective action of numerous individuals. In other terms, compared to medicine, we are still beginners, at the age of mystery, not unlike that of primitive tribes. We cannot even fear to lose our credibility. We have none, even though, as gurus, sorcerers or prophets, we may still impress the naïve (Prahalad & Hamel, 1994).

The object of research in strategy does however exist. It can be practical, both heuristic and methodological. Its heuristic nature is double faced:

1. Discovering, through the practice of managers, unusual regularities and patterns, specifying them and submitting them to analysis and debate. This systematic effort is scientific, because it helps describe and thus discover the varied nature of the phenomena to which managers are exposed. It can also help conduct an orderly discussion of the reasons that explain results.
2. Experimenting, modelling in an attempt to predict the behaviour of organizations.

In both of these, it is possible to call on the findings of the disciplines in an organized search for meaning. The nature of this work is to be compared to what has been done for some of the natural sciences, for the understanding of complexity and the development of scientific approaches to deal with it (Waldrop, 1990). It is indeed a relevant example for us, because strategic management phenomena, deal with unclear, non linear cause-effect relationships. As a consequence, the combination of research results to come to bear on reality, can be neither direct nor linear, but creative in the scientific sense of the word.

Similarly, the methodological nature of the strategic management researcher contribution is fundamental. In Strategy, we are not dealing with a traditional science but with a science of complexity. The approach that leads to propositions and conclusions is itself a stake and a result of research (complexity…). Thus, confronted with small samples, but with rich data, informed with a multiplicity of partial disciplinary research results, the

strategy researcher has to invent his approach and rationalize it, then build the needed heuristics, and constantly adjust both to reach convincing results.

The researcher in strategy is therefore a formidable intellectual. He must have the encyclopaedic knowledge needed to comprehend the results of the normal science, and the familiarity, the intimacy with the phenomena, that stimulates creativity in search of explanations and heuristics. His problem is similar to that of a management practitioner with two differences: (1) the practitioner does not have to justify intellectually his decisions, but (2) must survive to the consequences of his decisions. However, like the practitioner, the academic muddles in the dark.

To respond to the task, practitioners and researchers need a walking stick. Their sticks are of the same nature. However, for the practitioners of strategy the objective is to guide decisions and their implementation, while for the researcher in strategy the objective is to develop the approach to find a convincing meaning to strategic management phenomena and generate heuristics that can inform and guide practice.

A walking stick for researchers in strategy

The development of a general framework to give meaning to specialized research and also to generate research specifically designed to deal with reality in all its complexity calls for researchers who are generalists, concerned about syntheses, such as those offered by Bower (1970), Miles and Snow (1978), Miller (1996) or Mintzberg. Walking sticks in strategy research should be a creation of the researcher. His goal is to represent

reality by taking into account disciplinary research, but going beyond it in a search for convincing explanations of reality. At this point, we should be reminded of Henderson's recommendations about the utilitarian character of such a construction and its temporary nature.

Given the complexity of strategic management reality, it should be clear that the most appropriate frameworks are not those that emphasize content of strategy, because strategy is by definition necessarily situational and contingent. Rather, they must help reveal the mysterious mechanisms that lead to the formation of strategy and to its evolution over time. Patterns of strategic content can however be useful segments of a process based conceptual framework.

To illustrate this idea of a strategic management walking stick, we could have taken the example of the traditional Concept of strategy. We have decided to provide a different example to make its development transparent. In a search for meaning in strategy related research, we started off with research up to 2000. In a comprehensive literature review, we developed a framework that tries to encompass the findings. The resulting model suggests that strategy research can be clustered into two basic groupings:

1. Intellectual aspects, intended to conceive the mechanisms by which coordination and convergence of collective action is ensured;
2. Practical aspects, designed to achieve the intended convergence.

The intellectual aspects have been grouped into five different sections, which are somewhat different from the six dominant themes reported in the review by Furrer et al (2002) of research in SMJ from 1980-2000

i. Strategy as a leader's statement

ii. Strategy as a community's statement

iii. Strategy as a guiding track

iv. Strategy as the building of competitive advantage

v. Strategy as a relationship to the environment

The intellectual aspects are of course intimately related to the practical aspects of individual and collective actions and their effect on how the organization functions. When many are to act together there is a need for ways to keep the action orderly, through goals, structure and systems (planning, performance measurement and control, rewards and punishments, training, resource allocation, management information and communication).

Structure and systems give life to decision processes that are unique to each firm. As mentioned by Barnard (1938), managing these processes is a critical function of executives. It is through the combination, either rustic or savant, of management mechanisms that strategy is revealed and given life.

Ultimately, action is a series of decisions (Simon, 1945). How are decisions made ? What motivates them ? How are decisions integrated in specific situations? How are managerial mechanisms used to encourage or orient decisions? Are some of the questions that come out of the study of decision and planning models as integrating tools.

Finally, taking into account all these aspects, strategy can be seen as a revelation of the voluntary, systematic, rational, objective, but also emotional, sentimental, affective, intuitive action. To represent strategy as a model or theory of action, one has to include in it all the aspects mentioned and associate them in such a way as to recognize how they combine to generate action. The representation proposed here is as in Figure 1. The intellectual dimensions of strategy are represented as layers of reality, related to life through the mechanisms that lead to decision and action, which in turn feedback to affect the intellectual dimensions.

This construction has been described elsewhere ( ), and is drawn from a synthesis of the literature. The resulting model is not dissimilar from what has dominated the field until the new strategic management emerged out of the Strategic Management Society and its journal, SMJ. This model is a walking stick for the researcher and we have used it to help students make sense of the field. It can and has also been used by practitioners, especially when they are in the process of experimenting. Also the model can be easily related to the disciplines that contribute to strategic management research. All this makes it a useful guide for researchers to make sense of incoming research and practitioners to use it.

This model is just an example. It is not the only one available. Bower (1970) for large diversified firms, or Miles and Snow (1978) for less complex situations have provided models that have been an inspiration and a way to structure many investigations. Similarly, Mintzberg's work (1978) and the configuration school contributions, although focused mostly on content, have provided a map to deal with the strategy-structure configuration. Many others qualify in this search for strategy as a walking stick, and our call is to give them greater space and legitimacy in the knowledge adventure.

Conclusion: Avoiding Drift to Contribute to Scientific Development

General managers cannot manage a firm for sustainable performance and survival if they focus on the specialized activities of the functions. They are forced to integrate them to provide meaning and justify these functions development. The concept of strategy is the traditional instrument to help them do that. Such a model remained a "down to earth" instrument, since its development in the 1960s. Its general character has been at the source of its explanatory power and of its capacity to guide collective action.

The raison d'être of the field of strategy can be stated as being: helping through heuristics and creative methodologies to the understanding and transformation of reality. As research moves away from such a goal, strategic practice is left alone and the field dies. Managers may start losing interest in theoretical developments which help neither to explain reality, nor to facilitate action designed to influence it.

Such a situation is the source of concern about the future of Strategy as a field of academic teaching and learned research. From the traditional academic point of view it is much more legitimate to simply go back to the disciplinary research, since strategy research looks more and more like a pale reflection of these. It takes a deliberate resistance to innovate and resist the dominant isomorphic pressures (DiMaggio and Powell, 1983). We should not only conceive our research better so that the understanding and transformation of reality is not relegated to the background, but becomes center stage. Also, we should work at developing more and better ways to integrate and reconcile research findings.

These ways to reconcile and integrate research cannot be completely different from those that help managers in their practice. The scientific field of strategic management of organizations should be devoted to the development of the conceptual frameworks needed to bring together research and practice. As vehicles for policy thinking and dialogue (Thomas 1981) the model described in this paper is but a simple example of the direction that research should take. The most powerful models should emphasize process, leaving the specialized works to deal with content.

The field of strategy has no future except close to reality. Its methods are those of complexity, often qualitative and when possible experimental, simulating reality to better represent its dynamics. Despite the fact that practice and theory are not well-connected (Bettis, 1991) and that strategic management has not yet produced the volume of useful

results expected by management, perhaps one day, these methods will be powerful enough to really help managers predict such dynamics.

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[i] . Much of Henderson's argument is about problem framing and formulation given signals about the relevant phenomena; (see, for example, Porac and Thomas (2002), on cognition and strategy and Kahneman and Tversky's work (***DATE?)*** on the framing of decisions and the psychology of choice.)