For about eight years now, a group of us at McGill University's Faculty of Management has been researching the process of strategy formation. Defining a strategy as a pattern in a stream of decisions, our central theme has been the contrast between "deliberate" strategies, that is, patterns intended before being realized, and "emergent" strategies, patterns realized despite or in the absence of intentions. Emergent strategies are rather common in organizations, or, more to the point, almost all strategies seem to be in some part at least, emergent. To quote that expression so popular on posters these days, "Life is a journey, not a destination."

In this article I will describe my journey into research, to step back from the stream of decisions concerning my own research since I began a doctoral dissertation in 1966, and to discuss the patterns — the themes or strategies — that appear. In retrospect, some seem more deliberate to me, others more emergent, but in general they appear to represent a blending of the two. The point I wish to make is that these themes form their own strategy of research, one that I would like to contrast with a more conventional strategy of research that seems to have dominated our field.

A word on the data base. I have been involved in three major research projects these past 13 years (and since three do not a sharp pattern make, the title refers to an emerging rather than an emergent strategy). First was my doctoral dissertation, a study of the work of five managers through structured observation (Mintzberg, 1973). Essentially, I watched what each did for a week, and recorded it systematically — in terms of who they worked with, when, where, for how long, and for what purpose. These data were used to induce a set of characteristics and of roles of managerial work. Second, over the course of some years, teams of MBA students were sent out to study local organizations; one assignment was to take a single strategic decision and to describe the steps the organization had gone through from the very first stimulus to the authorization of the final choice. From these reports, we selected the 25 most complete and inferred a structure of the "unstructured" decision process (Mintzberg, Raisinghani, and Théorêt, 1976). Our third project, mentioned in the opening of this paper, involves the study of organizational strategies through the tracing of patterns in streams of decisions over periods of 30 or more years. This is a large project, at the present time involving a number of months of on-site research in each organization. We first spend a good deal of time reading whatever historical documents we can find, in order to develop thorough chronologies of decisions in various strategy areas. Then we switch to interviews to fill in the gaps in the decision chronologies and to probe into the reasons for breaks in the patterns (i.e., for strategic changes). We have so far completed five of these studies (Mintzberg, 1978, reports on the earliest phase of this research), and five more are now underway (in addition to about 15 similar but shorter studies carried out by MBA students as part of their term work). Two other projects, while not based on our own research, form part of the data base of this paper. These are attempts to synthesize the empirical literature in two areas — organizational structuring
Strategy of “Direct” Research

and power. These efforts have led to two books (Mintzberg, 1979, forthcoming), both revolving around the notion of configurations, or ideal types of many dimensions.

This paper focuses on seven basic themes each of which underlies to a greater or lesser degree these various research activities.

1. The research has been as purely descriptive as we have been able to make it. This hardly seems unusual in organization theory. But most of the work has been concentrated in the policy area, where prescription has been the norm for a long time. Moreover, one could argue that much of the “descriptive” research about organizations has set out to prove some prescription, for example that a participative managerial style is more effective than an autocratic one.

The orientation to as pure a form of description as possible has, I believe, enabled us to raise doubts about a good deal of accepted wisdom: to be able to say that managerial work observed has more to do with interruption, action orientation, and verbal communication than with coordinating and controlling; to say that diagnosis and timing count more in strategic decision making than the choice of an alternative from a given set; to say that strategy formation is better understood as a discontinuous, adaptive process than a formally planned one. The little boy in the Hans Christian Andersen story, who said that the emperor wore no clothes, has always served as a kind of model to me. This is not to imply that our work so exposes the manager; in fact, I believe it clothes him in more elegant garments. It is the literature of management that often emerges as naked, since much of what it says becomes transparent when held up to the scrutiny of descriptive research.

2. The research has relied on simple—in a sense, inelegant—methodologies. The field of organization theory has, I believe, paid dearly for the obsession with rigor in the choice of methodology. Too many of the results have been significant only in the statistical sense of the word. In our work, we have always found that simpler, more direct methodologies have yielded more useful results. Like sitting down in a manager’s office and watching what he does. Or tracing the flow of decisions in an organization.

What, for example, is wrong with samples of one? Why should researchers have to apologize for them? Should Piaget apologize for studying his own children, a physicist for splitting only one atom? A doctoral student I know was not allowed to observe managers because of the “problem” of sample size. He was required to measure what managers did through questionnaires, despite ample evidence in the literature that managers are poor estimators of their own time allocation (e.g., Burns, 1954; Horne and Lupton, 1965; Harper, 1968). Was it better to have less valid data that were statistically significant?

Given that we have one hundred people each prepared to do a year of research, we should ask ourselves whether we are better off to have each study 100 organizations, giving us superficial data on ten thousand, or each study one, giv-
ing us in-depth data on one hundred. The choice obviously depends on what is to be studied. But it should not preclude the small sample, which has often proved superior.

3. The research has been as purely inductive as possible. Our doctoral students get a dose of Popper (1968) in their research methodology course. Popper bypasses induction as not part of the logic of scientific inquiry, and the students emerge from the course — like many elsewhere — believing that somehow induction is not a valid part of science. I stand with Selye (1964) in concluding that, while deduction certainly is a part of science, it is the less interesting, less challenging part. It is discovery that attracts me to this business, not the checking out of what we think we already know.

I see two essential steps in inductive research. The first is detective work, the tracking down of patterns, consistencies. One searches through a phenomenon looking for order, following one lead to another. But the process itself is not neat.

Even in the nineteenth century, celebrated discoveries were often achieved enigmatically. Kekuly tortuously arrived at his theory of the benzene molecule; Davy blundered onto the anesthetic properties of nitrous oxide; Perkin’s failure to produce synthetic quinine circuitously revealed aniline dyes; and Ehrlich tried 606 times before he succeeded in compounding salvarsan in 1910 (Dalton, 1959: 273).

The second step in induction is the creative leap. Selye cites a list of “intellectual immoralities” published by a well-known physiology department. Number 4 read “Generalizing beyond one’s data.” He quotes approvingly a commentator who asked whether it would not have been more correct to word Number 4: “Not generalizing beyond one’s data” (1964: 228). The fact is that there would be no interesting hypothesis to test if no one ever generalized beyond his or her data. Every theory requires that creative leap, however small, that breaking away from the expected to describe something new. There is no one-to-one correspondence between data and theory. The data do not generate the theory — only researchers do that — any more than the theory can be proved true in terms of the data. All theories are false, because all abstract from data and simplify the world they purport to describe. Our choice, then, is not between true and false theories so much as between more and less useful theories. And usefulness, to repeat, stems from detective work well done, followed by creative leaps in relevant directions.

Call this research “exploratory” if you like, just so long as you do not use the term in a condescending sense: “OK, kid, we’ll let you get away with it this time, but don’t let us catch you doing it again.” No matter what the state of the field, whether it is new or mature, all of its interesting research explores. Indeed, it seems that the more deeply we probe into this field of organizations, the more complex we find it to be, and the more we need to fall back on so-called exploratory, as opposed to “rigorous,” research methodologies.

To take one case of good exploration and a small leap, a young doctoral student in France went into the company in
that country that was reputed to be most advanced in its long-range planning procedures (in a country that takes its planning dogma very seriously). He was there to document those procedures, the "right" way to plan. But he was a good enough detective to realize quickly that all was not what it seemed on the surface. So he began to poke around. And with small creative leaps he produced some interesting conclusions, for example, that planning really served as a tool by which top management centralized power (Sarrazin, 1977-78). Peripheral vision, poking around in relevant places, a good dose of creativity — that is what makes good research, and always has, in all fields.

Why do we let our doctoral students be guided by mechanical methodologies into banal research? Weick (1974: 487) quotes Somerset Maugham: "She plunged into a sea of platitudes, and with the powerful breast stroke of a channel swimmer made her confident way toward the white cliffs of the obvious." Why not, instead, throw them into the sea of complexity, the sea of the big questions, to find out if they can swim at all, if they can collect data as effective detectives, and if they are capable of even small leaps of creativity. If not, perhaps they have chosen the wrong profession.

4. The research has, nevertheless, been systematic in nature. I do not mean to offer license to fish at random in that sea. No matter how small our sample or what our interest, we have always tried to go into organizations with a well-defined focus — to collect specific kinds of data systematically. In one study we wanted to know who contacts the manager, how, for how long, and why; in the second we were interested in the sequence of steps used in making certain key decisions; in the third, we are after chronologies of decision in various strategic areas. Those are the "hard" data of our research, and they have been crucial in all of our studies.

5. The research has measured in real organizational terms. Systematic does not mean detached. Probably the greatest impediment to theory building in the study of organizations has been research that violates the organization,
that forces it into abstract categories that have nothing to do with how it functions. My favorite analogy is of an organization rich in flows and processes, as implied in the Figure, kind of like a marble cake. Then along comes a researcher with a machine much like those used to slice bread. In goes the organization and out come the cross-sectional slices. The researcher then holds up one of them, shown to the right in the Figure, and tries to figure out what it is he or she is seeing. “Hmmm . . . what have we here? The amount of control if 4.2, the complexity of environment, 3.6.” What does it mean to measure the “amount of control” in an organization, or the “complexity” of its environment? Some of these concepts may be useful in describing organizations in theory, but that does not mean we can plug them into our research holus-bolus as measures. As soon as the researcher insists on forcing the organization into abstract categories — into his terms instead of its own — he is reduced to using perceptual measures, which often distort the reality. The researcher intent on generating a direct measure of amount of control or of complexity of environment can only ask people what they believe, on seven-point scales or the like. He gets answers, all right, ready for the computer; what he does not get is any idea of what he has measured. (What does “amount of control” mean anyway?) The result is sterile description, of organizations as categories of abstract variables instead of flesh-and-blood processes. And theory building becomes impossible. Far from functioning like detectives, “In touching up dead data with false colors, [social scientists] function much like morticians” (Orlans, 1975: 109).

If someone is interested in studying perceptions, then by all means let him study perceptions. But let him not study perceptions if it is control or complexity he is after. There is no doubt that “the perceptions of the chief executive are important in understanding why organizations are structured as they are” (Pfeffer and Leblebici, 1973–74: 273). But that does not justify researchers — these and many others — in drawing conclusions about how the “environment,” as opposed to the “perception of the environment,” affects structure.

Measuring in real organizational terms means first of all getting out into the field, into real organizations. Questionnaires often won’t do. Nor will laboratory simulations, at least not in policy research. The individual or group psychologist can bring the phenomenon he is studying into his laboratory holus-bolus; the organization theorist cannot. What is the use of describing a reality that has been invented? The evidence of our research — of interruptions and soft data and information overload — suggests that we do not yet understand enough about organizations to simulate their functioning in the laboratory. It is their inherent complexity and dynamic nature that characterize phenomena such as policy making. Simplification squeezes out the very thing on which the research should focus.

Measuring in real organizational terms means measuring things that really happen in organizations, as they experience them. To draw on our research, it means measuring the proportion of letters sent by customers or the number

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1 A number of studies in management policy have sought correlations of performance and amount of planning — to show that planning pays. But what exactly is the definition of planning in the context of actual strategy formation? The answer to that question requires intensive research on decision-making processes, as in the research in France cited earlier, not a few measures on questionnaires or the counting up of a bunch of formal documents that management may never look at.
of new stores opened in a given year. It is the job of the researcher to abstract from the particular to the general, to develop concepts from his measurements in the field. I believe the researcher shirks his responsibility when he expects the manager to do the abstracting, to decide how complex is the environment (or even what complexity means). Managers do not think about complex environments; they think about new discoveries in plastics, about the problems of getting the R&D people to work with those in marketing, about how a proposed piece of legislation will affect sales.

My favorite anecdote in this regard concerns Peter Travis, an Australian and one of the world's great potters. He was approached by a researcher who wanted to study the creative process. The researcher proposed to elicit protocols from Travis as he worked. They tried that, but got nowhere. Travis felt he could not verbalize about the creative process; he had to demonstrate it visually. So he proposed to make a bowl on his wheel, then another, then another, and continue until he had a thousand pots. He might make ten alike and then vary the rim on the eleventh. By the twentieth he might modify the shape, and by the one-hundredth he might not feel like making bowls at all but instead decide to form bottles. One form would lead to another so that by the one-thousandth pot Travis would have a visual record of the creative process. The researcher could then come in and describe it. (Travis apparently really intends to carry out his side of the bargain. The best part of the story is that Travis, in recounting, thought that his proposal was so "obvious." One thousand pots. "How else would you study creativity?" It seems that we need creative minds to study creativity. And complex minds to study complexity. Too bad Peter Travis didn't choose to become a management researcher. But then again, which doctoral program would have let him in?)

6. The research, in its intensive nature, has ensured that **systematic data are supported by anecdotal data.** More and more we feel the need to be on site, and to be there long enough to be able to understand what is going on. (We began with a week and are now spending months and even years.) For while systematic data create the foundation for our theories, it is the anecdotal data that enable us to do the building. Theory building seems to require rich description, the richness that comes from anecdote. We uncover all kinds of relationships in our "hard" data, but it is only through the use of this "soft" data that we are able to "explain" them, and explanation is, of course, the purpose of research. I believe that the researcher who never goes near the water, who collects quantitative data from a distance without anecdote to support them, will always have difficulty explaining interesting relationships (although he may uncover them). Perhaps this has something to do with how our minds work. Those creative leaps seem to come from our subconscious mental processes, our intuition (e.g., Hadamard, 1949). And intuition apparently requires the "sense" of things — how they feel, smell, "seem." We need to be "in touch." Increasingly in our research, we are impressed by the importance of phenomena that cannot be
measured — by the impact of an organization’s history and its ideology on its current strategy, by the role that personality and intuition play in decision making. To miss this in research is to miss the very lifeblood of the organization. And missed it is in research what, by its very design, precludes the collection of anecdotal information.

7. The research has sought to synthesize, to integrate diverse elements into configurations of ideal or pure types. Organizations intermingle a great many elements in their functioning. Researchers who focus on two variables at a time — who catch what someone has called “the economists’ plague”: holding all other things constant — seem to cloud issues almost as often as they clarify them. Consider the unending debates about size and administrative ratio or bureaucracy and centralization. Literally, the more that has been published, the more confused the issues have become (and I apologize for this bivariate hypothesis). We shall never have closure so long as we pretend that other things can be held constant. We live in a world of dynamic systems. (A colleague of mine claims that everything in the world correlates with everything else at 0.3.) Organizations also experience all kinds of lags and discontinuities. For example, because structural change often follows strategic change (Chandler, 1962; Stopford and Wells, 1972; Rumelt, 1974), it is somewhat a matter of luck whether a two-variable cross-sectional study manages to capture the structure that reflects today’s situation — which it typically measures — or yesterday’s, which it typically does not. And the presence of discontinuities (e.g., Woodward, 1965, about structure as a function of technology; Klatzky, 1970, about A/P as a function of size) plays havoc with these conventional approaches.

We can also question whether the human brain prefers to think in terms of continuous and bivariate relationships, or searches for another kind of order, characterized by clusters or configurations, ideal or pure types. We seem to put diverse elements together into various envelopes. The word “democracy,” for example, seems to be captured less by some scale of freedom than by a configuration of elements — a free press, due process, elected officials, and so on. Likewise I believe that we prefer to understand our organizations in terms of pure types — configurations of many elements.

But to generate those configurations, I have more faith in typologies than taxonomies, if I understand correctly how these terms are used. In other words, while I believe we need empirical data to generate our categories — systematic data reinforced by a good deal of anecdote — I do not expect them to come from mechanical data reduction techniques. It is pattern recognition we are after, in the form of those creative leaps, and I believe that human, not electronic, brains are most capable of achieving those leaps.

To conclude, these seven characteristics underlie the research we have been doing these past 13 years. Together they seem to form their own configuration: research based on description and induction instead of implicit or explicit prescription and deduction; reliance on simple, inelegant, as
opposed to “rigorous” methods of data collection; the measurement of many elements in real organizational terms, supported by anecdote, instead of few variables in perceptual terms from a distance; and the synthesis of these elements into clusters, instead of the analysis of pairs of variables as continuous relationships. We might call this strategy, for want of a better term, “direct research.”

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