TEN YEARS LATER

Some Personal Reflections on

Management and Methodology*

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October, 1978

*Adapted from a recording of a speech given at Stanford University, January 19, 1978.

My intention here is not to present my research and writing as such. Rather, I wish to step back from it and look at some of its emerging themes, themes concerning both management and methodology.

I completed my Ph.D. ten years ago, in January 1968, at the MIT Sloan School. That was fortunate for me, because I did the doctorate in Management Policy, and there were no professors of Policy in the Sloan School at that time. Since then, I have defined myself as a professor of Policy. The more traditional Policy people have not quite known what to do with me. They haven't repudiated me, but they have never been quite sure whether or not to accept me. On the other hand, the Organization Theory people seem to have embraced me, which has always scared me a little bit. In any event, by now the McGill Faculty of Management--where I have taught these past ten years--has merged its Policy and Organization Theory courses at all levels, and so I can safely define myself as being in both fields, or, more exactly, on that side of Policy which merges into Organization Theory. That is the side that concerns itself with describing empirically the policy making behavior of organizations.

When I finished up at MIT, I decided to write a book called The Theory of Management Policy. It was intended to show that the field had substance, that there in fact existed a great deal of published research that described the policy making process, but that this material had never been drawn together in any one place.

Ten years later that book now exists in some sense. Of the five main chapters, the first is 500 pages long, the second 300 pages, the third also about 500 pages. Mercifully, the fourth is only about 150 pages long and the fifth has not yet been written. I do not mean to be facetious. Every one of what I had hoped would be chapters has emerged as a book in its own right, which I guess demonstrates my point that there was in fact an awful lot of research material related to Policy which was waiting to be brought together.

I wish to discuss briefly my approach to each of these five topics and then to draw them together to consider some of the emergent themes. In three of the topics, I have done my own research and have combined it with the existing research-based literature, while in the other two I have worked only from the literature, in all five cases seeking to synthesize rather than review that literature. These topics include managerial work, in which my research was completed in 1968, organizational structure and organizational power, which involve only the literature, strategic decision making, in which our research was completed in 1973, and strategy formation, on which we began research in 1971 and are still going strong. The book on managerial work appeared in 1973, that on structure is due out in early 1979, and those on power and decision making exist only as rough drafts. The book on strategy formation remains to be written.*

In the case of managerial work, my research--carried out as my doctoral dissertation--involved the "structured observation" of the work of each of five chief executives for one week. From data on the executives' mail and contacts, etc., I induced some conclusions concerning the roles and characteristics of managerial work. These results have received a good deal of play in the literature and popular press, far beyond what I ever expected. Given that I described what to me seemed like the patently obvious, articles in publications such as <u>The New York Times</u> and <u>Playboy</u> came as surprises. In retrospect, I guess it is fair to say that the interest stemmed from the fact

*See the bibliography for a full list of these "chapters" as well as details of the various book and article publications of each.

that my study broke with so many years of tradition about what managers were supposed to do. In contrast to the manager described as in control, on top, conducting the orchestra, my study (as well as others of the same period, such as those of Rosemary Stewart) described a job characterized by interruption, verbal as opposed to written forms of communication, an action instead of a planning orientation, a heavy emphasis on developing lateral contacts and the use of them for information, and an absence of pattern in the work.

The New York Times* used two expressions to describe these results: "calculated chaos" and "controlled disorder". Those expressions seemed to capture something I had not fully appreciated when I published my results. Bear them in mind; I wish to return to them in my concluding comments.

The second book is about organizational structuring. I undertook a reading of this literature because I wanted to find how organizations form their strategies, and felt I first needed to develop a sophisticated understanding of structure. In fact, in reading and writing about structure, I think I learned as much about strategy formation as in actually researching that process. I also learned a good deal about industrial democracy, about quality of working life, and about a number of other topics, more than I have learned from my reading of the literature on those topics themselves. Structure seems to underlie many of the important questions we raise about organizations.

I wish to focus on two themes that emerge from this book (which at the time of this writing is in press with Prentice-Hall). First is the importance of flows in understanding organizations, and our complete ignorance of them--the flows of objects and of information and of decision processes. A good deal of the research has focussed in a cross sectional way on two-variable

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*October 29, 1976

relationships, for example between the size of the organization and its ratio of administrative to production personnel. The trouble with this kind of research is that one cannot really explain the relationships one finds in research unless they can be traced in terms of oragnizational flows. We need to know, for example, what it is about organizational size that causes changes in "administrative ratio"? What influence does size have on communication and decision making that affects the use of administrative personnel? An inability to answer such questions--in these studies and many others--has impeded our ability to build theory from the findings of research.

A second theme that emerges from the structure book is the notion of "configurations" of variables. In discussing the structuring of organizations, we are dealing with a great many variables--specialization and formalization and decentralization and planning, etc., as well as the contingency factors of age, size, technical system, environment, and so on. Looking at these variables two at a time often leads to a great deal of confusion. Looking at them many at a time, in clusters or configurations, seems to clear away a good deal of the confusion. This approach also encourages a broader view of causation, specifically a systems view, that causation is two sided, that the same variables can be both dependent and independent. For example, the well known relationship between diversification and divisionalization has generally been expressed as: the more diversified its markets, the greater the propensity for an organization to divisionalize its structure. But some researchers have noticed that divisionalized organizations show clear tendencies to further diversify their markets (presumably because of the ease of doing so in such structures, and because these structures tend to generate corps of agressive general managers looking for new fields to conquer). Likewise, growth of the organization encourages bureaucratization of its structure, but bureaucracies also have a habit of trying to grow larger. The synthesis in The Structuring or Organizations revolves around five configurations--"ideal" or pure types--strongly suggested in the literature: Simple Structure, Machine Bureaucracy, Professional Bureaucracy, Divisionalized Form, and Adhocracy.

The third book, on Power In and Around Organizations, is similar to that on structuring, in that it was undertaken as a necessary prerequisite to the understanding of strategy formation, and that it involves an attempt to synthesize the elements of organizational power into a set of power configurations. Unlike most books in this area, this one does not focus on individual power, need for achievement, and the like. Rather, the focus is on the power structure--why individuals and groups have power by virtue of the roles they play in the organization--as directors, staff experts, customers, government officials.

In this work, I find it convenient to distinguish an external coalition (of outsiders) from an internal one (of essentially full-time employees). The former can be described as dominated, divided, or passive, and the latter as autocratic, bureaucratic, ideologic, meritocratic, or politicized. Putting together combinations of these two leads to six power configurations: the Continuous Chain (dominated, bureaucratic), the Closed System (passive, bureaucratic), the Commander (passive, autocratic), the Missionary (passive, ideologic), the Professional (divided, meritocratic), and the Conflictive (divided, politicized).

Our work on strategic decision making focusses on how organizations actually make single strategic (important) decisions, such as to introduce a new product line or build a new factory. Here we drew the empirical literature together with our own study of twenty-five strategic decision processes to develop a general model of the process and seven types of paths through it. What surprised us in this study was, first of all, the importance of dynamic factors in strategic decision processes. Those processes are characterized by interruptions, by a good deal of cycling, by the importance of timing. All three phenomena seem to be crucial in understanding such processes. Strategic decision making is also characterized by the fact that everything that matters remains a great mystery in the literature and everything that doesn't seems crystal clear. The literature--especially that of management science--focusses on the evaluation/choice aspect of decision making. Yet that seems to be the icing on the cake in most decision processes. What really seems to count is diagnosis of the situation in its early stages, but no one seems to say anything about that in the literature. The same is true for timing, and for the design of solutions. I shall return to this issue in my concluding comments as well.

Our fifth topic is strategy formation, concerned with the interrelating of strategic decisions over time. I have done some articles on the subject, but the book awaits the completion of a major research project, which is now seven years old and going stronger than ever. It also awaits the careful reading of four boxes of published materials sitting under my desk at home.

What matters most to us in this research is our definition of strategy, because it changes the whole view of how to research and understand strategy making. Strategy is inevitably defined as a plan, an intention, a guiding force into the future. But that definition makes the concept impossible to research--impossible, at least, if like me, you do not believe much in research on perceptions. Defining strategy as a plan forces the researcher to interview those who have the plans. Defining strategy, as we do, as "a pattern in a stream of decisions", enables the researcher to study behavior, namely the streams of decisions made by organizations over given time periods. To illustrate, the Picasso blue period would, by our definition, be called his blue strategy, in the sense that the painter made a consistent set of decisions over a period of time, to use blue paint.*

Once we define strategy in this way, we can go into organizations, find out which of their decision areas are of greatest importance, extract (from documents) chronological lists of decisions made in those areas over long periods of time (at least thirty years), analyze each list for patterns (i.e. strategies), identify major periods of change in strategies, and then probe intensively (through interviews and the reading of reports) into the reasons for these changes.

Using this approach, we have carried out a number of studies--of U.S. strategy in Vietnam from 1950 to 1973, of the strategy of Volkswagenwerk from 1934 to 1974, and of a Canadian magazine across fifty years and a film making agency across almost forty. A number of other studies are currently under way, including a large supermarket chain and a major airline. More and more in this research, we are trying to express the decision streams in quantitative form, for example, as plots of store openings on a year by year basis for the supermarket chain or of the number of films on a given topic per year for the film making agency. We are also trying more and more to bring in our ideas of structure and power into this research, to see the relationships between them and strategy making process.

In carrying out our intensive analysis of the development and changing of strategies, we are particularly interested in the links between strategies by the two definitions--"intended" strategies and "realized" strategies. We would like to know, for example, when organizations realize the strategies

*What I find interesting about this second definition of strategy is that while no-one so defines strategy, many people use it in this way. Asked about his company's actual strategy, a businessman will often describe consistent patterns in his decision making which were not originally intended.

they explicitly intended, which we call "deliberate" strategies (another way of asking under what conditions strategic planning work?), when they fail to realize their intended strategies, and when they exhibit "emergent" strategies, realized even though not intended.

This brings us to another reason why we insist on our second definition of the word strategy, despite a good deal of heated criticism that we are misusing the word. Our research suggests that no strategy is 100% deliberate, that is, realized exactly as intended. Perhaps 95%, but never 100%. Every strategy seems to be at least partly emergent: that is, elements of it evolve over time. So the emergent aspect must be recognized as an integral part of the strategy formation process. (And that is why we refer to the strategy "formation" process--strategies form in part without being formu<u>la</u>ted.) Recognition of this immediately puts into question so many of the accepted precepts about strategy making: that strategies should be explicit, that systematic, regular planning should always take precedence over a more adaptive process, that strategy "formulation" should be separated from strategy "implementation", and so on.

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I would like at this point to discuss some of the themes that emerge from my work in these five areas--in effect, to discuss the patterns in the ten-year stream of my own activities, my strategies, partly deliberate and partly emergent. I shall begin with themes concerning the research methodology I have used, and conclude with themes concerning the view of the management process that emerges from this work.

First of all, my research has been <u>descriptive</u> in all its aspects. If one feature above all characterizes my approach, I like to think it is the use of the description of the obvious to contradict the accepted truth: to say that managerial work observed has more to do with interruption, action orientation, and verbal communication than 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. I guess I like the image of that little boy in the Hans Christian Andersen story, the one willing to say that the emperor wore no clothes. I do not mean to imply that my work so exposes the manager; later I shall argue that I believe it clothes him in more elegant garments. It is the literature that I often find naked, since so much of what it says becomes transparent when held up to the scrutiny of descriptive research.

For some reason, this field of management is full of a kind of "normative naiveté", armchair prescription based on naive views of how the world works. Managers and organizations are told to spend more time planning, are expected to use simple MISs, are urged to make their strategies explicit, are expected to base their decision on one-sided cost-benefit analyses. As John Kenneth Galbraith has written, in the modern large corporation, "A vivid image of what should exist acts as a surrogate for reality. Pursuit of the image then prevents pursuit of the reality".*

In this regard, I take my role as academic to be that of generating and then disseminating the best descriptive theory possible, in other words, to train practitioners or practitioners-to-be to have the best possible understanding of management phenomena. As academic, I believe I have no business trying to generate normative, or prescriptive, theory. Being prescriptive in general--from the armchair--has caused many of our troubles. The best prescription comes from the practitioner, in context.** Descriptive theory, when it changes the

*J.K. Galbraith, The New Industrial State (Houghton Mifflin, 1967) p.72

**That could, of course, include the academic in the role of consultant.

worldview of the practitioner, becomes a tool for prescription in context, in fact, probably the most powerful one we have.

A second characteristic of my research has been my use of simple, if you like, inelegant, methodologies, a characteristic of which I am proud. I believe the field of management has paid a great price for the obsession of its researchers with elegance in methodology. Not the least of this has been the persistence of the normative naiveté, which would have been swept away by research more concerned with relevance than method. Too much of our research has been significant only in the statistical sense of the word.

If I may be forgiven this simplification, I find there are two kinds of researchers, those who love method and those who love knowledge. The former fill the journals with articles that begin with a half page of introduction, then go into a page or two on the previous literature and two or three on the methodology, and end with four or five pages on the results; there follows a paragraph or two of conclusions, that say, in effect, "This was such a great methodology that I think others should repeat its use. I can't give much in the way of conclusions, but the next study is bound to." There is only one test of a methodology, and that is the usefulness of the conclusions, of the theory that can be generated (or tested) from its results. For my part, I have always found that the simplest methodologies yield the most 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 is wrong, for example, with samples of one? Doctoral students have to apologize for samples of one all the time. Yet few have to apologize for large samples that tell them nothing of great interest. Thank goodness I did not have to apologize to my thesis advisors for studying five managers. Yet I know a student who was not allowed to observe managers because of the "problem" of sample size. He was forced to measure what they did through questionaires, despite ample evidence in the research literature that managers are unreliable estimators of their own time allocation. Better to have faulty data that was statistically significant!

Given we have one hundred people each prepared to do one year of research, were each of them to study one hundred organization, we would have superficial data on ten thousand; on the other hand, were each to study one, we would have in-depth data on one hundred. I'll take the latter any day. I'll take even one study by Crozier or by Dalton or by (Jay) Galbraith to all those Aston studies, simply because each of the former tells me so much more about organizations.

A third characteristic is that my research has been as purely <u>inductive</u> as I could possibly make it. Testing hypotheses may be a relevant side of science, but I am with Hans Selye* in considering it the less interesting and less challenging side and in leaving it to others. It is discovery that attracts me to this business, not checking out what we think we already know.

I see two essential steps in inductive research. The first is <u>research as detective work</u>--the tracing down of patterns, consistencies. One begins by tapping into a data stream, which suggests the need to look at another, which leads to another, and so on until we are able to reconstruct or describe a phenomenon. Despite the neatness of the description of the scientific method--a process by which, to my knowledge, no great scientist ever discovered anything--science in use is a messy business. "Even in the nineteenth century, celebrated discoveries were often achieved enigmatically. Kekule 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 time before he succeeded

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H. Selye <u>From Dream to Discovery: On Being a Scientist</u> (McGraw-Hill, 1964) in compounding salvarsan in 1910."

And the second step in induction is research as the creative leap. Selye cites a list of "intellectual immoralities" published by a well known physiology department. Number 4 was "Generalizing beyond one's data". He quotes approvingly a commentator who asked whether it would not have been more correct word Number 4 "Not generalizing beyond one's data."** The fact is that there would be no theory--no hypotheses to test with all those elegant methodologies--if no-one ever generalized beyond his 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. No theory is true; all 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: "O.K., 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--new, mature--all interesting research explores. Indeed, it seems to me that the more deeply we probe into this field of management, 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

*Dalton, M. Men Who Manage (Wiley, 1959) p.273

**Selye, op. cit., p.229

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 a small creative leap he produced some interesting conclusions, for example, that planning really served as a tool by which top management centralized power.* Peripheral vision, poking around in relevant places, a good dose of creativity--that is what makes good research, and always has, in all fields.

And so I ask myself why we let our doctoral students be guided by mechanical methodologies into banal research. Karl Weick quotes Somerset Maigham: "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, such as the role of intuition in decision making or the relationship between flows and structure in organizations, and see 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, maybe they have chosen the wrong profession.

In describing research with these characteristics, I do not mean to offer liscence to fish at random in that sea. I am not talking about detectives who knock on the doors of organizations and say "I am here to look around, to see if I can find anything interesting." Sherlock Holmes always went with a purpose and so do we. I believe that all three of the empirical studies I have been involved in have been characterized by a fourth characteristic. No matter how small the sample or what the intention, we have always gone into our organizations in an orderly way and with a clear focus--to collect

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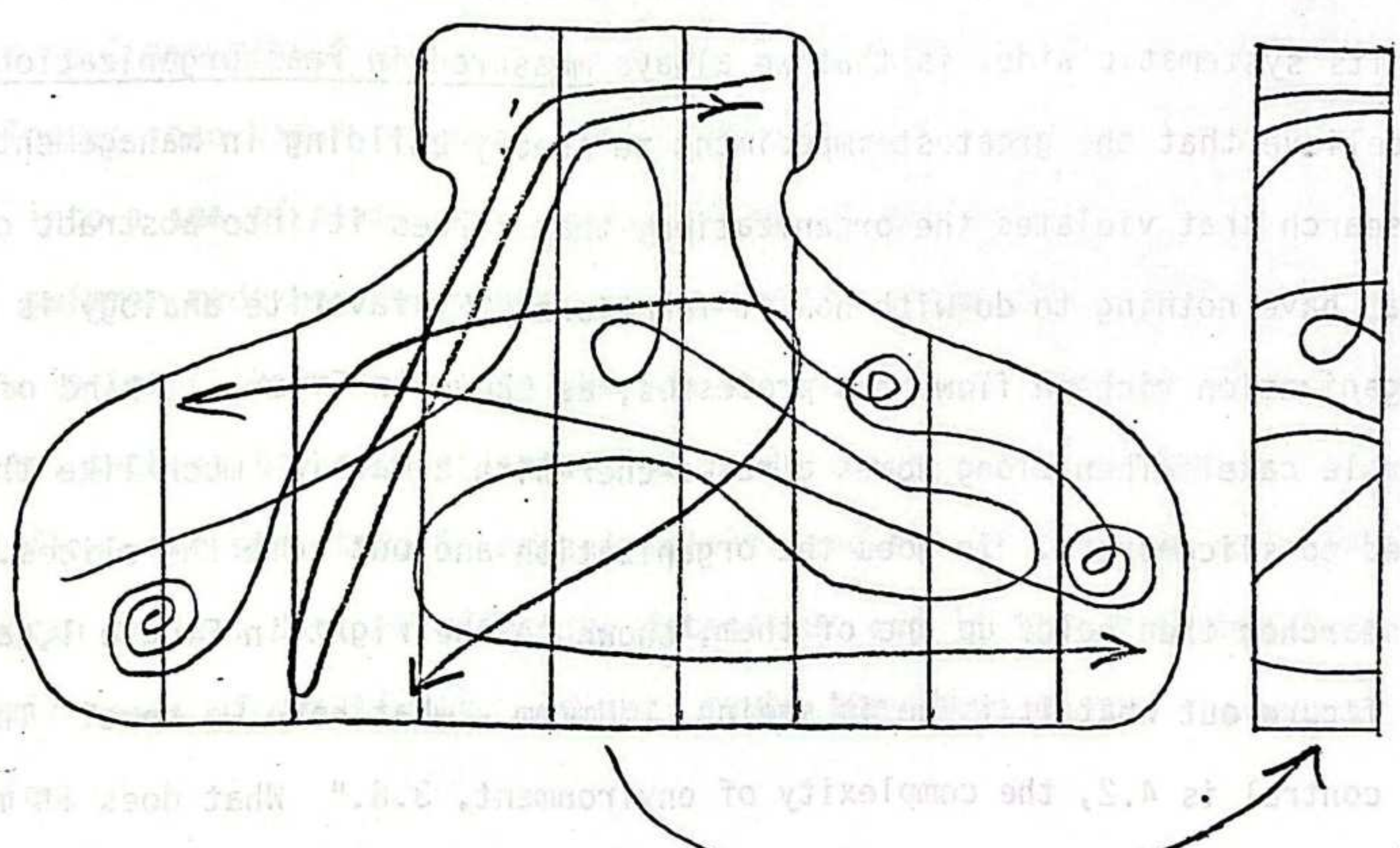
* J. Sarrazin, Decentralized Planning in a Large French Company: An Interpretive Study, International Studies of Management and Organization, Fall-Winter, 1977-78: 37-59.

** Quoted in K. Weick, Academy of Management Journal, September 1974: p.487.

specific kinds of data systematically. In one, 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 too in all of our studies.

But a fifth and equally crucial characteristic of our research, in terms of its systematic side, is that we always measured in real organizational terms. I believe that the greatest impediment to theory building in management 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 shown in Figure 1, 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 slices. The researcher then holds up one of them, shown to the right in Figure 1, and tries to figure out what it is he is seeing. "Hmmmm...what have we here? The amount of control is 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 (not all) of these concepts may be useful in describing organizations in theory, but that does not mean we can plug them into our research directly 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 complexity can only ask people what they believe, on seven point scales or the like. Answers he gets, all ready for the computer; what he does not get is any idea of what he has measured. (What does "amount of control" mean

Figure 1: Slicing Up the Organization



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."**

If someone is interested in studying perceptions, then by all means let him study perceptions. But let's not study perceptions if it is control or complexity we are after. There is no doubt that "the perceptions of the chief executive are important in understanding why organizations are structured as they are."*** 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 won't do. Nor will laboratory simulations, at least not in Organization Theory or Management Policy. 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? I recall a doctoral student who wanted to pose hypothetical questions to real merchant bankers to find out how they made decisions. I wondered out loud whether he would consider the effect of decisions made earlier in the day, for example, one just an hour earlier which committed a large proportion of his available funds. Or the call from a partner about sending up his cousin who has a pet project. The student decided to study the bankers' real decisions instead.

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*A number of studies in 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 questionaires or the counting up of a bunch of formal documents that management may never look at.

**H. Orlans "Neutrality and Advocacy in Policy Research" Policy Sciences (1974, p.109)

***J. Pfeffer and H. Leblebici "The Effect of Competition on Some Dimensions of Organization Structure" Social Forces (1973-74, p.273)

The "semi-laboratory" approach--studying real subjects in a *hypothetical* world--certainly simplifies the work of the researcher. But whose world ends up being studied? And what room do those cooked-up questions leave for that creative leap? Neatened-up research certainly generates neat conclusions--exactly those the researcher decided to find when he chose his methodology. But hardly those of the messy world of policy making. It is their complexity and dynamic nature that characterize phenomena such as policy making. Simplification squeezes out the very things on which the research must 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 of new stores oepned 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. To my mind, it is simply a shirking of responsibility to expect the manager to do the abstracting, to decide how complex is the environment (or even what the word complexity means). Managers do not think about complex environments; they think about new discoveries in plastics, about the problems of getting the R and D people to work with those in marketing, about problems in the relationships between the government and the union. Sherlock Holmes did not ask others to abstract for him. He wanted his data firsthand, not filtered through other brains. The abstracting was <u>his</u> job, <u>his</u> skill.

My favorite story 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 protocals from Travis as he worked. They tried that, but got nowhere. Travis felt he could not verbalize about the creative process taking place in his mind; he had to demonstrate it visually. So he proposed to make a bowl on the wheel, then another, and another, and continue until he had made 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 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 all of this to my wife, 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 allowed him in?

Systematic data has been vital in our research, but another kind of data has proven equally so--anecdotal data. And that leads to a sixth characteristic of the research, namely its <u>intensive</u> nature. We need to be on site, and to be there long enough to be able to understand what is going on. For while systematic data creates the foundation for our theories, it is the anecdotal data that enables us to do the building. Theory building seems not to be possible without rich description, richness that comes from anecdote. We see all kinds of relationships in our "hard" data, but it is only through the use of this "soft" data that we can "explain" them, and explanation is, of course, the purpose of science. I am firmly convinced that the researcher who never goes near the water, who collects only quantitative data without anecdote to support it, will never explain anything very interesting (although he may discover interesting relationships). Perhaps this has something to do with how our brains work. It is our intuition--our subconscious mental processes--that make those creative leaps; and intuition apparently requires the "sense" of things--how they feel, smell, "seem". We need to be "in touch". So effective research for me means research that couples systematic data, gathered in real organizational terms, with anecdotal data, with rich flexible description.

The seventh and final characteristic of our research--and attempts to make sense of the literature--is the emphasis on synthesis, on the integration of diverse elements. If the economists say that organizations maximize, and Herbert Simon insists that they satisfice, then perhaps they do both, but under different conditions.

Moreover, we are dealing with a great many variables in management theory. We do not get far by focussing on variables two at a time, by catching what one writer called "the economists' plague": holding all other things constant. They simply will not stay still. Too many variables intervene. Besides, I do not believe the human brain generally thinks in terms of continuous and bivariate relationships. Rather we seem to search for a broader kind of order, characterized by clusters or configurations, ideal or pure types. We seem to like to put elements together in various envelopes. For example, I believe that to most people the word "democracy" does not mean more or less freedom along some scale; rather it means a set or configuration of elements--a free press, due process, elected officials, and so on. Likewise I believe we prefer to understand organizations in terms of pure types. Configurations have served me well. It is too early to say whether mine will serve others well; I can only express my hope.

Now let me turn to the themes that emerge from my work concerning management itself. What struck me about the terms "calculated chaos" and "controlled disorder" is that they reject both the classical notion of the manager fully in charge, on the podium, and its converse, the manager out of control, swept by the forces of "irrationality", the manager in Jim March's "garbage can".

To me these terms suggest a higher order rationality, a manager smart enough to know how to make his way through chaos, disorder. The manager of my research doesn't pretend to live in the simple, malleable world of the principle theorists. But neither does he give up in frustration. He realizes the world is too complex for him. But it is too complex for everyone. Someone has to make out. Why not him. That makes him, to my mind, more rational, smarter, than many of the researchers and theorists who have tried to describe him. He relies to a great extent on processes that researchers have precluded from their research. His approach--his higher order rationality--resides in those processes we have reluctantly called "intuition". And if there is one overall theme in my research, it is that intuition is very much alive in organizations (even if it has been hiding all these years in the mute right hemisphere of the human brain*)--in the nature of managerial work, in strategic decision making, and in strategy formation.

This is in sharp contrast with so much that is written and taught about management--POSDCORB, MIS, MBO, LRP, PPBS, systems, systems, and more systems. Many years ago, Frederick Taylor said: "In the past the man has been first; in the future the system must be first."** How prophetic. Systems now dominate--in how we think about management and how we think about methodology. Rationality, systems, rigorous methodologies--these constitute one meta-configuration (if I may be permitted the term) whose elegance has, I believe, hidden its nakedness all these years. I like to think that my work points to another meta-configuration, one that reintroduces the man into management and methodology--managers who can be intuitive and achieve a higher order rationality, and researchers who can be detectives and thereby help to describe the kinds of organizations these managers lead.

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*H. Mintzberg "Planning on the Left Side and Managing on the Right" Harvard Business Review (July-August, 1976)

**Quoted in J.C. Worthy, Big Business and Free Men (Harper & Row, 1959), p.73.

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The Theory of Management Policy

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*This book and those on the next four topics will be published by Prentice-Hall in a special grouping entitled The Theory of Management Policy Series.