

THE ILLUSIVE STRATEGY

...25 YEARS LATER

by Henry Mintzberg

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Admonitions

I was going to close this piece with the advice to the young scholar that you should always take your work seriously but never yourself. I put it here instead to express my apprehensions in doing this. I think it is useful to have on record comments on how careers that were lucky enough to emerge successfully unfolded, but there is the danger that the person in question will be taken, and will take him or herself, too seriously. To have succeeded in studying something or other has never made anyone intrinsically interesting; indeed I find some of my successful colleagues terrible bores.

In line with this, I try here to avoid discussing my private life. That is my own business; the issue in question is my working life. But because the two are obviously intertwined, I would like to make a single comment here about them. When I wrote on the back cover of *Mintzberg on Management: Inside Our Strange World of Organizations* (1989a¹) that it "is written for those of us who spend our public lives dealing with organizations and our private lives escaping from them," I was not joking. That, if anything, has characterized much of my behavior.

I am intrigued by organizations; all my work has set out to understand them. But when I play, I distance myself from them as far as possible. For example, I love to cycle on back roads in Europe, but I would never dream of taking an organized tour. Sure, I need an airline to get me there, but once I get off the plane, typically with a friend or two, we just get on our bikes and go. I hate to be organized by organizations. So my fascination with them works best at a distance, in commitment at least - not space, because I love to get inside them, as an observer or temporary advisor, and sense their behavior.

To tell my career story, I shall begin with how I fell into this business - academia as a vocation, and about business itself, or at least organizations in general. Then I shall outline my career in three phases. But in order to do this, I shall present some hard data - tracks of some patterns in my behavior over time. I can explain this in terms of the title of this essay.

I spent the last semester (Fall, 1990) at the London Business School. A member of the strategy group there called me in Montreal in the summer to arrange a faculty seminar and I was to get back to him on the title. When I didn't, he left a message that it would be called "The Illusive Strategy." Perfect, I thought, I'll speak to that. It was an inspired suggestion (of Charles Hampden-Turner).

Strategy formation has been my most sustained subject (as will be seen in the data): it was the subject of

¹ Years in parentheses without names refer to items on my own publication list, of books and articles, available as separate files on my website.

my first article, my greatest number of articles, and my steadiest stream of articles. Much of this work has revolved around the definitions of strategy as realized (pattern in action) in addition to intended, and emergent (realized despite intentions) in addition to deliberate (intentions realized; 1972, 1979, 1985, 1987). Nonetheless, my own realized strategies have, if anything, tended to be rather deliberate. At least until recently. In the talk in London, I wished to review my work at that time, which involved a rather wide-ranging collection of papers and projects. Because the patterns among them may not have been clear, I thought it would be fun to use the talk to search them out - to infer my own strategies. Hence the appropriateness of the suggested title there, and my use of it here.

In 1978, I wrote a working paper entitled "Ten Years Later: Some Personal Reflections on Management and Methodology," to review the first ten years of my career (parts of which appeared in "An Emerging Strategy of Direct Research"; 1979b). So here we have "twenty-five years later," more or less.² One final warning. This is my career story as told by me. It is not reality but my own reconstruction of reality through my own perceptions. I did some research for this paper - went back into old files and documents, reviewed all the c.v.s I did over the years, reread some of my earliest papers, did a systematic analysis of my publications and course teaching, etc. That helped me to pin some things down, but it also revealed the fallacy of my memory if not my outright biases. While such a reconstruction may be of interest in and of itself, it should be read only for what it is.

Origins of My Career

I was hiking on the moors of Somerset with a friend a few weeks before writing this when he suddenly asked "How did you come to study organizations anyway?" "I don't know," I answered, "it just happened... One thing led to another. I never really thought about it. But it's worked out quite well." I guess I should try to answer his question here, which will require a bit of personal background.

I was born to a pretty comfortable family; my father owned a successful small firm that manufactured women's dresses. It may be true, as I claimed in the preface to my first book, that as a young boy I wondered what my father, as manager, did at the office. But this was certainly no more than a passing curiosity. Overall, I think I grew up as a pretty ordinary kid, not a bad student but never one in danger of being selected "most likely to succeed."

After reasonable grades in high school, I entered engineering studies at McGill, in mechanical because I

² Pardon the slight ambiguity; "Ten Years Later" was meant to date from the start of my teaching career; the data I present on my publications covers exactly twenty-five distinct years, back into one year as a doctoral student.

used to love to tear engines apart (although I could never quite put all of them back together again). I really wanted to do industrial engineering, but McGill had no such program. My grades were average or a bit better, but in any event, engineering grew into an excuse to do extra-curricular activities. Summers were spent mostly working in factories, from die making to time studies.

When graduation came, I do not recall having very clear intentions, other than that I was determined not to work for my father. I had to know if I could make it on my own. So I half-heartedly registered for the cycle of company interviews, and after discussing the future prospects of the McGill Redmen (football team) with several personnel types who read on my c.v. that I had been sports editor of the *McGill Daily*, I walked into an interview with Canadian National Railways. Imagine, this guy had a beard! (The year was 1961; only Fidel Castro had a beard then.) Not only that, but he was a biologist, working for the railroad, and talking about these strange studies he and a mixed group of colleagues were doing under what he called "operational research." When he looked at my c.v. and asked "What did you accomplish as sports editor of the *McGill Daily*?", I knew this was the place for me.

So there I found myself, doing OR when it was still common sense analysis rather than a lot of greedy technique. The CN was an exciting place to be in those days, one of the most progressive railroads in the world.

It was a good way to begin. For example, at one point I found myself fishing in a hump yard. A hump yard sorts incoming trains by passing them over a hill, off which they role one car at a time, switched into the appropriate outgoing track and braked automatically according to various parameters fed into an analog computer - the distance to the last car on the track, coefficients for the friction effects on the track and the car, etc. - to ensure an impact speed great enough to couple and gentle enough to spare the car and its contents (about 2-4 mph. as I recall). It was wonderful, new technology. Even if the yard was littered with the debris of broken coupling gears! So a Rube Goldberg type in the CN lab made a fishing rod, with a magnet for a hook and a speedometer on the reel, and I went a-fishing - to catch a histogram of impact speeds. Amid great blasts of mating from cars labelled "chinaware - do not hump," I drew my chart of coupling speeds: a fair proportion at zero that never made it, some in the desired range, and many others on up (well into the double figures, I recall - anyone who wants to learn about organizations should just once stand next to two boxcars meeting at 12 m.p.h.). The upshot was a meeting of the executive committee in which a presentation by a regional vice-president about the glories of his Montreal hump yard was followed by the flinging my histogram on the table by our vice-president of research and development. A political battle ensued, and I was learning about organizations - at the top and the bottom, as well as all that empty space in between!

I always intended to go to graduate school, certainly not one of those soft business schools with all those obnoxious (later-to-be called) "fast trackers," but for a master's degree in industrial engineering or operational research, to become a consultant to small businesses. An uncle of mine, Jack Mintzberg, with whom I was rather close, had encouraged me in this direction, in fact hired me the previous summer to develop a costing system for his tag and label company after sending me to learn about it on a course in the United States. (My c.v. still lists my very first speech - in 1963, four years before my second - to the Society of Paper Box Manufacturers of Quebec, arranged that summer by Jack. I should add that despite his directing me toward business, it was Jack, I later came to realize, who first planted the seed of an academic career in my mind. As a young man, he had worked as

a research assistant for Hans Salye, the eminent physiologist, and always regretted having given in to family pressures to go into business. But if an image was set back then, it was deeply buried in my subconscious, because I recall having no pretensions whatever of a career in academia. It was not that I dismissed my academic record as insufficient; I no more aspired to be a professor than to try out for quarterback of the Green Bay Packers.)

At the suggestion of an acquaintance, I applied to the industrial engineering master's program at New York University, well ahead of time. Finding myself one day in New York, after having been accepted, I called to meet someone in the department. After getting the runaround on the telephone, I decided this would never do, and so went over to Columbia to apply to their department. Still I would not consider a business school. But, of course, MIT was not a real business school; it was then called the School of Industrial Management and it gave a Master of Science degree. There was no way I would get in with my grades, but on a lark I applied anyway. For some reason (perhaps my extra-curricular activities) they accepted me, so I had a decision to make. I went to see Sebastian B. Littauer, the grand old man of industrial engineering at Columbia, and he said "Go there; we could never do for you what they will do." One of those critical moments of one's life.

And so this aspiring industrial engineer went to MIT, and within weeks was writing articles in the student newspaper condemning the excessiveness of quantitative materials in the curriculum. I even published an editorial in November dismissing any claims that they couldn't change the program by January. (I still had a bit to learn about professional bureaucracies!) The old journalist in me had come out once again (I would likely have ended up in journalism had I not become an academic...maybe I did!), not only literally in my extra-curricular activities but also in my attraction, to the softer interpretations of reality in place of the hard core analyses that surrounded me in the classroom.

I am not sure why I applied to the doctoral program at M.I.T. Perhaps it was the easy thing to do (easier than getting a job), perhaps I was getting increasingly interested in some of the softer questions of management - probably a bit of both. My grades in the master's program had been good but certainly not top; my GMAT of 602 was not bad for those days (though, based on figures I have seen recently, I would probably not even be considered for the current MIT master's program).

I applied to do the degree in policy. At the time, MIT had no area of policy, no professor of policy, no doctoral concentration in policy. All that obviously suited fine someone who wanted to escape the control of organizations. That it also suited the doctoral committee is, I believe, a tribute to its members' open-mindedness. A professor of operations management named Edward (Ned) Bowman, who had just returned from a year out as assistant to the president of Honeywell Computers, and was teaching one first policy course, had just taken over the chairmanship of the committee, and on informing me of my acceptance also said he had decided to supervise me himself, to find out what this field of policy was all about.

It was an unusual course of study to say the least - in terms of American doctoral program conventions, if not European. I went into Ned's office one day to ask what I should read for my comprehensive examinations, and he replied, more or less: "I don't know. Why don't you just draw up a reading list and read it." He did add a few books to the list I drew up from my own reading, helped by a visit across the river to Roland Christensen at Harvard

which had lots of policy doctoral students. I also recall vividly - though the now chaired professor of policy at Wharton does not - that after walking away from a brief meeting with Ned in the hall, he called out that "I've decided there is no future in policy." "You'll change your mind," I called back.

I had a clear mission in my studies. Theory was challenging cases in those days, inspired by the Carnegie Graduate School of Industrial Administrative innovations of Bach, Simon, Cyert, et. al., and MIT was one of the faithful adherents. But policy, and management in general, were stepchildren in these schools, often barely taught at all (see, Len Sayles' [1970] "Whatever Happened to Management"). Why could policy not be taught conceptually as marketing and finance were then so commonly done? So I set out on a search for conceptual materials - mostly in related fields, as there was little research base in policy itself - and began to outline a theoretical approach to the field. In December of 1965, just a few months into the doctoral program, I submitted a course paper titled "The Future of Business Policy," which I wrote was "in response to a request by my program supervisor, Ned Bowman, to try to define the field. The explicit objective of this paper is to argue for the recognition of Business Policy as a management discipline at MIT." I viewed the field in terms of two processes, "guiding the firm: strategy making and planning" and "leadership: purpose, relationship to society, leadership style, and power." A section on "The Research Base" categorized the "underlying research" in terms of power, game theory, the Carnegie School, military strategy, and organizational goals, and the "applied research" in terms of leadership, firm in society, business policy texts, systems analysis (PPBS), and long-range planning, all supported by numerous references. (An appendix listed forty-two books that I had read, etc.). I concluded that while "the literature is growing rapidly," the field appears to be less developed "largely due to the fact that there has been almost no attempt to classify and identify the literature that has been published."

Ned Bowman left MIT before I started thesis work (to take on the controllership of Yale, later the business deanship at Ohio State, before going to Wharton, with periods back at MIT in between), but he was still there for my comprehensive examinations, which must have happened in late 1965. My major was in policy (but Ned did not present me with an examination that read "Write a comprehensive examination in Policy and answer it"), with minors in "Organizational Studies" and "Information and Control Systems." My underlying discipline was Political Science, which included some weird course material on all kinds of ways to fight a nuclear war.

With the exams behind me, there was merely the question of the thesis. Strategy making was my main interest, but in truth I had no sense of what I wanted to do, and I wasted six months finding out.

Igor Ansoff's book, *Corporate Strategy*, had just appeared (in 1965), and I was as taken with it as everyone else. So I decided I would try to extend the application of the Ansoff model from mergers and expansions to strategic planning in general. (Another course paper I wrote in December 1965 had considered "the arguments for and against planning" and outlined a model based on the conclusion "that bureaucracies can and must plan." I also came across a thick file of thesis proposals from March to September 1966, about a "Programmed" or "An Analytical Procedure for Strategic Planning.") But, once again, I was saved from myself: I could not find an organization in which to apply the model (or, nearer to the truth, my feeble attempt to convince the new dean at MIT to let me do so in the management school failed).

Some time earlier, James Webb, who headed up NASA in the Apollo era, approached that same individual

(Bill Pounds) to be studied personally, as a manager. He believed NASA would ultimately be evaluated by its technological spin-offs on this planet, and he counted among these its own management advances (including, evidently, his own managerial style). As the only doctoral student at this school of management interested in management, I was approached to do this as a thesis. I dismissed the idea quickly - studying one chief executive was no way to make your way through the bastion of science that was MIT. I did, though, get a wonderful tour of NASA installations with several faculty members.

That was in late April of 1966, and immediately upon my return to MIT, a conference took place to discuss "The Impact of Computers on Management" (the title of the subsequent book of proceedings, Myers, 1967). There I saw a number of impressive individuals bog down on the question for lack of a conceptual framework within which to consider managerial work. Gulick's (1937) POSDCORB (planning, organizing, staffing, directing, coordinating, reporting, and budgeting) was not of help, and in their discussions, they could scarcely get beyond attempts to equate computers with programmed activities and managerial work with the unprogrammed. It occurred to me, having listened to people who certainly "knew" what managers did - all were involved with management in one way or another, including a number as successful managers themselves - that they did not "know" conceptually. And without that second kind of knowing, many of the most critical issues in management simply could not even be addressed. Clearly we needed to take a closer look at what managers really do.³

And so, bastion of science and all that notwithstanding, I came to study managerial work for my doctoral thesis. No-one had bothered to tell me that doctoral dissertations are supposed to probe narrow, researchable issues; I don't bother to tell my own doctoral students either. (One professor did once tell me that an MIT dissertation should be "elegant;" he was referring to the method, not the results, and I have always prided myself on the inelegance, or at least the simplicity, of the methodology I used to study managerial work.)

Webb was no longer available, and I decided that I would observe the activities of five chief executives.

³ When this occurred to me I am not exactly sure. The conference was in April and I began work on the dissertation by November at the latest. I have on file a dissertation proposal called "Programmed Strategic Decision-Making" dated September. So there must have been some kind of delayed reaction. On the other hand, I did a course paper in May 1965 called "On-Line Real-Time Presidents? A Study of Computer Applications at the Presidential Level" that included interviews with two Boston chief executives and descriptions of their work. So the topic had been on my mind even before the Webb approach.

That number had no special significance, other than being several and manageable, nor did my choice of organizations, except to ensure that all were in different domains. (Years later, having done a categorization of organizations [1979a], I realized there was somewhat of a bias toward professional bureaucracies and adhocracies. But I doubt this much influenced the dimensions I was studying.)

I started to write letters to possible subjects. I hit it lucky quite early. James Gaven, who headed up the Arthur D. Little consulting firm, accepted immediately by return mail. He was well known in the U.S. at the time as the first retired army general to have publicly criticize the Vietnam war effort. Thus, when I received a telephone call from the secretary of John Knowles, head of the Massachusetts General Hospital, saying he wouldn't do it but that he would like to talk to me, after he offered various reasons, I chipped in with "That's too bad because General Gaven has accepted," and without losing a breath, Knowles added "and that's why I can't do it for at least several weeks!" (Knowles was a wonderfully extroverted subject; years later he told me someone gave him an article I did on the research with the comment, "John, this sounds exactly like you!") I do not recall having to ask more than about ten chief executives altogether; the others who accepted were Bernard O'Keefe of E.G. & G., Inc., Harry B. (for Bulova) Henshel of the Bulova Watch Company, and, of course, Charlie Brown, this one head of the Newton Massachusetts school system. They were a wonderfully cooperative group.

Jim Hekimian, an MIT professor of control with an interest in policy, took over the chairmanship of my committee, which included Charlie Myers in industrial relations, and Don Carroll in operations management. (As Jim left MIT to become dean at Northeastern, Don ended up officially signing the thesis as chairman, before he too went off deaning, at Wharton.) For the most part, they knew as much - or, I should say, as little - about the subject as I did, except for Charlie Myers who had touched on it in his book with Harbison, *Management in the Industrial World* (McGraw-Hill, 1959) . But they formed a wonderfully enthusiastic support group that encouraged me to put in everything (hence the host of asides in my thesis).

When the time came to defend the thesis (an event of obvious consequence at MIT - one doctoral student showed up aside from my committee), after I presented the results, having been assured earlier by Charlie Myers that there was nothing to worry about, the committee deliberated for about twenty minutes. An anxious me was finally informed that, oh, they were just discussing the publishing possibilities.

Having been assured by someone like Charlie Myers that the thesis was publishable, I kind of dropped it in the mail to a publisher (McGraw-Hill as I recall), not quite with a note as to where to send the checks. The reply was in kind, more or less to "occupant," saying "No, thank you." Not to worry, it must have been a mistake, so I sent the thesis off to a second publisher. Perhaps a dozen publishers later, I had no contract. So Charlie Myers stepped in and proposed it to the MIT Press. It seemed headed for publication when someone on their board questioned it and out it went for another review. I met that reviewer years later, who apologized to me. Carnegie-Mellon methodology jock meets sample of five, and I was back at the beginning.

It was at that time that I surprised my wife one evening by blurting out that I knew this was an important piece of work - that I just knew it would be prominent one day - and so, in the face of all those rejection letters, I would rewrite it and proceed. I was not prone to such claims, and I was not expressing a wish or some manifestation of arrogance so much as what felt like a certainty.

And so I rewrote the thesis and resubmitted it to the whole cluster of publishers. Again they all rejected it, luckily except for Harper and Row and Random House. And so the former published *The Nature of Managerial Work* in 1973. To quote the last line of Philip Roth's *Portnoy's Complaint*: "So [said the doctor]. Now vee may perhaps to begin. Yes? (Random House, 1967, p. 274).

Some Real Hard Data

My occasional diatribes notwithstanding, hard data is not a bad thing; indeed some of my own articles are full of it. It helps to pin down some of the vagueness (and in turn to create some of its own). So I thought it might be useful to present some tangible traces of my own work, before I begin to describe the phases of my career, as I see them. I took all my publications of any consequence (i.e., excluding letters, short newspaper articles, etc.), whether good or bad, academic or not, and categorized them in three ways: first as to whether they were empirical (deriving directly from my own research, interpreting this rather narrowly as articles rooted in the research rather than drawing off it), substantially conceptual, or practitioner (intended to populize findings); second as to subject matter, comprising strategy making, managerial work, organization (including structural and power issues), management in general (including a few publications on research and on the field of policy), analysis and/or intuition, and decision making; and third, as to co-authorship. The histograms for all of this are shown in Figure 1. Books are shown shaded in; obviously each was the equivalent work of many articles.

I published my first article, called "The Science of Strategy Making," as a doctoral student in 1967, and with the exception of 1969, my second year as a professor, I have published every year ever since, from a single article in five of the twenty-five years in question to as many as six articles and two books in 1983. (Figures for 1991 include material already accepted for publication; I write this in February 1991). Probably the most consistent substream is the empirical, almost regularly one per year, interspersed by some empty years and a few with two publications. But the conceptual stream is far fuller, with more than double the number of publications and sometimes quite frequent in a single year (e.g., 1983 with the two books and five articles). Partly this reflects my propensity to conceptualize, but it also reflects the fact that conceptual articles, especially when spun off books in which I had already worked out the issue, were easier to do than empirical ones. But I should add that I always took great care and time with almost all of my writing, books as well as articles, conceptual and practitioner as well as empirical, with five drafts or more being the norm. Practitioner publications represent a thinner stream, more sporadic, but indicating my commitment from early on to trying to reach both audiences. Indeed, in mid-1976 I took great pride in having published at the same time one article in *Administrative Science Quarterly* and another in the *Harvard Business Review* (1976 a,b).

In terms of content, it could be concluded from the data that I passed from one focus to another, initially on managerial work in the early 1970s, then over to analysis and its relationship with intuition from the mid-1970s, then to a heavy concentration on organizational issues (especially structure, power, and forms of organizations) from 1979 to 1984, and then to a heavier and more sustained focus on strategy making through the rest of the 1980s, with a rise at the end in the management-general category, representing especially two editions of a

textbook (1988a, 1991a) and one summary book for practitioners (1989a).

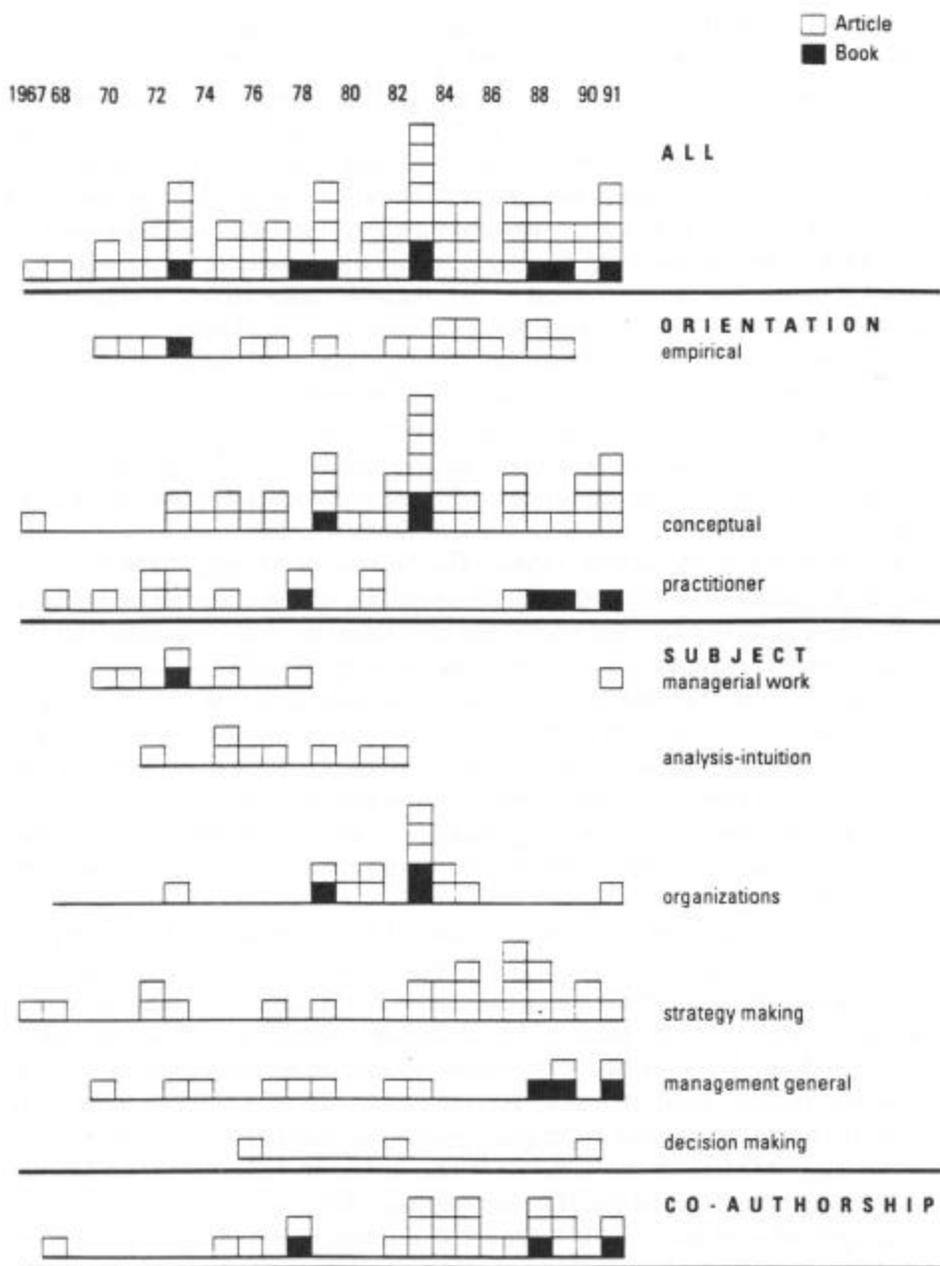


Figure 1. Publications

Three articles on decision making are shown, well spread out, although the heaviest concentration of work was in 1973, for the first one (published in 1976 [a], which indicates the need to take into account the long lead times in these figures). Management-general shows a thin trickle from early on to 1983, representing some of my practitioner articles as well as ones on research and on the field of policy. Finally, while strategy making clearly peaks as by far my heaviest concentration of articles ever, through most of the 1980s, that is a theme I have

returned to throughout my career. Indeed, that first article as a doctoral student, in 1967, opened with a comparison of two approaches to strategy making that anticipated my much later comparison (1979a) of deliberate and emergent strategy:

Man's beginnings were described in the Bible in terms of conscious planning and grand strategy. The opposing theory, developed by Darwin, suggested that no such grand design existed but that environmental forces gradually shaped man's evolution.

The disagreement between the biblical and Darwinian theorists is paralleled on a more mundane level in the study of strategy-making. There are those who envision grand calculated designs for the corporate entity, and there are those who cite current practice to argue that organizational strategy evolves, shaped less by man than by his environment. (1967:71)

I have included a histogram on co-authorship for what it may reflect. In general, I was a solo writer for most of my early years (although I did co-author my second article with Jim Hekimian, in 1968). There was some joint work in the mid-1970s, based on a contracted monograph (1975b), as well as my first article on decision making with two of my students (1976a), but co-authorship became a serious and sustained activity only after Jim Waters joined the McGill faculty in 1976. Jim left McGill several years ago, and we suffered his tragic loss a short while ago. In the last few years, I have had an equally delightful collaboration with my colleague, Frances Westley, again around issues of strategy, although in recent years my variety of co-authorships has increased significantly (as can be seen, alongside everything else here, in the attached list of these publications). Finally, I should mention my collaboration with Danny Miller. Danny was my first doctoral student, and we have shared ideas closely and energetically for many years, although our formal collaboration has been restricted to that 1975 contracted monograph and one published paper called "The Case for Configuration" (1983a).⁴

In Figure 2, I plot all my teaching activity. (My accessible records here are not complete, so there may be some inaccuracies, but this should not affect the overall patterns; the year recorded refers to the first of the whole academic year. e.g., the Carnegie course shown in 1972 was actually taught in the Spring of 1973.)

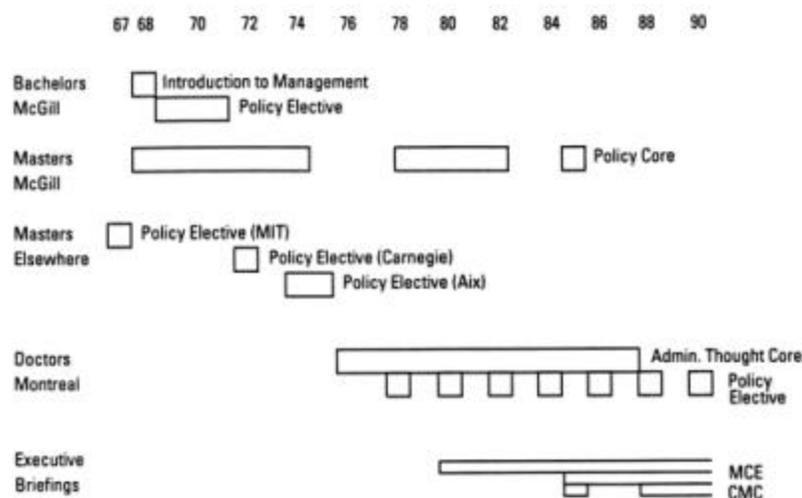


Figure 2. Teaching

⁴ As Danny used to be somewhat of a recluse, we had this line about Danny Miller being the pseudonym I use for my quantitative articles.

As can be seen, I have not taught a great variety of courses, given the number of years. The mainstay of my teaching was mostly the MBA policy course; I was hired at McGill in 1968 to take over the full year core course that had fallen into disarray. In general, I taught one or two sections of it in almost all my years at McGill until recently, aside from periods of sabbatical or leave (to France in 1974-76 and Switzerland in the winter of 1983) and a three and a half year spell running the doctoral program in the late 1970s. I taught a similar master's elective course as a doctoral student at MIT, again as a visiting professor at Carnegie-Mellon in the Spring of 1973, and again on a sabbatical followed by a year's leave of absence in Aix-en-Provence, France in 1974-76 (technically in a "third cycle doctoral" program, but coded, as I see it, as master's level). I also taught some undergraduate courses in my early years at McGill - an introduction to management in my very first year and an elective "Seminar in Organizational Strategy" in the next three. In 1986 I negotiated myself out of McGill MBA teaching altogether to concentrate my efforts on research, writing, and doctoral training, at a reduced salary. (Figure 3 tabulate my appointments, sabbaticals and visits, etc.)

Our doctoral activity started up (as a joint program among the four Montreal universities) in 1976; I played a major role in its design and championing, and when I returned from Aix-en-Provence in 1976, near its inception, I was asked to run it, which I did for three and a half years. For many years, I co-taught the introductory required course for all doctoral students, called "Fundamentals of Administrative Thought," and I have also offered a policy elective every second year, more or less, beginning in 1976 and occurring again right now, which remains my only teaching commitment at McGill.

Finally, the chart shows a stream of "executive briefings" beginning in 1980. This is a two-day public program that I do myself, which draws much of my work together for managers, who register from a wide variety of contexts. Originally offered by the Management Centre Europe (a Brussels-based training group associated with the American Management Association), it has become a regular activity, expanding in Europe from one program a year to two in 1985, and then to Canada on a regular basis by their sister organization in 1988.

These data suggest (to me at least) that there has always been a clear pedagogical focus in my work. In fact, one central course has always served to integrate much of my thinking and activity, including the stimulation and direction of much of my writing. In the earliest years of my career, it was the McGill MBA core policy course. After my return from France, that focus shifted to my doctoral teaching, particularly the administrative thought course. And in recent years, the two-day briefings have emerged as the focal course, directing me to particular issues and helping me to address them. As I shall specify later, the push of theory has gradually been supplemented by the pull of issues in my approach to the world of organizations.

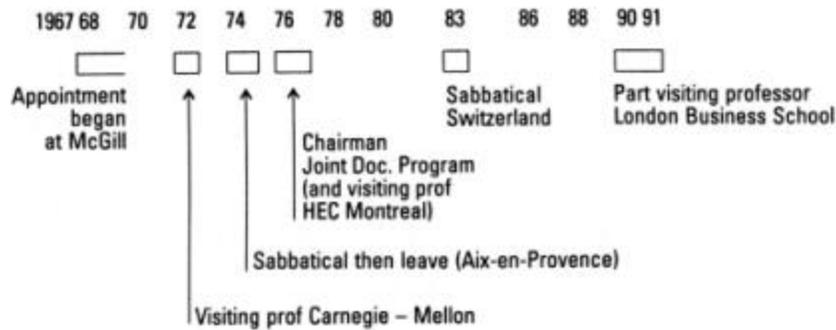
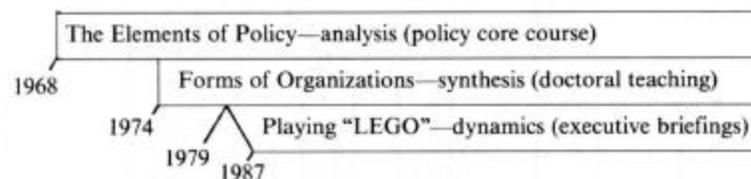


Figure 3. Appointments

Those hard data that tell the story best I shall save for later. These are the diagrams that I have used in my publications over the years. When, as part of this exercise, I begin to consider all of them chronologically, the results were most startling: they clustered into three clearly distinct groups.

All of these findings, reinforced (or produced in the first place) by my prior beliefs, cause me to see my career as having unfolded in three fairly distinct phases. I can, in fact, identify the commencement of all three with rather tangible events, although the second and third took some time to manifest themselves fully. It is not so much, that one phase ended when another began. Rather the second and then the third added to the first as my mindset shifted over time. As we travel through life, we don't so much replace baggage as add to what we already have. To be discussed in the following sections, the three phases can be labelled as follows:



Rectangle Phase: The Elements of Policy-Analysis (from 1968)

I returned to Montreal in 1968 with not only a thesis but an outline for a book to be called "The Theory of Management Policy." I then set out to write it, chapter-by-chapter, week-by-week, the first time I taught the McGill MBA Policy course. I am still writing it!

The MIT management school back in those days was also a bastion of theory (as was McGill's, it should be added, Canada's representative in that small club of the late 1960s), what we saw then as one of the Carnegie-inspired lights of conceptual clarity glowing in the darkness of all that Harvard case study chatter. Harvard's policy textbooks (the textbooks of the time) were (and remain) either devoid of conceptual material or else soft peddled bits of it lightly. Policy or general management was, as a result, almost absent from these theory-based schools. But my doctoral studies had convinced me that it need not have been: lacking was not teachable theory so much

as someone to pull together all the relevant theory that did exist, much of it in related disciplines. I intended to be that someone. I simply misjudged the task, or at least my obsessive way of going about it.

The outline I had developed by 1968 was not, I suspect, much different from the earliest one I could now find, dated 1973, shown in Figure 4, which itself did not much change subsequently (see, for example, the comparable published diagrams in 1979c:iv and 1983a: viii). I opened files on each chapter, to collect notes and relevant articles. Pendaflex folders soon became boxes, and the boxes soon began to overflow and multiply. I code named each chapter TT1, TT2, etc. (for The Theory of Management Policy) for purposes of filing notes, etc. And so I had my writing plan all set out for me - my intended strategy was well formulated, merely to be implemented.

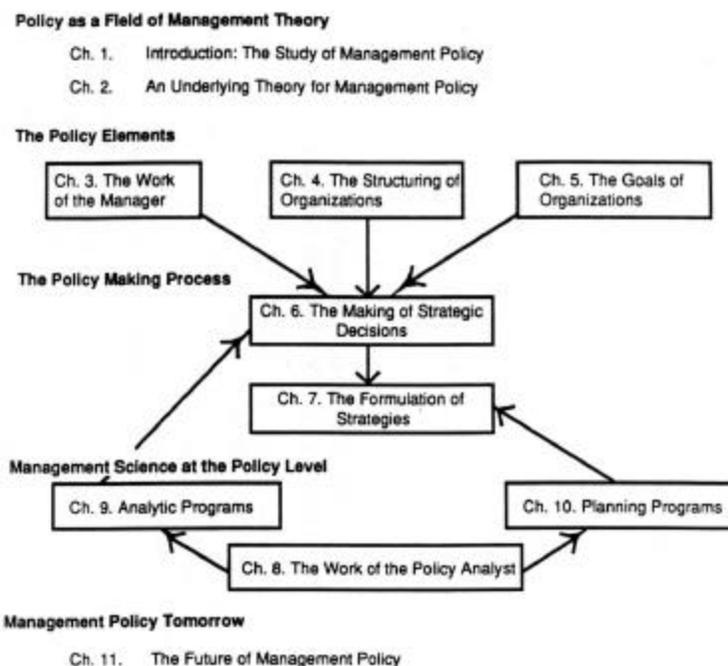


Figure 4. Outline of "Theory Management Policy" circa 1973

As I wrote the chapters, I began to bind them together to hand out as a kind of text in my MBA policy course. The oldest version I have of this (and probably the first, bound at least) is dated July 1972, although I did find the 1970-71 course outline which showed me handing out Chapters 1,2,3 (on goals), 4 (on structure), and 7. The 1972 text opened with a fifteen-page bibliography, followed by a working paper entitled "Policy as a Field of Management Theory," which argued:

The student of Business or Management requires a useful conceptual framework with which to view the world of Policy that he shall face during his career. He requires answers to the following questions:

- o What is the job of the manager?
- o How do organizations determine goals?
- o How do different organizations develop their structures and which are appropriate for each?
- o By what processes are significant decisions made?
- o What are the organizational strategies, and how are

- o they made?
- o What is the role of management science at the Policy level?

The Management Policy Course should provide answers to these questions on the following bases:

1. The answers to questions of Policy must be based on empirical research. We must observe and study the management process in a systematic way. Then, what we teach our students should be researched or at least researchable.
2. The answers should blend into an integrated theory of Management Policy. We must bring together the theories that we now have, integrate them based on some underlying theory, and use research to fill in the gaps that remain. We have much to do, but we have the basis for a beginning. Many theories are available that provide partial answers to our questions, although they are often not recognized as Policy theories. But so long as they shed light on the questions asked in the Policy course, they must be used there.
3. Description must precede prescription. There has been too great a tendency to prescribe in the literature of Policy, to tell managers how to manage without first understanding why they do what they do.... We must avoid the temptation to arm our students with simplistic prescriptions (e.g., planning is good per se); rather we and they must come to understand the complex processes of management. Prescription is meaningful only when it is grounded in valid description.
4. The Policy course should integrate the lessons of management science. The Policy course is the integrative one of the MBA curriculum, linking the applied fields of marketing, finance, and production. The modern curriculum places increasing emphasis on the scientific tools of these fields (e.g., marketing research, capital budgeting, mathematical programming). The Policy course can maintain its integrative role by serving to interpret the lessons of management science for the policy maker. The Policy student should learn to assess the relevance and weaknesses of each management science technique (especially strategic planning) in the light of his or her knowledge of the actual management process. The Policy course must walk a line between behavioral science and management science, drawing on one for descriptive theory, on the other for prescriptive theory.
5. The Policy course must link theory with practice. Theory alone is no better than practice alone. The reality of Policy is sufficiently complex to require that the student have the opportunity to assess the relevance of theory in practice. Ideally, the students armed with theory, will observe reality firsthand, via a field study or live case. Alternately, he can assess his theory in the context of a written case or business game.⁵

That working paper became Chapter 1, which was rewritten in 1974 and 1978. It was never published as such, although parts of it appeared in two different articles, both titled "Policy as a Field of Management Theory" (1973b and 1977a). Chapter 2, titled "An Underlying Theory for Management Policy," sought to build some roots for the material to follow by combining the administrative theory of Herbert Simon (e.g., 1957) with the general systems theory of Ludwig Bertalanffy (1968). Although over twenty years old (and also rewritten in 1974 and 1978) and never published, I still intend eventually to turn it into some kind of article!

Chapters 3-5 dealt with the "policy elements." There was no chapter on managerial work in that first edition, and never has been. I was just publishing my thesis as a book and knew I could easily summarize it in chapter form when necessary. I did write an early chapter on structure entitled "Organizational Structure and the Coupling of Programs" (undated) which took up 67 pages in that 1973 edition, and an early chapter (dated 1970)

⁵ See 1973b, later 1977a, for published versions of this paper.

entitled "Influences and the Organizational Goal System," which took up 60 pages, the framework of which appeared in print only in one article for French speaking practitioners (1972e).

Chapters 6 and 7 considered the "policy making process," first in terms of strategic decision making (single important choices) and then strategy making (streams of choices over time). A very long version of the latter was written in the early 1970s (125 pages, undated), under the title "The Strategy Concept," although I included only parts of it in the 1973 edition. These comprised material I published as "Strategy Making in Three Modes" (1973d) and three pages of propositions under the label "A General Theory of Strategy Making," each backed up by considerable text in the full version (and never published).

For Chapter 6, the 1973 edition contained one page with the words "Chapter 6 to come." It came the next year. In January of 1973, I took off to Carnegie-Mellon for a semester, to soak up the energies of that famous school of administrative thought (Simon, Cyert, March, etc.), and, appropriately, I thought, to write up some data I had been collecting on the making of strategic decisions, which together with a literature review, was to become Chapter 6. Carnegie did not distract me from that task: it turned out that there was nothing left of those energies. Simon had gone off to the psychology department, Cyert was doing administration instead of writing about it, and March had long since left. For the doctoral students from Europe roaming the corridors, like me, in search of that glorious past, I became the closest thing to a resident expert on administrative theory! (I was, after all, writing a paper on decision making!) And so I did the chapter, quite large, about 80 pages, single spaced. It also remains unpublished, although its essence was captured in "The Structure of 'Unstructured' Decision Processes" (1976a), with Duru Raisinghani and André Theoret, McGill students in the MBA and doctoral programs respectively, who had helped with the earlier analysis of the data. Figure 5 reproduces a representative decision process from that article.

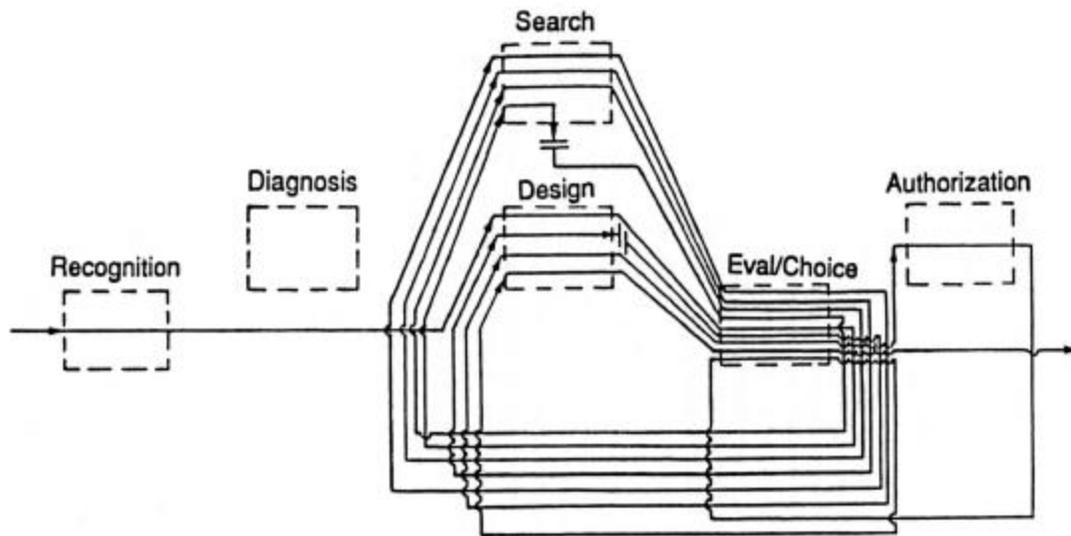
The third, prescriptive section of the book, on "Management Science at the Policy Level," consisted of chapters on the work of the policy analysts and their analytic programs for decision making and planning programs for strategy making. It was represented in that 1973 edition only by a six-page piece entitled "A Program for Strategic Planning" (dated January 1969), my rendition of the classic strategy model (which I now prefer to call the "design school;" see 1990c). A Chapter 8 on "The Role of the Analyst at Policy Level" did appear in 1978 (52 pages), which I shall discuss later.

And so I devoted much of what I am calling the first phase of my career (up to 1974) to writing the chapters of that book as well as to pursuing research that fitted in with those chapters - notably on strategic decision making and strategy formation. An initial proceedings publication (the only one in my reference list) outlined a major project that I was undertaking on strategy making through the analysis of patterns in behavior (1972a).

Likewise, most of my publications of this period related to these chapters, two on strategy making (1972a, 1973d) and several on managerial work, including my first book (1973a), based on my thesis, and various articles spun off of it (1970a, 1971, 1973c), including one that delved into the information systems consequence of the findings (1972c). Figure 6 shows the book's depiction of the manager's working roles. Miscellaneous publications of the period included two for Canadian practitioners (1970b and 1973e) and one for a Canadian government publication (1972b), spun off a consulting assignment, that sought to describe government activities in terms of

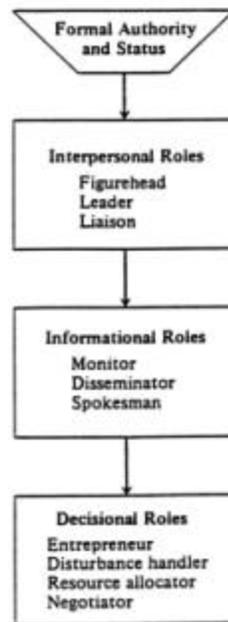
Maslow's (1954) needs-hierarchy theory.

I should also mention a project carried out for the U.S. National Association of Accountants and the Canadian Society of Industrial Accountants. They wondered why managers didn't use accounting information the way (accountants at least) thought they should, and were prepared to fund research to find out. One project came to us, to do a kind of compendium of "Normative Models in Managerial Decision Making," the title of the monograph published in 1975 (b), co-authored with Danny Miller, who came to McGill in 1972 as my first doctoral student in 1972, and Larry Gordon, a professor of accounting. But I felt they were wasting a lot of money funding original research when many of the answers were already in the published literature. So for a tiny fraction of their budget, I surveyed the literature and produced another monograph, called "Impediments to the Use of Management Information" (1975a). It laid the blame on hard information itself (too limited, too aggregated, too late, unreliable), as well as on the nature of organizations (rigid objectives, politics, verbal nature of managerial work) and the nature of our own brains (cognitive limitations, biases, psychological reactions to failures).



Source: "The Structure of 'Unstructured' Decision Processes" (1976, p. 273)

Figure 5. Illustration of Strategic Design Process



Source: The Nature of Managerial Work (1973), p. 39.

Figure 6. The Manager's Working Roles

Looking back on that phase of my career, I see myself as a rather conventional academic, except for my obsession with that textbook. I had my world neatly compartmentalized - organizations conveniently chopped up into various elements and processes - for myself probably more than for my MBA students. But they did serve as the focus of my pedagogical activity, the market force if you like, which in turn served to focus my writing and research. To bring some reality to the classroom, not to mention to myself, beginning with my first course in 1968 I sent those students out to groups to study Montreal organizations of their own choosing (see 1978a). In fact, in the early years they were given a sequence of assignments that corresponded to the main chapters: to study one manager, to describe the organization's structure and its goals, influencers, and coalitions, to trace one of its strategic decision processes, and to describe its strategy making process. Over the years, these studies (some of the reports of which became the data base for the 1976 article on decision making), provided me with a wonderful variety of examples, as well as the opportunity to test the applicability of my theoretical materials in practice, and to enrich them.

Research and especially conceptual development is what drove me in the first phase of my career, with a decidedly academic orientation, although I did do several practitioner articles. Despite some rumblings about my upcoming attention to emergent strategies and managerial intuition, in retrospect I see my work then as most decidedly deliberate and analytical. One need only look at all those nicely sequenced sets of rectangles in the figures (4, 5, and 6) that I was drawing in those days!

Blob Phase: Forms of Organizations - Synthesis (from 1974)

Leaving for sabbatical to Aix-en-Provence in the fall of 1974 proved to be a turning point in my life, or at

least coincided with one. Personally I opened up to the splendor of southern France, finding my escape from organizations in that rugged nature, and professionally an important shift began to occur in my mind.

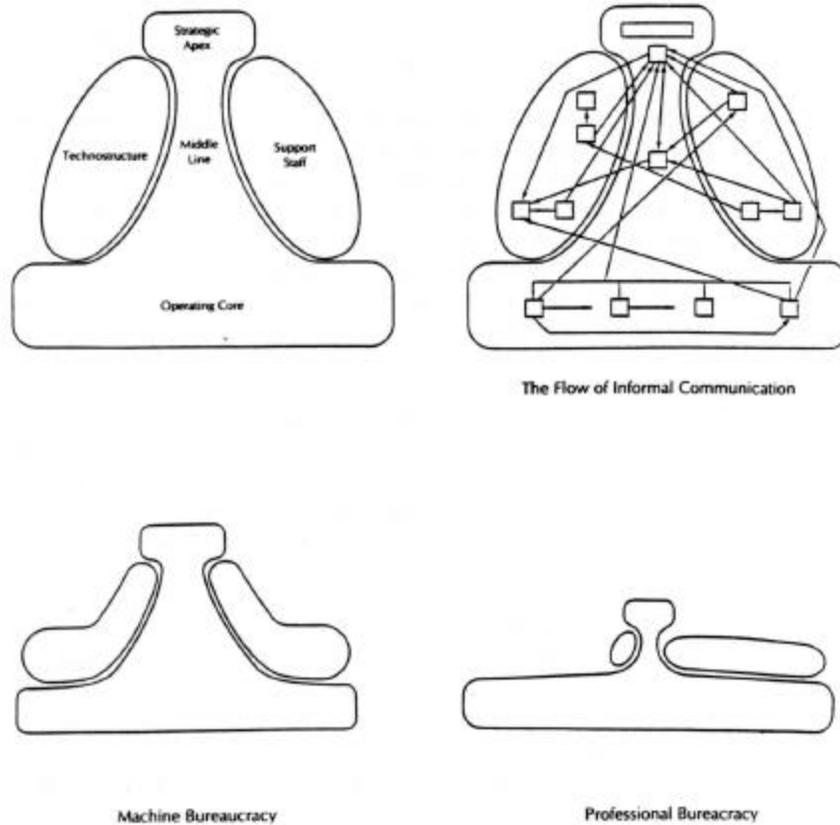
I began serious work on a rework of the structuring chapter before I left, having read all the literature collected in my boxes and setting out to develop a detailed outline to take with me. My memory is generally awful about many details, but one event I do recall vividly in the spring of 1974 is sitting in my basement office at home when a friend dropped in, and expressing to him my intense frustration in trying to draw the huge, disparate, and awfully narrow literature on organizational structuring into some kind of comprehensive framework. Bivariate relationships concerning "administrative ratios," "amount of control," "environmental heterogeneity," and the like, just didn't help. As Danny Miller and I explained in a later article (1983a), that was why the museums of organizational structuring were empty of people even if its archives were full.

Pradhip Khandwalla joined the McGill Faculty of Management in 1971 after completing his doctorate at Carnegie-Mellon. We soon became close colleagues and good friends. In his thesis, Pradhip found that organizational effectiveness depended less on doing any particular thing (such as planning formally or decentralizing power over decision making) than on the interrelationship among several such things done (such as centralizing power and staying small and remaining informal). Early on, as I recall, I thought of this finding as configurational. There the seed was probably planted for the resolution of my frustration in the basement. I have often tried, without success, to recall exactly how the idea of synthesizing the literature of organizational structuring around distinct configurations, or "ideal types" of organizations, came to me. All I do remember is the critical role played by the occasional insight, most notably in the work of Joan Woodward (1965) on forms of technology. On at least two occasions, when my struggles with anomalies in my notes were suddenly resolved as I looked through her rich description.

By the time I left for Aix in September of 1974, I carried a 200-page outline of the "chapter," so specific that I wrote the first draft of what was to become the 512-page book, *The Structuring of Organizations* (1979a) by December. (I keep that outline handy today, perhaps to remind myself of what I am capable of doing though never did before or since.) The full book, of course, loaded with references and quotes as befits a determined young academic, took much longer. (But later, I took much of that out for a textbook/practitioner version called *Structure in Fives* [1983b].)

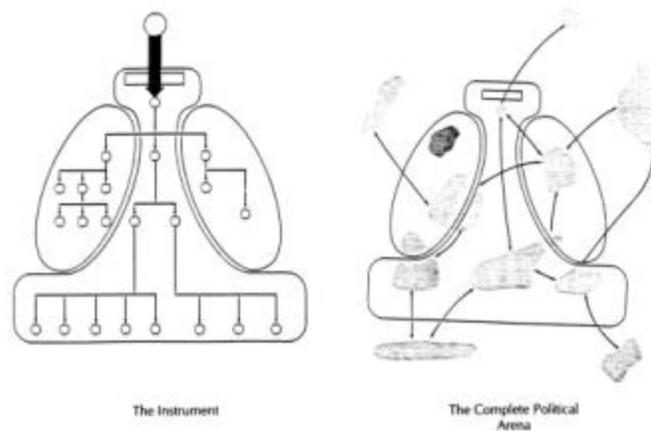
The first parts of the book laid out various elements of structure and the findings I had extracted from the research literature, more or less in their own terms. But this was a prelude to the last part of the book, which described five basic forms of organizations, labelled simple structure, machine bureaucracy, professional bureaucracy, diversified form, and adhocracy. Everything seemed to fall naturally into place in these five forms, so that the book achieved an integration that delighted me, down even to the link between the opening and closing stories. It remains my favorite publication, in form if not also substance. Part of the fun in doing the books was in my use a funny little diagram which has become my logo of sorts, overlaid and distorted in various ways to integrate graphically across the text, as reproduced in Figure 7. I gave my rough sketch of it to a young American woman artist in Aix to render it clean, and she took one look and declared it obscene - "everyone will see the same thing." Well, I certainly hadn't, and over the years this pseudo-Rorschach has been described as a mushroom (in

China!), my nose (by London Business School students), a telephone (at AT&T), the cross section of a rail, a woman's uterus, a kidney bean, and who knows what else. To me, it's just an organization!



Source: *The Structure of Organizations: A Synthesis of the Research* (1979).

Figure 7. Structuring Book Use of Logo



Source: *Power In and Around Organizations* (1983).

Figure 8. Power Book Use of Logo

The sabbatical was so enjoyable that I decided I needed another. Happily, the Aix business school (the

Institute d'administration des entreprises of the Université d'Aix-Marseilles) invited me to stay another year at their expense, and McGill granted me a leave. So I set out to write the next chapter on goals and power. That went less quickly: I failed to do a detailed outline, and paid the price for it in rewrite after rewrite over the next six years or so although it should be added that this literature was far more diffuse and nuanced, and so far harder to integrate than that of structure, even if I did have the notion of configuration from the outset). *Power In and Around Organizations*, exactly 700-pages long, was finally published in 1983(c). It laid out the elements of the power game, within and around organizations, to draw them together into configurations described in terms of power relationships. The logo renditions of these are shown in Figure 8. (I should add that I dragged along to Aix my file on another "chapter" - two full boxes, as I recall, on strategy making - and dragged them back to Montreal two years later, unopened.)

The thought had dawned on me by the time I left Aix, perhaps as a result of having to carry around those 512 and 700 page "chapters," that my textbook was becoming something else. So, back in Montreal, I convinced Prentice-Hall, the eventual publisher of both, to label them "The Theory of Management Policy Series," so that the original conception of the textbook could at least be maintained in a series of books. One day they might even be able to issue all the "chapters" in one jacket! (With this in mind, Prentice-Hall kindly negotiated permission to reprint *The Nature of Managerial Work* in the series, which it did in 1983, and I still have contracts, dated 1977 and 1982, for one volume on decision making and another on strategy making. Maybe I should frame them!)

The significance of these two books to my work was that, whereas before them I was cutting up the world of organizations in my terms - the conceptual categories of the academic - as a result of them I began to cut that world up in its own terms - forms of whole organizations. If before I saw the world like this

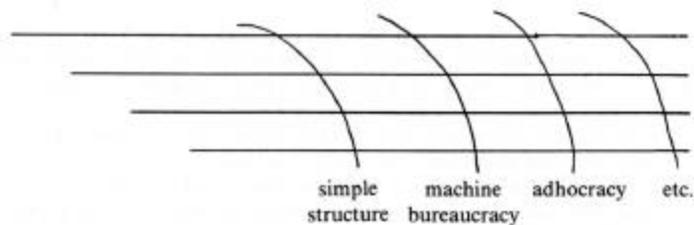
managerial work

structure

power

strategy making, etc.

with the horizontal lines slicing through organizations, now I was perceiving it this way,



seeking to combine the elements in identifiable contexts, however idealized. I began to realize - perhaps best articulated in my article with Danny Miller on "The Case for Configuration" (1983a) - that the field of organization theory needed to distinguish its species no less than did that of biology. Otherwise we would continue to distort our description and prescription, extrapolating research findings out of context and following the long and costly tradition of promoting that ever changing "one best way."

Thus, not long after I returned to McGill in 1976, I began to open a new set of files, with names such as

machine bureaucracy, adhocacy, missionary, and typologies, although I maintained the old ones too, especially the unfinished one on strategy making, whose literature was soon to mushroom. In effect, my mental set had been turned from the pat categories of organizational dimensions to the (no less pat) categories of organizations themselves. But that represented an important gestalt shift for me.

There was another shift too. Before I left for France, I had drawn up a proposal for a McGill doctoral program in management. When the other Montreal schools got word of it, given the need for approval at the level of the provincial government and the fear that our program might preempt others, they produced proposals of their own. The government committee reviewed our horse and countered with what looked like a camel - that we all get together and produce a joint doctoral program. So we sat down and did just that, and I believe (if some others don't) that the results have been excellent. Institutions are notoriously reluctant to cooperate, especially in academia. But somehow the 2 x 2 arrangement, two English, two French language schools, in each case one older and more established, the other newer and more applied, fell into a natural balance. The program was quite innovative - blending European self-study with American coursework, and involving serious cooperation in all aspects, including courses and individual student committees. Today it registers one hundred and seventy-five students, a plurality in the policy area.

My return was greeted with a request that I run the new program, which I accepted to do. So the management of adhocacy came to life, as I entered the world of mutual adjustment - no subordinates, just peers with whom to negotiate and cajole. I learned that I could do it, also that I could think strategically, but I also learned that the "calculated chaos" (Andrews, 1976) of practising management was not very compatible with the concentrated life of scholarship, and so I was glad to return to where I felt I belonged after three and a half years of managing.

There has been a long and solid Jesuit tradition in the schooling of French Quebec, such that philosophy runs deep in its best students. I, on the other hand, was educated in Quebec's pragmatic English schools, and became an engineer who never had a philosophy course in his life. So when a kind of philosophy course was proposed for the joint program, to be required by students from all areas and all schools, I was not terribly enthusiastic. Little did I suspect that upon my return from France, I was designated as McGill's representative to teach the course (alongside a colleague from each of the other schools). "Fundamentals of Administrative Thought" turned out to be one of the great pedagogical joys of my life; I remained with it for about ten years, using it to help socialize the incoming class into a coherent unit and to play intellectually with a wide range of ideas, as one can only do in a course that doesn't have to convey some established body of theory. A number of my ideas developed in that course, particularly concerning Herbert Simon and the role of intuition in management. I consider my review of his *New Science of Management Decision* (1977b), which grew out of work I prepared for that course, to have been an important step for me, as I shall discuss below.

I also began to teach a doctoral seminar on policy every second year, which I continue to do today. Thus, while I picked up my MBA policy course again, after completing my chairmanship of the joint doctoral program, my pedagogical focus was shifting from twenty-three year old aspiring managers to thirty-plus year old aspiring scholars.

Configurations had entered my mind, but the elements of the original textbooks's outline hardly left my behavior. In fact, by far my most concerted effort after returning from France was my research on strategy making.

It had been conceived in the early 1970s (see 1972a) around the notion of (realized) strategy as pattern in actions (originally decisions; see 1990a). With a sizable research grant, we began to track the behaviors of organizations over time, isolating streams of activities, inferring patterns in them as strategies, combining these strategies to infer distinct periods in their histories, and finally brainstorming around the results to develop theory about the formation (not just formulation) of strategy. (So now the logic of this paper should be evident, which means, of course, that it would have to be tabulated on Figure 1 as empirical!) I found a new colleague at McGill when I returned from France, named Jim Waters. We hit it off immediately, and after he convinced me to rename "emergent" what I had originally called "retroactive" strategy, a most productive collaboration emerged on this research (1982a, 1983d, 1984a, 1984b, 1985a, 1986, 1990a; some of these also including student co-authors). Of course, the configuration notion was not lost; gradually we began to see the influence of organizational form on the strategy making process (e.g., entrepreneurial, 1982a and adhocracy, 1985b; also professional bureaucracy, [1983d], in a conceptual article with Cynthia Hardy, Ann Langley, and Janet Rose; see especially 1983d for a first attempt by Jim and myself to draw these together, and my latest book [1989a] in which I added descriptions of the strategy making process to the chapters on each of the configurations).

I published a variety of other articles in what I am calling this second phase of my career. A number were spun off of my two books (1979c, 1980a, 1981a,b, 1982b, 1983d, 1984c, 1985a), including a few from a last section of the power book (entitled "Who Should Control the Corporation?") that took me into the new realm of broader social issues (1983g,h; 1984d). A major piece entitled "Beyond Implementation: An Analysis of the Resistance to Policy Analysis" (1979c), based on a Chapter 8 that I finally wrote in 1978 (52 pages), was produced for an operations research conference.

I also published two articles on research methodology. One, entitled "If You're Not Serving Bill and Barbara, then You're Not Serving Leadership" (1982c), was a diatribe of sorts, done in my role as commentator for a conference on leadership research. Realizing how dismal was most of that research, I argued for the getting rid of definitions, measurements, instruments, variables, etc. not for shock effect but because I really believed that they didn't serve a phenomenon as fuzzy as leadership style. Bill is a close personal friend (who edited both my thesis and my latest book), and Barbara was his colleague in marketing at the National Film Board, both the kind of intelligent practitioners who, I argued, should be used as gatekeepers to decide who gets funded and published in the field of leadership research. Their comments on the papers at the conference, which I included in my own paper, merit reading in and of themselves.

The other article, "An Emerging Strategy of Direct Research" (1979b), outlined the approach that has characterized my own research - its descriptive and inductive nature, its use of simple ("inelegant") methodologies, the presence of a systematic focus (always addressing a clear issue if never testing a hypothesis), concern with synthesis (particularly around the notion of configuration), the need to measure, where appropriate, in real organizational terms (e.g., the pattern of store openings as opposed to "amount of control" on some perceptual 7-point scale), and always supporting systematic data by others of a richer, anecdotal nature in order to explain and

not just describe what has been found (and so to theorize). Were I to add one more prescription today, it would be to cherish anomalies. Time and again, as I worked with dozens or hundreds of little notes all over the place, it was my inclination to those I could not explain, and to return to them periodically, that made all the difference. I suspect that weak theorists tend to dismiss the anomalies, while others succeed because they don't let go of them until they are explained.⁶

Finally, I published two articles in the *Harvard Business Review* during this phase of my work, each of which made a big difference in its own way. The first, "The Manager's Job: Folklore and Fact" (1975c), published while I was in Aix, summarized the conclusions of my thesis for a wide practitioner audience. That it reached, with a bit of vengeance, although one additional year in Aix shielded me from its full influence.

The other, published exactly one year later (1976b), had quite a different effect, on me personally. I am not particularly prone to "knocking off" an article - getting an idea, writing it up quickly, and sending it off. But on a quiet farm in the summer of 1975 in the Perigord region of France, where we were subsequently to spend many months of August, I read Robert Ornstein's (1975) book, *The Psychology of Consciousness*, about the consequences of Roger Sperry's research on the two hemispheres of the brain. I had a sense of revelation - it seemed to explain so much of what I had found in my own research, including those two kinds of knowing things, managerial work as "calculated chaos," the realization in studying strategic decision making that everything that seemed to matter (such as diagnosis and design) remained a great mystery while whatever didn't (such as the evaluation of alternatives) was crystal clear. Whether or not the physiology was correct - and that debate continues vigorously, but to my mind less a question of scientific validity than of scientists' propensities to draw inferences - to me there was clearly a critical message here.

⁶ Besides anomalies can be fun. Some people collect stamps; I collect typographical errors. I write badly, and so my secretaries over the years have obliged with wonderful ones: "statistics quo," "Karl Propper," "consultants tend to come in times of charge," and, best of all, "diversification" (what a difference an "a" makes). I use these almost subliminally in my executive programs.

The title came first - "Planning on the Left Side and Managing on the Right" - and then I wrote the article, rather quickly. I sent it to the *Harvard Business Review*, which accepted it, and in March 1976 I sent a copy to Herbert Simon. He replied soon after, commenting in his letter that "I believe the left-right distinction is important, but not (a) that Ornstein has described it correctly, or (b) that it has anything to do with the distinction between planning and managing or conscious-unconscious"; he referred to it as "the latest of a long series of fads."⁷ A day or two later, as I recall, the *Harvard Business Review* wired me to France that they needed the final draft immediately.

Herbert Simon was to me not just the most eminent management theorist of our time but one with no close equal. He had been devoting the later part of his career to intensive research on issues of human cognition, in the psychology laboratory. And here I was about to go into print in direct contradiction to his conclusions, based on the casual reading of a popular book he referred to as a "fad"! Did Simon know something I didn't, or was there some kind of block in his thinking? My heart battled with my head (or was it my right hemisphere with my left?), over whether hearts sometimes know more than heads, and after an agonizing day or two, the "right side" won.⁸

⁷ This and subsequently referenced correspondence with Simon has been reprinted with his permission in my latest book (1989a:58-61).

⁸ I must admit to something a bit sneaky here. As Ornstein points out, there is a good deal of symbolism associated with right and left, the former correct, strong, straight (in French), masculine, etc., the latter "gauche" (which means left in French), "sinister" (left in Italian and Latin), mysterious, feminine, etc. Of course, these refer to the right and left sides of the body, which are controlled by the opposite hemispheres of the human brain. By titling the article "Planning on the Left Side and Managing on the Right," with references to hemispheres instead of arms, I was turning the effect of that symbolism to my advantage.

I had a comment in the original paper that both I and the *Review* editors deleted independently in the final version as too controversial: "I am tempted to raise the issue of extra-sensory perception here. There is clearly too much evidence to dismiss this as a medium of communication, at least for some people, and as Ornstein suggests, it is presumably a right hemisphere activity." Simon had picked up on this in his letter. "The temptations are so great to romanticize about human performance (and even to credit it with ESP for which there is no evidence)!" My decision turned on rereading that sentence. I am certainly no mystic, not even a numerologist (William McKelvey's worries in *Organizational Systematics: Taxonomy, Evolution, Classification* [University of California Press, (McKelvey's [1982] about my playing with the number five notwithstanding), and I have as much trouble entertaining the notion of precognition as anyone else. But for Simon to dismiss the possibility that we pick up information in as yet unspecified ways - for example, when we "read" someone's eyes or "feel" tension in a room - struck me as a blockage. And so I decided to go with my inner "sense" instead of Herbert Simon's learned knowledge.⁹

I believe our lives are determined in large part by the occasional choice that later proves to have been a turning point. In other words, we don't get to choose critically very often, and we can, in fact, hedge and stall and do all kinds of dumb things day in and day out, but every once in a while we had better get it right. And getting it right at those times usually seems to mean listening to that inner voice, which goes by the name of "intuition," not to the babble of the social world or the logic of formal analysis.

My intuitive decision to opt for intuition subsequently opened up my work to that concept and myself to that process. It was as if I had been climbing up to a knife edge of analysis-intuition ever since I joined the operational research group at the Canadian National. The Simon letter put me right on that edge, and the decision I took began my journey down the other side.

Subsequently, my attention turned increasingly to the softer notions in management, and I broke increasingly with the long dominant rationalist view, perhaps the one real paradigm in the field, represented especially in the work of Simon himself (who, in recent years, has come to define intuition as "analyses frozen into habit", e.g., Simon, 1987). Today I am inclined to compare a "cerebral" with an "insightful" approach to management, one based on words and numbers (in academia, Harvard's words or Stanford's numbers, both equally cerebral), the other on images and "feel" (as in strategic "vision" or being "in touch"). In a recent paper on decision making discussed in the next phase, I wrote a section that characterizes Simon's "bounded rationality" as really a "cerebral rationality" because it slights people's ability to perform great feats of synthesis (such as Simon's

⁹ In a letter of reply to Simon, I quoted Turing (19_), the great British mathematician, who wrote in his famous article (before computers) on why it should not be assumed that machines cannot think:

I assume that the reader is familiar with the idea of extrasensory perception, and the meaning of the four items of it, viz., telepathy, clairvoyance, precognition and psychokinesis. These disturbing phenomena seem to deny all our usual scientific ideas. How we should like to discredit them! Unfortunately the statistical evidence, at least for telepathy, is overwhelming. It is very difficult to rearrange one's ideas so as to fit these new facts in... This argument is to my mind quite a strong one. One can say in reply that many scientific theories seem to remain workable in practice, in spite of clashing with ESP; that in fact one can get along very nicely if one forgets about it. This is rather cold comfort, and one fears that thinking is just the kind of phenomenon where ESP may be especially relevant.

[1957] own writing of *Administrative Behavior*).

My debate with Simon continued with some sporadic correspondence as well as a critical review in 1977(b) of his revised *New Science of Management Decision*. This associated excessive rationality with the excesses of the Vietnam war, especially as reflected in the "professional management" of Robert McNamara. It concluded by juxtaposing Simon's claim that "We now know a good deal about what goes on in the human head when a person is exercising judgement or having an intuition, to the point where many of these processes can be simulated on a computer" (Simon, 1977:81) with Sperry's conclusion that "The right [hemisphere], by contrast [with the left] is spacial, and performs with a synthetic space-perceptual and mechanical kind of information processing and yet simulateable on computers" (1974:30). Simon was awarded the Nobel Prize in economics in 1978; Sperry won his in physiology in 1981!

So what am I to make of this second phase of my career. This was certainly a time of loosening up and of opening up, a time of shift from the rather analytic to (what I like to think of as) more balance between the analytic and the intuitive, certainly a time in which synthesis more vigorously entered my work and thinking. I would not label my realized strategy of this period as emergent, but surely it had become less formally deliberate. One might describe it, using our own labels (1985a), as umbrella in nature, guided by the notion of configuration, with, of course, that old textbook outline, now compromised by the size of its "chapters," still providing one sense of direction. I also started to become more playful in this period, both literally in my private life and figuratively in my work. One need only look back at those blob forms in Figures 7 and 8!

Circle Phase: Playing "LEGO"- Dynamics (from 1979/1987)

Alain Noël joined our doctoral program in September 1978, and about five months later, after having read my work on structure and power, asked me a question that was to change my thinking a second time: "Do you mean to play jigsaw puzzle or LEGO with the elements of organizations?"

I had to reply that I guess it was jigsaw puzzle¹⁰, at least for my readers: I was asking them to select known images of their organizations, to put the elements of structure together in one of five predetermined ways. But Alain's question so intrigued me that I soon opened a file called "LEGO," to collect examples of all those weird and wonderful organizations that refused to fit into one or other of my pat categories. It was not that I couldn't find examples of ones that did - I knew from my own experiences, and those of others, that many effective organizations conformed remarkably well. (I later began to ask the McGill MBA students to record their perception of fit with their field organizations: in just over half the cases recorded - 66 out of 123 - the students felt a single form fitted best [see 1989a:259-260, 266].) But some of the most effective organizations, and certainly many of

¹⁰A passage in the Preface to *The Structuring of Organizations* reads:

In retrospect, I felt I had been working on a giant jigsaw puzzle, with many missing pieces. Some of the pieces I had seemed to fit in obvious places, and once enough of them were placed, an image began to appear in my mind. Thereafter, each new piece in place clarified that image. By the time I finished, I felt I had found a logical place for all the pieces available to me. (1979a: xii)

the most interesting, did not. For example, there were seemingly bureaucratic machines that managed to innovate when they had to (McDonald's? IBM?), and what seemed like adhocracies that had rather tight control systems (Hewlett-Packard? 3M?). These became of increasing interest to me.

I show the third phase of my work as beginning on a diagonal line from 1979, when Alain first posed that question, to 1987, when I began work on a serious answer (published in *Mintzberg on Management [1989]*) as "Forces and Forms in Effective Organizations"; see preferably the 1991[a] version). Here I did not so much dismiss the five forms as covert than to a set of five forces, arranged around the nodes of a pentagon, each drawing the organization in a different direction. In the middle, I added from my power book the two forces of ideology and politics (the former of "cooperation," pulling together, and the latter, "competition," pulling apart) which I described as catalytic (centripetal and centrifugal). Altogether, shown in Figure 9, this constituted what I have found to be a most useful framework by which to diagnose the problems of organizational design.

In 1980, I was invited to do a "top management briefing" for the Management Centre Europe (MCE) - two days, me alone, with senior European executives. I guess I always subscribed to Jim Water's guide of tying a rope to a rock, the other end to your ankle, and then throwing the rock over some attractive cliff. It's called commitment. But when the time came, I was petrified, more worried than I had been for any other working engagement. I slept not a wink the night before, and making it through the first day was somewhat of a miracle. I recall in the afternoon sitting up near the ceiling of the room listening to me talking down below (a phenomenon I have heard described by others), and praying that guy wouldn't make any mistakes. Fortunately he didn't, and after a good night's sleep, and a successful second day, with all of me together on the ground, I have been doing MCE briefings ever since. By 1985, I was doing two per year, and in 1988 I began to do one regularly in Canada as well. These programs are exhausting and I limit the number, but they have come to represent the core of my thinking, focusing my writing, research, and general mental set much as did the MBA policy course when I began teaching at McGill. They force me to ask questions of relevance: how do all the concepts I teach help practising managers to deal with serious problems.

These briefings have thus become part of a shift in my mind from the push of concepts to the pull of issues. Push promotes some idea, technique, or angle, whether transaction cost in the university or strategic planning in the corporation. It reflects the rule of the tool, that given a hammer everything begins to look like a nail. Pull begins with a problem or issue, and then draws on whatever it takes to deal with it, sometimes including concepts and techniques. Business schools, especially in America, have become prisoners of push, to their great discredit. So has much of American business practice, given its obsession with technique driven by that almighty "bottom line." I believe it is time for pull.¹¹

My shift from push to pull should not be interpreted as a change to a prescriptive, applied orientation; indeed, sometimes the result has been exactly the opposite. Having to address the concerns of thoughtful

¹¹ This year I even redesigned my doctoral seminar in policy around pull. Whereas I used to hand out a set of readings each week on some aspect of organizations (not unexpectedly, strategy, structure, managerial work, etc.), to be discussed in class, this time the students and I identified a series of issues - organizing across borders and boundaries, managing professional institutions, effecting strategic change, etc. - which they introduce and we all discuss each week, backed up by some related readings to stimulate understanding.

managers (as Europeans in general and MCE attendees in particular, tend to be) can be a helpful experience for even the most theoretical academic. To take a not incidental case in point, my description of managerial roles in *The Nature of Managerial Work* (1973a) has never worked in the MCE briefings. In an effort to provide a framework by which my materials on strategy and structure (which do work) can be assessed in the attendees' own jobs, I have, therefore, had to rethink the roles.

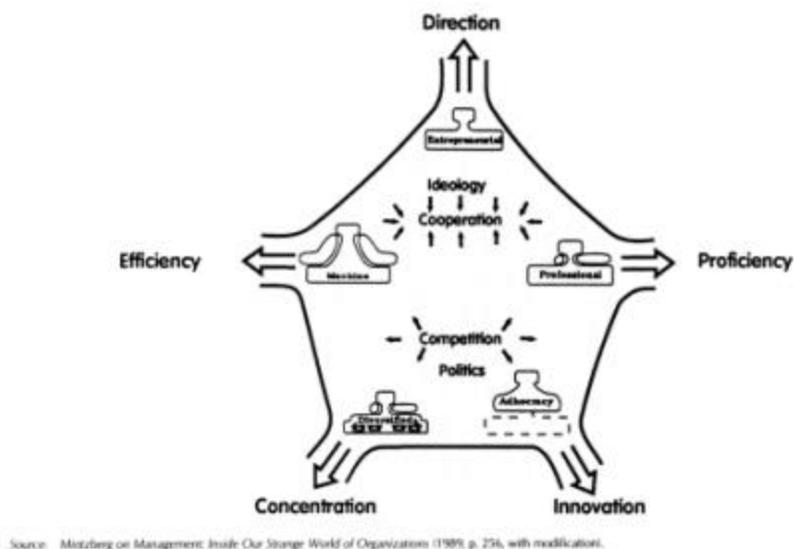


Figure 9. Framework to Play "Organizational LEGO"

In 1985, I served as the faculty representative on a McGill university-wide committee on budgeting problems. I proposed - seriously as an idea but with no expectation of implementation - that the university could eliminate its deficit if it paid professors for what they really did. McGill remains a very collegial and responsible institution, a place with a real sense of its own excellence despite a battering by a sometimes hostile milieu, but like any other, it has its share of faculty who have retired from research but not from drawing full salary. Since I consider research part of professors' duties - in other words, we are paid full time to work more or less full time - people who have stopped doing research should be receiving something like sixty percent of their salaries.

It soon occurred to me that I should really do just that myself, with a slightly different twist. I was becoming increasingly disillusioned with conventional management education (see 1989a: Chapter 5). It just did not make sense to me anymore, socially or economically, to continue the pretense of training barely experienced hotshots to be managers. The MCE experience had convinced me that management education should be reserved for people who know how organizations work, whose experience is deep as well as tacit, and whose place in the classroom is determined by their accomplishments rather than their aspirations.

So I decided to stop teaching the MBA policy course, proposing to the McGill administration that I go on reduced load and salary and concentrate my university work on writing, research, and doctoral supervision and the biannual course. In other words, I would divide my labor between scholarship in the university and application (including pedagogy) in the field. Long ago, *Fortune* magazine published an article entitled "The MBA - the Man,

the Myth, and the Method" (Zalaznick, 1969) that concluded that the real contribution of the American business schools lay not in its graduates from teaching so much as its insights from research. I always thought that to be true - now I was putting it into practice myself.

The arrangement has worked out marvellously well. All at once, I relieved myself of significant pressure (from doing what felt wrong even more than from trying to do too much), clarified my efforts, and ironically, significantly increased the time I had for what I like to do best, namely scholarship. McGill has a rule of a maximum of four days of consulting per month for full-time faculty; even now, on reduced load, I do not yet reach it. Much of the time that went into MBA teaching now goes into my writing and research.

Outside the university, I do those few two-day programs, a number of one-day public conferences in Europe that I am directing more toward what I like to call "discovery conferences" (where a panel of us from theory and practice addresses some pull issue in a mostly unrehearsed way, for example, the management of strategic change), an occasional in-house workshop in the form of executive retreats, etc., as well as a certain amount of consulting, which tends to take on a wide variety of interesting forms. Recently, for example, I helped a couple of McKinsey consultants rethink the structure of a large South American firm and decided with people at the Brookhaven Laboratories in New York whether or not nuclear power plants are best thought of as machine bureaucracies (concluding yes and no, depending on whether one chose to play jig-saw puzzle or LEGO [see 1989a:267-268]).

These latter experiences offered me wonderful opportunities to discover issues and to root my theoretical concerns in the realities of messy practice. I do not pretend that they constitute research. (I have always considered so-called "action research" to be an attempt by some academics to have their research cake and eat the consulting fees too.) But I do find them invaluable for drawing me toward issues that seem to be significant, for exposure that frames reality in my head, and even for enhancing the scholarship in my work. Direct exposure has always been my best stimulus; lately I have been able to request tours of a fascinating variety of physical facilities, through which I also meet operating people. That has made an enormous difference - not only for the consulting work itself but also for my own development of conceptual material. At Brookhaven, for example, I asked to tour a nuclear power plant, and what I saw in just one morning made all the difference to my conclusions. Similarly, a friend in London took me out to a hospital recently, and after listening to the National Health Service regional manager talk about negotiating a new facility through the politics of the bureaucracy, followed by a tour by the head nurse who revealed her wonderful intimacy of the operations, I came home talking about "managing up" and "managing down." Where those meet - in fact, whether they meet - strikes me as a critical issue in the practice of management today. I feel privileged to have this kind of varied access to organizations at this stage in my career.

I explain all this before discussing my publications of this third phase because the order of presentation reflects the phase itself. These are the things that have increasingly begun to drive my work in the past several years. As befits more pull and closer connection to practice, my writings have also become more disparate; hence the "illusive strategy." But I should add here that when we drew conclusions about "Strategy Formation in an Adhocracy" (1985b) in our own research, which we found to have oscillated between periods of convergence and

divergence (typically about six years of concentration on particular product/market themes, followed by about equal periods of less focused experimentation, of "riding off in all directions,"), it turned out that the organization was sometimes most effective during the periods of divergence. In other words, focused strategy is not necessarily a prerequisite to success, at least not perpetually in some kinds of organizations (which also means, I guess, that my own behavior has shifted from that of professional bureaucracy toward adhocracy).

To make the link between my practice and my writing, I should perhaps begin with my most applied recent efforts, two books in particular. As noted earlier, I have always sought to produce a stream of work aimed at practitioners. Some of this has been easily accessible to them - articles in the *Harvard Business Review*, for example - while other material was not. And so I accepted an invitation from Bob Wallace at the Free Press to do a kind of practitioner compendium of my work. It was initially meant to be a packaging job of existing materials, but (surprise!) I ended up writing and rewriting a good part of it. The book contains some new materials (including some of the Simon exchange and the forces and forms paper discussed above, as well as other two pieces to be discussed below), and it presents the configurations renamed (as in the pentagon in Figure 9) and rewritten to encompass the dimensions of managerial work and strategy formation. The publisher calls it *Mintzberg on Management*; I prefer the subtitle *Inside Our Strange World of Organizations* (1989a). The book draws together almost all of the themes in my work over the years, such that it is probably the closest thing in print to my original textbook, though not so organized, and with a particular slant (that I shall discuss below). It is aimed at the practising manager, but also, I had hoped, a far broader audience. As I noted in the introduction, in today's world of organizations, "pop organization theory" should be as important to the general public as pop psychology.

As the for second book, James Brian Quinn of the Tuck School at Dartmouth approached me a few years ago to collaborate on a textbook. He had developed a series of excellent cases on strategic management, and proposed that I do the text. I agreed, but not in the usual way. Rather than writing it, I suggested we design it by pulling together the best work of other authors in the field, carefully edited for conciseness, interspersed with our own published writings. I just could not face the thought of doing a conventional "gather-around-children-while-we-tell-you-about-strategic-management" textbook. Why should intelligent students not be exposed to what intelligent writers had to say in their own words. *The Strategy Process* was published in 1988(a), also co-authored with Bob James, and the second edition has just appeared in 1991(a).

There is, of course, a lovely irony here. Finally given the chance to do a real policy textbook after so many years of struggling with one, indeed with most of the chapters already written and awaiting publication, I casually turned my back on it!

Turning to my work of a more scholarly nature, many of my publications before the late 1980s were in fact manifestations of the previous phase, especially the coming to fruition of our studies of strategy formation (already listed), followed by an article called "Crafting Strategy" [1987a] to draw out the broad conclusions. More recently, my pace of writing on this subject has not slowed down so much as shifted (not fully reflected in the Figure 1 histogram, due to the size and publication lag of some of this work), meaning that I continue to pursue work of the first stage too.

I took another sabbatical, this time to a tiny village in an obscure valley in the Swiss Jura mountains,

during the first six months of 1984. I dragged along what amounted to many boxes of the old Chapter 7 on strategy making, and spent all that time reading and organizing it. (The analyst in me still likes to keep score, so I recorded somewhere that I read 1,495 items - I calculated it would have stood fourteen feet high.) It was all intended to write the chapter that this time I knew would be a book. Well, I was wrong again. It may be recalled that my first book, on managerial work, was 298 pages long, my second, on structuring was 512 pages, and my third, on power, exactly 700. One need not even be the statistical jock I am to conclude that the next book in the "Theory of Management Policy Series" has to be about 900 pages long. In fact, the outline I brought home from Switzerland divided that book into two volumes, the first called *Strategy Formation: Schools of Thought*, intended to review the literature, and the second, *Strategy Formation: Towards a General Theory*, to extend the configuration notion to strategic stages across the evolution of organizations.

A number of other subsequent commitments, as well as an increasing propensity to go with ideas that come up (to be detailed below), have slowed this work down somewhat, or at least its publication, because I have in fact already produced a sizable quantity of material for the first volume. Chapter 1 on the concept of strategy has been written, and published as two articles (1987 b,c). The rest of the volume is devoted to the schools, ten in number. A lengthy chapter exists on the first, design school, much of it published in a recent article (1990c), and I finished in 1985 a 358 page "chapter" on the planning school, which I am now revising for book publication. I also began work several years ago on the "positioning school" chapter, which exists as a long, untyped manuscript, one part of which has appeared as "Generic Strategies: Toward a Comprehensive Framework" (1988b, also in shortened form in 1991a). I might add that in doing this chapter, I was having a wonderful time playing Michael Porter: my own work may hardly be loaded with nuance and affect, but as a consummate categorizer, I was revelling in developing the rather purely logical typologies of strategy content. The chapters on the other schools - entrepreneurial, cognitive, learning, political, cultural, environmental, and configurational - remain unwritten, although I did publish a very long paper titled "Strategy Formation: Schools of Thought" (1990), that summarized them all.

Since this is my final comment on "The Theory of Management Policy," I thought it might be helpful to include an accounting of where that book now stands, which I do in Exhibit 1.¹² So near and yet so far.

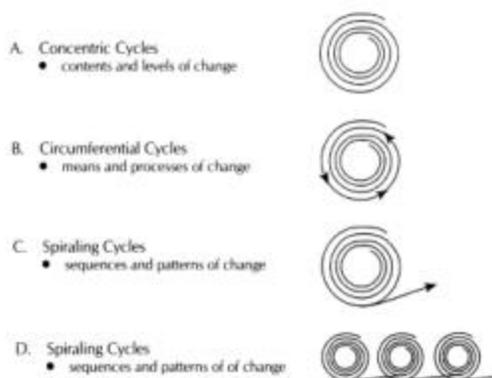
¹² The latest materials I could find in the files still reflecting a real textbook, are the bound chapters dated 1980-81 and an outline dated November 1981. The former shows Chapters 1,2,3 (now on structure), 5,6, and 8 as written, while 4 (now on managerial work) and 7 list published materials. The later outline shows efforts to incorporate the notion of configuration, with one new chapter on "Elements of Situation," another on "Configurations" themselves, and seven at the end on each configuration as well as a final one on "Transitions." I also found a document dated May 21, 1980, called "Management: Another View," that outlined a trade version of the book which was more integrated around the configurations and listed a "LEGO" chapter. Another scribbled note read "TT writing in 1976-77: concentrate on integrating the chapters." Sometime later I had crossed out the two decade 7s and replaced them with 8s, and sometime after that (in this case at least whimsically) I had crossed out the 8s and put in 9s!

Exhibit 1. Current Status of "The Theory of Management Policy"

Chapter	Title	State
1	The Field of Management Policy	Chapter (1974, 1978) and two articles
2	An Underlying Theory for Management Policy	Chapter (1974, 1978)
3	The Structuring of Organizations	Book, several articles, and chapter (1980)
4	The Nature of Managerial Work	Book and several articles
5	Power In and Around Organizations	Book and several articles (early chapter [1970] superceded)
6	The Making of Strategic Decisions	Chapter (1973) and article
7	The Formation of Strategy	Two volume outline, of which several articles and chapters, one as book manuscript (early chapter [1970] superceded)
8	The Role of the Policy Analyst	Chapter (1978) and article, also book manuscript on planning
9 or 16	Toward Effectiveness in Organizations	Article (forces and forms)
9-15	Possible chapters on seven configurations: L. Entrepreneurial, Machine, Diversified, Professional, Adhocracy, Ideology and the Missionary Organization, Politics and the Political Organization	Chapters in other books

Alongside the volumes on strategy formation has been my research work in that area with Frances Westley, who joined the McGill faculty in 1983. Frances is one of those rare people who came out of the push of a strong discipline and slipped ever so naturally into the pull of the management field. As a result we have developed a wonderful collaboration, of her sociological affect with my engineering effect, by which we have worked out some tricky conceptual issues with great glee. (Having made a habit of doing so just before conference presentations, we have decided that our real talent lies in "just-in-time theorizing"!)

We published two articles on strategic vision (1988b,c), and after many go-arounds are now preparing to publish another on a general model of strategic change. As shown in Figure 10, it is conceived in terms of circles and cycles, one representing the concentric levels of change, a second the means of change, a third the episodes of change, and a fourth, the evolving cycles of change.



Source: "Cycles of Organizational Change" (1993, p. 41).

Figure 10. Overall Cycles of Organizational Change

One characteristic of this third phase of my work, as already noted, is my greater willingness to go with ideas as they come up, much as did our adhococracy, the National Film Board of Canada, in its periods of divergence.

This has resulted in a number of "one-off" papers and articles, mostly co-authored, one with Joe Lampel (ex of our doctoral program) on customizing strategies (in draft), another with Maria Gonzalez (ex of our MBA program) on strategies for financial services (1991d), based on a consulting assignment, a third with Angela Dumas of the London Business School on managing design (1989c).¹³

Two major activities of late have included revisiting work I did initially in the first phase of my career. I have already mentioned my return to the managerial roles because of the difficulties of using them in my MCE program. Moreover, it occurred to me, based on my own notion of emergent strategy and Karl Weick's (1979) concept of "sensemaking," that the order in which I had presented the roles (interpersonal providing information enabling decision making, as shown in Figure 6) could just as easily have been reversed; in other words, I had a list, not a theory. An invitation to speak at a celebration of the eightieth birthday of Sune Carlson, author of the classic study of managerial work titled *Executive Behavior: A Study of the Work Load and the Working Methods of Managing Directors* (1951), sent me back to this literature (specifically to my two unopened boxes of Chapter 4). There I realized that, at best, all the other descriptions were no more than lists themselves (see 1991c). So I set out to develop something better, and last fall in London, across a series of seminars (rocks going over cliffs, each preceded by some JITT), it all came together, quite literally so in a framework of five concentric circles. [Figure 11 shows the framework as finalized in 1993.] Once again the work has been conceived but not yet written up.

I think there is something to the fact that this figure (developed in considerably more detail) preceded the outline. What matters in developing theory about managerial work, in my opinion, is not so much the fully articulated text as the comprehensive representation of the model. People need to see the various dimensions that appear to constitute managerial work all in one place. That way, they can begin to discuss the job of

¹³ I have long been fascinated with the subject of design, not just the concept in my professional work but the aesthetics in my personal reading - or preferably, looking at the pictures. In engineering school, in fact, I harbored the idea for a time of becoming a packaging consultant.

managing comprehensively and interactively. I have found this to be true as I got to use the model to develop the theory, and when I drew the diagram for Bill at dinner one evening, he immediately began using it to diagnose the problems of managing in his own firm.

For different reasons, I have for some years now wished to revisit the subject of strategic decision making. The tendency to show it as the linear unfolding of sequences of steps, in our own work (1976a) no less than that of others, somehow seemed to miss something important. This subject (in effect, the old Chapter 6) was not, however, on my agenda, especially given my heavy commitment to strategy formation. But with the new spirit of one-off papers, combined with a pedagogical idea I had been considering for awhile, I made room for it.

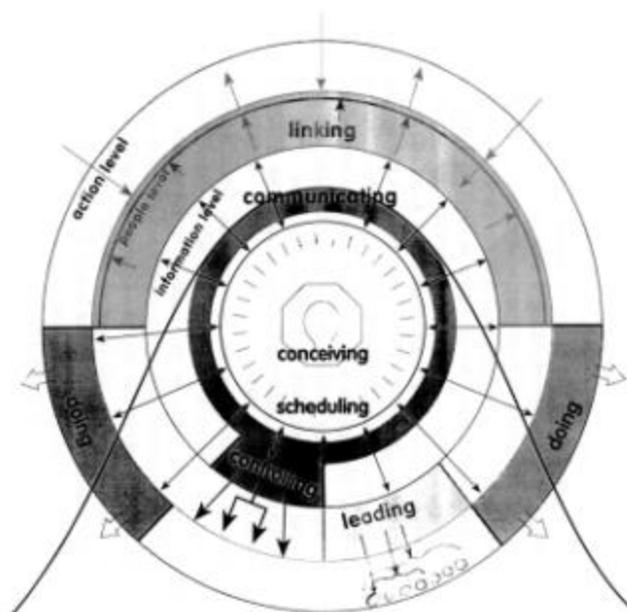


Figure 11. Framework For Describing Managerial Work

Doctoral study is really an apprenticeship. Courses are fine, as far as they go, and research assistantships can be useful, but they often subordinate the students' interests to those of the professor. So I developed the idea of a "research cell," somewhere between the two, in which a small team of selected faculty members and doctoral students could address an issue together. Five of us, including one other colleague and three doctoral students (from all four Montreal universities, as it happens), took on the issue of "opening up decision making," seeking to reconceive what we perceived to be this narrow line of research. A most exciting paper has resulted (submitted for publication), which opens up the concept of decision itself to the ambiguities that surround the moment of choice, opens up the decision maker to history and experience, affect, insight, and inspiration, and opens up decision making to a host of dynamic linkages such that isolated processes come to be seen as networks of issues.

Finally, there is the side of my current work I call the polemics, which address some broader social issues. Two papers were published as chapters in my last book, *Mintzberg on Management* (1989). One, called "Training Managers, Not MBAs," explains my dissatisfaction with conventional management education. And the other,

"Society Has Become Unmanageable as a Result of Management," expresses my concerns about the prevailing practices of management. These chapters give the book a somewhat caustic flavor, but not, to my mind, one unwarranted in today's world of bottom-line banality. Still, I felt the need to back off after these, and so the work I continue to do on broader social issues is perhaps more provocative than polemic. Last fall, for example, invited to give a speech to a large management congress in Prague on "East Meets West," I used the occasion to do some reading and thinking on the issues facing eastern Europe. The resulting paper (just submitted for publication) emphasizes the need for grass roots strategic learning rather than formal planning in times of difficult change, and suggests that it was not capitalism that "triumphed" in the west so much as balance in our economic system, a balance that ironically, risks being upset by these very changes.

I like what I have been doing recently. I like the style of life I have established for myself and I like the variety of work I can do, so long as it remains under the umbrella of the themes that have always rooted my work - to understand management and organizations, especially with regard to strategy (setting direction) and structure (establishing state and process). Everything seems to fit together very nicely. So I have no intentions of making any changes now; I foresee no fourth stage in the near future (but neither did I earlier foresee a second or a third). I hope to consider the issue of intuition more carefully, especially to develop the idea of an "insightful" face of management in contrast to its long dominant "cerebral" face. I wish to consider via direct exposure, certain of the trickier organizational contexts, notably the delivery of public professional services (especially health care), the management of the softer sides of government, the organization of corporations across businesses and boundaries, and the long standing problem of rendering adaptive our bureaucratic machines. I intend to redo *Structure in Fives* as "Structure in Sevens," so that I can play more elaborate LEGO; to do another research cell, on organizational effectiveness; and, of course, to keep plugging away at "Chapter 7" on strategy formation. So where is that illusive strategy? Not so illusive, I conclude, unless one insists on the kind of deliberate plan that drove my earliest work. As my activities have grown more varied, I believe that the overall thrust has become more integrated. It all comes together around the themes that have been struggling to get out these past twenty-five years: thinking, designing, acting, and learning to achieve more effective and human organizations. Organizations do this, biased perhaps toward that order, while I do it perhaps with a growing bias toward the opposite order. Gradually I have gotten closer to organizations while always maintaining my distance, being able to consider their impact more broadly while probing more narrowly into their details. Overall, I have been searching for their deceptive effectiveness, first through study of their elements, subsequently combined to describe their forms, and then exposed to reveal their dynamics, all the while attuned to their dark recesses of intuition hidden amidst the brilliance of their formal analysis. Cycling has characterized my own behavior as well as the actual theory I have been developing recently, as I have come to see organizations in increasingly dynamic terms. One need only look back at the circling and cycling diagrams I have been doing in recent years, illustrated in Figures 9, 10, and 11! Thus this third stage of my work can be depicted as follows:



"We shall not cease from exploration," wrote T.S. Eliot in one of my favorite quotes, "and the end of all our exploring will be to arrive where we started and know the place for the first time."

This ends my story. When Art Bedeian informed me, rather casually, of the page limit, I thought to myself, who in the world would ever write that much about himself. Now look what I've done. And me who started with the admonition about not taking yourself too seriously. Could we, perhaps, just attribute this to me taking my work very seriously?

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