

Manufacturing strategy: The story of its evolution[☆]

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Abstract

This essay has two stories to tell: first, as promised, the evolution of what is known as “manufacturing strategy” and, also, the parallel story of the value of combining teaching and research through the Harvard Business School’s focus on teaching by the case method. This second tale may be of particular interest in view of recent critiques of business schools and their research practices [Bennis, W.G., O’Toole, J., 2005. How business schools lost their way. *Harvard Business Review* 83 (5), 96–104].

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1. From private university to private U.S. Army to private industry

My college years at Yale during World War II were non-stop: universities were teaching year-round. As soon as I had my degree in chemical engineering, I volunteered for immediate induction into the U.S. Army and right away became an infantry private. Days before being shipped to Europe, I was transferred to the Engineer Corps for duty on the Manhattan Project at Los Alamos, New Mexico. I learned some industrial engineering and became fascinated with the valuable insights, which came from measuring and analyzing material and manpower flows. As soon as I left the Army, I headed for the Harvard Business School. In 1946 and 1947, I experienced exciting and demanding analytic breakthroughs three times a day, 5 days a week. In concert with

my classmates in the case method, I was consistently wrong in my pre-class analyses and this was humbling. But the power of in-depth, hard-worked analysis eventually produced confidence and, often, contrarian insights. Contrarian! That role felt somehow natural.

The next 10 years at Honeywell provided tough bosses and unambiguous jobs in production (6 years), marketing and sales (2 years) and 2 years as a divisional finance officer. Honeywell also trained me in the process of leading management development classes which reminded me of my admiration for my Harvard MBA instructors. I became fascinated with why organizations worked and, when they didn’t, what to do about it. In 1958, I resigned from Honeywell and entered the Harvard Business School’s doctoral program.

2. From doctoral student to junior member of the faculty

In my first year, in addition to doctoral coursework and exams, case writing took me into 15 companies in 12 industries. My thesis focused on U.S. companies manufacturing abroad. This was followed by an

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invitation to join the HBS faculty in the Production Area.

I was immediately teaching full-time but the signals from senior colleagues were clear: I should be “doing some research” and reporting on it in a faculty seminar. Since I had been away from academia for a decade, I decided that I’d better catch up on what I’d been missing. Harvard’s Baker Library had everything I needed. In October 1960, a doctoral student – Henry B. Eyring – asked me what I was doing so I explained. Henry then kindly asked:

“Why don’t you not only study these new concepts and techniques but at the same time from your Honeywell experience and recent case writing in industry come up with a sense of what is happening in industry and then see whether the new stuff from academia is now or ever will be useful in solving industrial problems?”

His comment changed everything and set me on a track, which I have never left. I took his advice and what I learned was that we had a substantial mismatch between problems in industry and the then-current research and teaching. Unfortunately, I shared this insight with my Production colleagues at an Area faculty research seminar and was blunt in my diagnosis. What I said is on the record:

“The U.S. production manager is attempting to fight big battles with small weapons, and the ‘small weapons’ are being provided by academics.”

In hindsight I wonder how I could have been so arrogant and untactful—indeed, self-defeating, but I really expected them to be excited and intrigued by my new ideas. They were excited all right—excited like hornets whose nest has just been stepped on by a blundering hiker. They swarmed all over me until the hour grew late and the room emptied.

In retrospect it is surprising that I was so sure I was right but neither humbled nor frightened away from my line of reasoning. I was no gutsy hero: this non-tenured lecturer went to earth with his nascent ideas, while overtly concentrating on teaching and course development. The fact was that I had come across early but undeniable signs of industrial deterioration: the fact that it was management-related made me indignant that colleagues at a professional school of management were wasting their research efforts and teaching academic stuff that seemed really silly. I kept up my own research but went “underground” with it. I did not arouse my colleagues again with heresy of any sort for a full 5 years.

3. Industry and the academy

The problems in American industry were widespread, serious and hard to miss when one got into the factories. In the late 1950s, while academics were still teaching time and motion study and being titillated by simulation, linear programming and algorithms, industry was awash with problems—quality and productivity, labor morale, the growing loss of markets to foreign competitors, equipment and process technology puzzles, to name only a few. Meanwhile, Operations Management professors were so absorbed in intellectually exciting new techniques that they did not see what was happening in our factories. So since no one was asking why things were going wrong, there was no way for managers to know what to do about it.

I think it was an unusual and slippery time in HBS history, largely in Operations Management but also reflecting an uneasiness throughout the School which had its source in an acceleration of the incorporation of quantitative techniques into the entire curriculum as a response to the criticisms of the Ford and Carnegie Foundations’ reports on the quality of business education in the U.S. (Gordon and Howell, 1959; Pierson, 1959) The pressure for change was particularly strong in the Operations Area because many faculty in other areas were concerned that we might be falling behind intellectually. We had been thrown out of executive programs because executives found our content dull and low-level. Colleagues in other areas considered us the weak area at the School. Therefore, several capable new hires had been made from distant doctoral programs devoted to simulation, game theory, etc.: these newcomers were riding high.

After my pummeling at the Area faculty seminar it was obvious that my ideas were politically incorrect. I did manage to get promoted to Associate Professor (a 5-year appointment) probably simply because I was pretty hot in the classroom and a few other “old-fashioned” professors of senior rank backed me. But I was driven by a compulsion to make sense of things and thereby be able to teach and write something important because it was useful. I became a quiet rebel and slunk underground to try to figure out what was really going on in industry and what should be done about it.

4. Working out some ideas

My problem was that I was long on criticism but short on remedies. U.S. industry was failing the country on all counts but no one I could find in industry or the academy seemed to be able to analyze what was wrong

other than the surprisingly rapid surge of new foreign competition driven, it seemed, by cheap labor. Competing successfully with Germany and Japan seemed impossible when their costs were so much lower. I wanted very much to know “why?” and also to know “what should be done?”

To show how smart I was, it took me 8 years to figure out an answer! Let me now describe the process: it’s pretty self-centered, but the rationale for this essay is to explore the processes of intellectual development. My progress was slow: I think I set a record in that department.

At first I was teaching the first-year course: this was an uphill battle because the students found it so low-level and operational that they could not imagine their ever holding such positions. So 191% of my energy went into trying to make my sections in that course educationally exciting in spite of the drab content. My intellectual growth came from taking my ideas out to alumni and industry groups in speeches and receiving very positive receptions. I said, “We’re slipping badly in our industrial prowess and we’d better watch out.” The speeches reflected my thinking at that time. I laid the blame on top management’s lack of focus on the production function, failure to invest in modern equipment, and excluding top production managers from top-management councils. I protested that great marketing and financial manipulation and sharp control systems could not alone make a company competitive. Audiences reacted well because I jolted them. The truth was that very few thinking people were thinking about manufacturing.

Back at HBS no one paid any attention to what I was saying off campus. This was just as well because some smart colleague would have criticized my shallow explanations and I would have been shot down again. I realized that my “answers” to our industrial problems were weak and vacuous so I kept my own counsel.

Fortunately, my next assignment was to teach the second year elective course called “advanced production problems” (APP), a course started in 1946 by John Mclean which had been successful for 17 years but was suffering from out-of-date materials when I took it over in 1964. The conceptual basis of the course was a focus on the production function in one industry at a time.

The idea, a very effective one pedagogically, was to demonstrate that companies in one industry – with one marketplace, one technology, one set of economics, and one external environment – usually differed substantially in their basic manufacturing policies, and their results differed accordingly. Three APP classes were devoted to understanding the technology of an industry:

these sessions were focused on grasping the fundamental physical processes, equipment choices, and the key operating decisions affected by the equipment and process technologies. This included lots of numbers: output per hour, operating costs, capital costs, setup and changeover times, product ranges, specs and skill levels of operators.

The next three classes focused on the industry: competitors, market shares, financial results, norms for costs of goods sold, industry trends, etc. Then, we turned to cases—individual companies in the same industry. This was a set of powerful notions—the industry focus was invented by McClean, then developed, refined and taught over the years by Abe Zaleznik, Jim Bright, David Rodgers and Stanley Miller.

In two fascinating hours in his office, Miller taught me how to teach APP. What you do, he said, is get the class to analyze the operating problem(s) in the case and then back up and examine, first, their implicit manufacturing policies and, then, their competitive issues. This analytic process always demonstrated a conflict between the manufacturing policies and the company’s competitive strategic situation, which conflict produced the operating problems. Pow! Wow! It was dynamite in the classroom! (Incidentally, these conflicts are as present today – typically in 95% of every company I enter or study – as they were in the 1960s.)

In 3 years, I updated all six industries. The course was always over-subscribed and got the highest student ratings. And I changed the course title to Manufacturing Policy.

Curiously, none of my predecessors had ever written up for managers the wisdom offered in this course. I wrote it up as a personal statement for my tenure decision but, like my predecessors, totally missed perceiving the implications for U.S. industry for another 3 years. Now, nearly four decades later, this seems amazing. But something was missing: I wasn’t ready to write to managers about manufacturing policy. I did not latch onto a sense that there was something in the basics of the course, which might apply to restoring the rapidly declining fortunes of American industry.

I did, however, get over my fear of irritating my colleagues and, in spite of not being tenured, decided to go public with my concerns about the U.S. industrial decline. “Production Under Pressure” was published in the *Harvard Business Review* (HBR) Skinner (1966). The article was important because no one else was writing about the decline, which was only just then becoming more apparent. But the message surely needed broadcasting. This article said that we were in trouble externally from foreign competitors and internally from

lack of management attention and investment. I must admit that it was intellectually no starburst and it received little attention from readers. With the exception of one or two senior professors, my HBS colleagues were silent. I am sure that it was because I had failed to probe and dig for fundamental causes and solutions. I cannot fault my colleagues for their indifferent yawns.

To get better ideas it took another 3 years, five more formative experiences, and one really new discovery to integrate all these separate concepts into a single unified theory. Two of these learning experiences were provided by demanding students who insisted on “why’s” and “what to do’s” and forced the teacher to sweat and strain for greater depth, clarity and integrity of ideas. The first set of students were second year MBAs; the second set – several years later – were the advanced management programs (AMP participants), i.e., senior executives.

I taught an elective MBA course Management of Production Operations (MOPO). Its focus, developed by Prof. Arch R. Dooley, was on application and implementation of new industrial technologies, both hardware and software. The hardware consisted of then new equipment such as automated, numerically controlled machine tools, real-time on-line controls, and computer-linked boxes at the workplace, which fed production and inventory data back to a central office. The software were developments such as material requirements planning (MRP), job-shop scheduling systems, Monte Carlo gaming programs work sampling, and simple simulations and models for inventory control and lead-time reduction. All neat stuff, offering managers substantial benefits. The only problem, we discovered, was that no matter what it was, it never worked.

The hardware was usually OK and the software was usually OK. The problem was that the new gear was hobbled by or interfered with existing equipment or systems or habits or skills of the workers, supervisors and staff. So each case was a disaster scene and usually the new investment or change that had looked so promising to the students, as well as to the companies, turned out to be a fiasco. It was enough to make teacher and students – and indeed managers – cautious and gun-shy. “Nothing new ever works” was a quick lesson. “Always let another company go first” was heard around the classroom. This seemed like great wisdom but it was pretty shallow. It would certainly be of little use to managers who needed new technologies for turning around non-competitive factories.

It was not good enough for good students either, so I was forced to dig deeper. At just that moment I was

asked to address the annual meeting of the Numerical Control Society (NCS) of America so I put the ideas from the MOPO course into a speech called “The Stubborn Infrastructure of the Factory”. The notion was pretty simple but it helped me and it helped my students. And the enthusiasm of the conference audience was tremendous! I clearly was onto something useful and maybe its simplicity made it better.

The idea was simply that NC machinery was not catching on and fulfilling its obvious potential because, to work well, it required changes in the production control system, supervision, maintenance, process specifications, job content, wage systems, etc. Any one element of the factory’s infrastructure could bring NC down and render it ineffective. No wonder it was catching on slowly and the NC Society members were frustrated and disappointed. In the MOPO course this was the troubled outcome of every new hardware or software technology: it was shot down or crippled by some extraneous part of the factory’s infrastructure. The lesson learned: infrastructure is as important as new technology and must be designed to fit and support whatever is new.

As I began working on an article broadening the “stubborn infrastructure” concept, I observed that new technology A surfaced another problem X that when solved by new technology B resulted in another problem Y. Nothing was ever right all at once. Why was something nearly always out of kilter?

An AMP class finally furnished the catalytic spark. After being banished for many years, the Production Area had regained a place in the AMPs curriculum. I took it on, on a trial basis, mostly because of the success of the Manufacturing Policy course. We had eight classes. I had promised the Course Head that these classes would be a great success. The heat was on. We could not afford to fail.

The first six classes went well: the AMPs wrestled heartily with cases in which manufacturing failures threatened to bring down whole companies. Dramatic and important, the cases were mostly *Fortune* magazine articles—authentic and in the national view. The AMPs were fascinated: how could such great companies get into such great trouble? I pushed them hard, wondering myself. In the early morning alone in my office before the seventh class, somewhat desperate to understand more myself and have something useful for the class, suddenly it all began to come together.

I saw that the companies had gotten into trouble in manufacturing because experienced production executives had applied their hard-earned wisdom and conventional premises of their profession to reach

fundamental manufacturing policy decisions that were just plain wrong. They did not work. Disasters followed. Consternation. Disbelief.

How could manufacturing managers go wrong by applying conventional premises of industrial management developed and tested and improved over a century? Can the “making of continuous improvements” be wrong? By maximizing productivity? By minimizing excess capacity? By keeping inventories low? By consolidating operations into one big, efficient factory? By mechanizing, automating and computerizing to the utmost?

In fact, all the foregoing were “wrong” in the companies we studied and were being “wrong” in U.S. industry, for they were resulting in plants with structures and infrastructures that were internally non-congruent and, thereby, in dissonance. The equipment may have been chosen for high-volume production, the plant capacity set for low investment, the production control system to handle small lot-sizes, the wage system to minimize turnover, etc. Every system pulled its own way and the plants, run by separate conventional industrial management concepts, were not outstanding at anything, so were not able to compete with foreign imports.

The class saw that the wisdom of industrial engineering, of control and scheduling experts, and of labor economics, did not always work since they could interfere with one another. And they saw that economists and accountants and financial experts pushed production managers toward decisions, which looked like good business but often simply did not meet the company’s strategic realities.

So, with one class to go, we were all on new high ground: smart production people make dumb decisions if those decisions are premised on maximizing (or minimizing) outcomes that are dysfunctional to the firm’s competitive success. We were onto the “why?” of the cases; left to be worked out was “what should be done?”

It was not all so clear. I knew that some in the class were wondering and would raise up the conundrum: “How can any good production manager fail to try to maximize productivity?”

Day 8 dawned. Early, in my office again alone, I asked myself how I would answer if a student asked “So how can top management manage manufacturing so as to prevent these fiascos we’ve been studying?”

How was top management doing it now? They weren’t. They were concentrating on the big, strategic problems of finance and products and markets and marketing. Production was technical, engineering,

routine, standardized, repetitive, lots of people, training, grievances, inspection, inventories. . . none of this was top-management stuff. Everyone was trying to “optimize” their own parts of the puzzle.

Put this all together and the bad results were so bad that they became strategic, setting the company back competitively—in markets, in financial results, in the stock market. And the causes were poor manufacturing policies—those expensive, long-term structural decisions with massive and pervasive on-going consequences.

Pow! Wow! That was it! Top management should manage manufacturing by making sure that those manufacturing policy decisions were right. “Right” would be when the manufacturing function worked. And what made it “work”? It worked when the whole thing fit and supported or created corporate strategy.

But why couldn’t the conventional practices and premises of production management make manufacturing “work”? This question was hanging out there for the first seven classes. What was the central premise? It was that low cost and high productivity are always the key success criteria. But what about delivery reliability, or quality, or short lead times for new product introductions, or minimizing investment—all criteria which had emerged in one or more cases. It was suddenly clear: there were trade-offs!

A given manufacturing system could not perform equally well on all success criteria: someone had to decide. All-purpose plants could no longer succeed anymore: plants had to be designed for a purpose. The purpose? Achieving the company’s competitive strategy.

And that defined “the manufacturing task”: what was it that manufacturing had to be especially good at to make manufacturing a competitive weapon? Now I was ready for the eighth class:

A manufacturing strategy is a set of manufacturing policies designed to maximize performance among trade-offs among success criteria to meet the manufacturing task determined by a corporate strategy. Top management’s job is to ensure that there is a coherent manufacturing strategy in which all manufacturing policies are designed as a whole to support or lead the corporate strategy.

It was a great class for the instructor, and for the students too. They seemed almost as excited as I was. Almost.

Now I had a set of ideas that fit with the facts of current industrial results and began to provide answers to “why” we were slipping. And I could now begin to

answer “what should be done?”: focus all manufacturing policies toward a strategic company objective because a factory cannot excel on every success criterion. That summer was spent in Maine writing the “Missing Link” paper (Skinner, 1969).

5. Getting the ideas into practice

In industry, the “Missing Link” paper caught on like wildfire and, to my great surprise, the phone began to ring and I was soon caught up in the center of “Guru Life”—a flood of speeches, seminars, consulting invitations and lectures on other campuses. The ideas struck some kind of chord because American industry was by then clearly in trouble and managers were ready to try almost anything. Getting them to really understand that they were the problem was more difficult.

The manufacturing in corporate strategy (MCS) approach relentlessly demands whole new premises, insights and skills of industrial managers: these run counter to more than a century of contrary, well-reinforced habits and beliefs. I respected the tools and techniques of industrial management that I was taught – and also taught – but they never seemed as exciting as top management problems. They seemed like elements of housekeeping. My interest was in *architecture* and I came to see that all the *housekeeping* in the world could not make a plant competitive if the architectural design was wrong. Like a house or an airplane, factories had to be designed for a particular use. The worst ones were those that tried to do everything and did nothing well.

Consulting experiences, it turned out, proved to be the most demanding and, therefore, the best laboratories for developing these ideas. In company after company, division managers and presidents told me that manufacturing was a competitive millstone dragging the company down. One President said, “Professor Skinner, we don’t have a philosophy of manufacturing.” When I asked him what he meant by “a philosophy of manufacturing”, he said, “Professor, I don’t know what I mean but I know we don’t have one.”

Actually, this company did have one: it was the product of financial rules of thumb. What the company needed was a set of five strategies of manufacturing to suit each of the five divisions’ unique competitive, economic and technological realities. Placing all of these demands on one plant was a recipe for disaster—which was a good description of their situation. This company was desperate enough to try: led by the five Division Managers, the organizationally separated operations saw immediate improvements which accelerated as the separate manufacturing policies went into

full effect. The President and his financial experts were overjoyed, if thoroughly surprised. From this (frankly risky) exercise came the notion of the *focused factory*, the subject of a follow-on *Harvard Business Review* article (Skinner, 1974).

The ideas in the two articles – “Missing Link” and “Focused Factory” – are actually the same, but the second article deals more with “what to do”. “The Focused Factory” turned out to be a catchy phrase (my worst critics admit that at least my titles are great). This caught the attention of not only production people but also top management, which led to more implementation challenges.

6. Getting the ideas into the academy

The doctoral years in a good doctoral program are intensive and formative. The program and the process require intensive investment of time and talent, generally with influential teachers who are demanding. The result is the formation of considerable solid expertise. It may be narrow and deep but, in a good program, it is exciting and extremely satisfying to become very good at something. And that something is with the doctoral graduate forever. So trying to get them to invest deeply in some other approach to their field or (even worse) a different theory of how it all works, can be uninteresting or even threatening if it defies their hard-earned premises and assumptions.

Back at HBS, the Manufacturing Policy course was even more popular and the first year MBA course began to use the “Missing Link” as a reading at the end of the course. Students complained, “Why didn’t you give this at the beginning of the course?” They liked it, of course, but at the beginning of the course it would not have meant anything to them. But, like most of their colleagues in business schools throughout the country, they were studying simulation, job-shop scheduling, queuing theory, and operations in services, government, hospitals and international settings. The variety of settings was good but the paradigm continued to be breaking the job down into small parts and improving the efficiency of each part. Curricula and thinking patterns were stuck: the fundamental ideas of MCS were unrealized in the classroom. To make matters worse, there were tensions between “the quantitatives” and “the managements”.

Now academic tradition came into serious play: selection of new faculty and promotion and tenure criteria. In 1972, the appointment process in the Production Area at HBS quickly focused clearly and simply on whether the future lay best in *quantitative* or

management territory. As faculty appointments are made one by one, the contest turned into a nasty zero sum game. After a major appointment came up and an ongoing crisis loomed, the younger faculty, caught between two warring camps, went to Dean Lawrence Fouraker and asked for his intervention. After interviewing quite a few of us, he asked me to take over as Area Chair.

With the gracious support and total cooperation of Dick Rosenbloom, a leader from the other side, the process of renewal was quite straightforward. We agreed as an Area that we should enrich ourselves and our work by learning from each other, and to make this possible by employing and promoting competent and well-qualified people of every stripe. We quickly agreed that roughly 20% of our people would have a primary focus in one of the following sectors: production management, service operations, international operations, quantitative analysis and, because of particular current interests, operations scheduling and inventory control. We also agreed that any Area member should feel free to move about, that the first-year course needed to be retooled and that each member of our group – having been carefully selected and coached – deserved full respect and all possible support for his or her research. We agreed that we would all support success. Whatever worked well would benefit us all. Let the market prevail.

I served as Area Chair for about 3 years and can say that after my regime was over, the Area went on to do even better. We were in gear. Once we had agreed on the advantages of diversity and openness we did let market forces prevail and this evolutionary process led to new courses, new people, and the survival of individuals with skills and ideas that worked.

Under subsequent leaders – particularly Bob Hayes, Earl Sasser and Kim Clark, all of whom I claim with immodest pride as my progeny – year by year we grew stronger. The most able people attracted younger ones with relevant interests; new hires found excitement in management issues. Extraordinary attention was placed continually on the development needs of non-tenured faculty.

7. Where are we today?

The history of Operations Management – and of “Manufacturing in Corporate Strategy” – is part of the history of business schools. The story of my experience within my Area at HBS has parallels in other Areas and Departments in business schools, which were alive in the 1950s and 1960s. This was an

era in which the place of business in the university was challenged and business schools mounted a response. The theme of the response was to move towards faculty and curricula with “scientific” credentials—faculty and curricula, which would be recognized and respected by colleagues within the university establishment. The issue of the developing relationship between the “quantitatives” and the “managements” was not unique to the Production Area, nor to Harvard. What is the situation in business schools nearly five decades later?

Here is what Bennis and O’Toole (2005) have said:

“Business schools are on the wrong track. . . MBA programs face intense criticism for failing to impart useful skills, failing to prepare leaders, failing to instill norms of ethical behavior – and even failing to lead graduates to good corporate jobs. . . One outspoken critic, McGill University professor Henry Mintzberg, says that the main culprit is a less-than-relevant MBA curriculum. If the number of reform efforts underway is any indication, many deans seem to agree with this charge. But genuine reform of the MBA curriculum remains elusive. We believe that is because the curriculum is the effect, not the cause, of what ails the modern business school.” (pages 97–98)

These distinguished professors from the University of Southern California’s Marshall School of Business trace the cause of the malaise:

“During the past several decades, many leading B schools have quietly adopted an inappropriate – and ultimately self-defeating – model of academic excellence. Instead of measuring themselves in terms of the competence of their graduates, or by how well their faculties understand important drivers of business performance, they measure themselves almost solely by the rigor of their scientific research. . .

This *scientific model*, as we call it, is predicated on the faulty assumption that business is an academic discipline like chemistry or geology. In fact, business is a profession, akin to medicine and the law, and business schools are professional schools – or should be. . . The distinction between a profession and an academic discipline is crucial. In our view, no curricular reforms will work until the scientific model is replaced by a more appropriate model rooted in the special requirements of a profession.” (ibid., page 98)

It is beyond my abilities and, indeed, my purpose in telling the history of my own thought development, to comment on business schools' approaches to research today. For one thing, I have not been a faculty member recently and, further and more important, it is clear that there are many effective avenues to research, which can lead to new knowledge, techniques and/or concepts useful to managers. What I can say from my own experiences as a researcher is:

1. I started all my research because of a concern that a large number of managers were up against serious problems or dilemmas for which they lacked an understanding, a technique, or a concept, which could lead them to effective actions. In other words, I tried to work only on problems: real problems, big problems, difficult problems.
2. I gathered knowledge and data from getting out into industry, generally under pressure to figure out what was happening and why. So I wrote cases and reports and talked with managers at all levels, always wrestling with why smart people were doing things, which were generally not working out well.
3. This drove me to search out their premises, beliefs, habits of thinking and modes of taking action.
4. This led to new ideas and often contrarian concepts.

Basic to all this work was case type research, case teaching, and the writing. At each stage I was forced to test my "conclusions": were they really true, accurate, or "complete"? Did I like them? Did my students or clients like them? This approach drives the researcher to ask again and again, "Does this make sense?" "Will it be useful?"

Clearly this is not the typical tightly designed, controlled and documented type of research generally considered "scholarly". But it is perfectly scholarly if it is based on carefully collected facts, plenty of data and

not just a few anecdotal experiences, and presented in a clear, reasonably calm, manner.

The advantage of this *case method research methodology* is its breadth, its realism, and its not having to "prove" a narrow point by limiting variables. Conventional "scholarly" research is often, by its very rigor, limited to a minor scale of tightly defined problems. Hence, this approach can use up a lot of the researcher's time while producing little of use or value to the practitioner.

I believe that this is the essence of many criticisms of the current professional schools' research output. It is fostered by promotion systems, which narrowly restrict the young researcher to "manageable" problems upon which scholastic rigor can be demonstrated. I am shamelessly chauvinistic: I know this full well. But I must state that the Harvard Business School is different and better because we have no departments, we try to avoid minutiae, we get out in the field for data, we write hundreds of cases every year, we try to listen to and help managers, we promote based on effective impact on students and managers, and we turn down candidates for promotion if the candidate publishes trivia.

Well, to each their own. I can only say in closing that my approach has certainly been a lot of fun!

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