Developing Theory about the Development of Theory

by Henry Mintzberg

I have no clue how I develop theory. I don't think about it; I just try to do it. Indeed, thinking about it could be dangerous:

> The centipede was happy quite
> Until a toad in fun
> Said, "Pray, which leg goes after which?"
> That worked her mind to such a pitch,
> She lay distracted in a ditch
> Considering how to run.
>
> (Mrs. Edward Craster, 1871)

I have no desire to lay distracted in a ditch considering how to develop theory. Besides, that's the work of cognitive psychologists, who study concept attainment, pattern recognition, and the like, but never really tell us much about how we think. Nonetheless, I'll take the bait, this once, at the request of the editors of this book, because I probably won't get far either.

I want to start with what theory isn't and then go on to what theory development isn't, for me at least, before turning, very tentatively, to what they seem to be.

What Theory Isn't: true

It is important to realize, at the outset, that all theories are false. They are, after all, just words and symbols on pieces of paper, about the reality they purport to describe; they are not that reality. So they simplify it. This means we must choose our theories according to how useful they are, not how true they are. A simple example will explain.

In 1492, we discovered truth. The earth is round, not flat. Or did we; is it?

To make this discovery, Columbus sailed on the sea. Did the builders of his ships, or at least subsequent ones, correct for the curvature of the sea? I suspect not; to this day, the flat earth theory works perfectly well for the building of ships.

But not for the sailing of ships. Here the round earth theory works much better. Otherwise we would not have heard from Columbus again.

Actually that theory is not true either, as a trip to Switzerland will quickly show. It is no coincidence that it was not a Swiss who came up with the round earth theory. Switzerland is the land of the bumpy earth theory, also quite accurate—there. Finally, even considered overall, say from a satellite, the earth is not round; it bulges at the equator (although what to do with this theory I'm not sure).

If the earth isn't quite round or flat or even even, then how can we expect any other theory to be true? Donald Hebb, the renowned psychologist, resolved this problem quite nicely: "A good theory is one that holds together long enough to get you to a better theory."

But as our examples just made clear, the next theory is often not better so much as more useful for another application. For example, we probably still use Newton's physics far more than that of Einstein. This is what makes fashion in the social sciences so dysfunctional, whether the economists' current obsession with free markets or the psychologists' earlier captivation with behaviorism. So much effort about arm's lengths and salivating dogs. Theory itself may be neutral, but the promotion of any one theory as truth is dogma, and that stops thinking in favor of indoctrination.

So we need all kinds of theories—the more, the better. As researchers, scholars, and teachers, our obligation is to stimulate thinking, and a good way to do that is to offer alternate theories—multiple explanations of the same phenomena. Our students and readers should leave our classrooms and publications pondering, wondering, thinking—not knowing.

What Theory Development Isn't: objective, and deductive

If theories aren't true, how can they be objective? We make a great fuss about objectivity in science, and research, and in so doing, often confuse its two very different processes. There is the creation of theory, which this book is supposed to be about, and there is the testing of theory. The former relies on the process of induction—from the particular to the general, tangible data to general concepts—while the latter is rooted in deduction—from the general to the particular.

These two processes can certainly feed each other; in fact great scholarship, at least in the hard sciences, goes back and forth between them. But not necessarily by the same person. I'm glad that other people test theory—i.e., do deductive research. That is useful; we need to find out, if not that any particular theory is false (since all are), at least how, why, when and where it works best, compared with other theories. I just don't believe we need so many people doing that in our field, compared with the few who create interesting theory (for reasons I shall suggest shortly).

As for myself, I have always considered life too short to test theories. It never ceases to amaze me how we tie ourselves in knots testing hypotheses in our field, whether it be "does planning pay?" or "do companies do well by doing good?" Maybe the problem is that our theories are about ourselves, and how can we be objective about that, compared with researchers who study molecules and stones.

What makes me salivate is induction: inventing explanations about things. Not finding them—that's truth; inventing them. We don't discover theory; we create it. And that's great fun; if only more of our doctoral students took the chance. But no, they are taught to be objective, scientific (in the narrow sense of the term), which means no invention please, only deduction. *That* is academically correct.

Propper Research In the *Strategic Management Journal* a few years ago, its editor wrote in an editorial that "if our field is to continue its growth, and develop important linkages between research and practice, as it must, then we need to improve our research and understand that relevance comes from rigor" (Schendel 1995:1). This claim itself was not rigorous, since no evidence was presented on its behalf. As usual, it was taken as an article of faith.

Read the "rigorous" literature in our field, and you may come to the opposite conclusion: that this kind of rigor—methodological rigor—gets in the way of relevance. People too concerned about doing their research correctly often fail to do it insightfully.

Of course, intellectual rigor—namely, clear thinking—does not get in the way of relevance. The editor referred to this too in his editorial (as "careful logic"), but what he meant was the following: "Research in this field should not be speculation, opinion, or clever journalism; it should be about producing replicable work from which conclusions can be drawn independently of whoever does the work or applies the work result" (p.1).

I think of this as bureaucratic research, because it seeks to factor out the human dimension—imagination, insight, discovery. If I study a phenomenon and come up with an interesting theory, is that not rigorous because someone else would not have come up with the same theory? Accept that and you must reject pretty much all theory, from physics to philosophy, because all were idiosyncratic efforts, the inventions of creative minds. ("I'm sorry, Mr. Einstein, but your theory of relativity is speculative, not proven, so we cannot publish it.") Sumantra Ghoshal wrote to the same editor about an article that he had earlier reviewed:

> I have seen the article three times… The reviewing process, over these iterations, has changed the flavor of the article significantly. I believe that the new argument… is interesting but unavoidably superficial… Citations and literature linkages have driven out most of the richness and almost all of the speculation that I liked so much in the first draft. While the article perhaps looks more "scholarly," I am not sure who exactly gains from this look… I cannot get over the regret of description, insight and speculation losing out to citation, definition and tightness. (reprinted in Mintzberg, 2004: 399)

But it does so much of the time, because we confuse rigor with relevance, and deduction with induction. Indeed the proposal I received for this very book did that: "…the process of theory building and testing is objective and enjoys a self-correcting characteristic that is unique to science. Thus the checks and balances involved in the development and testing of theory are so conceived and used that they control and verify knowledge development in an objective manner independent of the scientist."

They sure do: that is why we see so little induction in our field, the creation of so little interesting theory.

Karl Popper, whose name a secretary of mine once mistyped as "Propper," wrote a whole book about *The Logic of Scientific Discovery* (1934, 1959). In the first four pages (27-30), in a section entitled "The Problem of Induction," he dismissed this process, or more exactly what he called, oxymoronically, "inductive logic." Yet with regard to theory development itself, he came out much as I did above.

> The initial stage, the act of conceiving or inventing a theory, seems to me neither to call nor logic analysis not to be susceptible of it. The question how it happens that a new idea occurs to a man—whether it is a musical theme, a dramatic conflict, or a scientific theory—may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge. This latter is concerned not with *questions of fact* (Kant's *quid facti?*), but only with questions of *justification or validity* (Kant's *quid juris?*)… Accordingly I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically. (P. 31)

Fair enough. But why, when he devoted the rest of his book to "the deductive method of testing" (p. 30), did Popper title his book "The Logic of Scientific *Discovery*"? What discovery is there in deduction? Maybe something about the how, why, when, and where of given theory perhaps (as noted earlier), but not the what—not the creation of the theory itself. (Indeed why did Popper call his book *The Logic of Scientific Discovery* when in the passage above he used, more correctly, the phrase “scientific *knowledge*”?) And why have untold numbers of researchers-in-training been given this book to read as if it is science, and research, when it is only one side of these, and the side wholly dependent on the other, which is dismissed with a few words at the beginning? What impression has that left on doctoral students in our fields? (Read the journals.) As Karl Weick (1969: p. 63) quoted Somerset Maugham, “She plunged into a sea of platitudes, and with the powerful breast stroke of a channel swimmer made her confident way towards the while cliffs of the obvious.”

Popper devoted his book to deductive research for the purposes of falsifying theories. But as noted earlier, falsification by itself adds nothing; only when it is followed by the creation of new theories or at least the significant adaptation of old ones do we get the necessary insights. As Albert Hirschman put it, “A model is never defeated by the facts, however damaging, but only by another model.”

Qualitative Research? While on this subject, let me try to clarify another confusion, the use of the terms “quantative” and “qualitative” when we mean “deductive” and “inductive”. It is as if all deduction is quantative and all induction is qualitative. Not so. Theories can be assessed without numbers (even, dare I say, judgmentally—which, by the way, is what most seven point scales really amount to), just as numbers can be used to induce theories. Indeed, I was invited to contribute to this volume because of an inductive study I did that was loaded with numbers (*The Nature of Managerial Work,* 1973; for a better example, see my paper with Alexandra McHugh, “Strategy Formation in an Adhocracy" [1985], which has often been referred to as qualitative even though it is based on a study of 3000 films tabulated every which way).

This mix-up leaves the impression that "quantative" research is somehow proper (or Propper)—i.e., "scientific"—even if it contributes no insight, while qualitative research is something to be tolerated at best, and then only when exemplary. This is the double standard that pervades our academic journals to their terrible discredit. It also manifests itself destructively in doctoral courses that teach quantative methods (mostly statistics) as rites of passages. Those who cannot handle the fancy techniques cannot get the doctoral degree, even though there is all kinds of wonderful research with no numbers. Why not instead preclude from doctoral program students incapable of coming up with interesting ideas. Imagine that!

What Theory Seems to Be: a continuum

I have not thought much about what theory is either. I am interested in explanation, and don't much care what it's called, theory or otherwise.

When I think about it, however, I see explanation along a continuum, from lists (categories), to typologies (comprehensive lists), to impressions of relationships among factors (not necessarily "variables": that sounds too reified for many of the factors I work with), to causations between and patterns among these relationships, to fully explanatory models (which interweave all the factors in question).

I think of myself as an obsessive categorizer—I love neat typologies—but I have done my share of trying to develop relationships and models too.

As noted earlier, I am supposed to be using here my research on managerial work, presumably as I first developed it for 1973). There I described various characteristics of managerial work and a framework of the roles managers seem to perform, as well as discussing variations in managerial work. Much of that was more about lists and typologies, with lots of impressions as well as numbers, than a full-blown model. (Put more exactly, perhaps, the models in that book were its weakest part.) Only much later ("Rounding Out the Manager's Job," 1994), did I come up with more of a model, by using the categories of my earlier work as well as those of other studies. (Diagrams from these two works follow later.)

The theory development of which I am more proud—I see it as my most parsimonious work—is in my book *The Structuring of Organizations* (1979). I described first how organizations function, in terms of five basic parts and five essential mechanisms of coordination. After describing the basic parameters of designing organizations (positions, superstructure, linkages, etc.), and contingency factors influencing that designing (age and size of the organization, complexity and dynamism of its environment, etc.), I wove all this together around a typology of five models: forms, or "configurations" (i.e., patterns) of organizing, each a theory unto itself, with detailed explanations and causations. Later (1989, Section II) did I weave these different models into a model in its own right, using what I called forces alongside forms, to discuss configuration, combination, conversion, contradiction, and competencies, ending with a life cycle model of organizations.

What Theory Development Seems to Be: unexpected

We get interesting theory when we let go of all this scientific correctness, or to use a famous phrase, suspend our disbeliefs, and allow our minds to roam freely and creatively—to muse like mad, albeit immersed in an interesting, revealing context. Hans Selye, the great endocrinologist, captured this sentiment perfectly in quoting one item on a list of "Intellectual Immortalities" put out by a well-known physiology department: "Generalizing beyond one's data." Selye quoted approvingly a commentator who asked whether it would have been more correct for this to read: "*Not* generalizing beyond one's data" (1964:228). No generalizing beyond the data, no theory. And no theory, no insight. And if no insight, why do research?

Theory is insightful when it surprises, when it allows us to see profoundly, imaginatively, unconventionally into phenomena we thought we understood. To quote Will Henry, "What is research, but a blind date with knowledge." No matter how accepted eventually, theory is of no use unless it initially surprises—that is, changes perceptions. (A professor of mine once said that theories go through three stages: first they're wrong; then they're subversive; finally they're obvious.)

All of this is to say that there is a great deal of art and craft in true science. In fact, an obsession with the science, narrowly considered, gets in the way of scientific development. To quote Berger, "In science, as in love, a concentration on technique is likely to lead to impotence" (1963:13).

Some [Emerging] Propositions about Theory Development

So how to do this generalizing beyond the data, this subjective, idiosyncratic musing like mad in order to climb the scale from lists to models? I have no idea what goes on in my head, as I noted earlier, but I can describe, in a series of propositions, some of the things that happen outside of it, up to and after the point where my head takes over. So let's look at what can be articulated, while accepting that this is about a process that is most significantly tacit.

First, I start with an interesting question, not a fancy hypothesis. Hypotheses close me down; questions open me up. I have started with, for example: What do managers do? How do organizations structure themselves? How do strategies form? And now: How can we redress the balance in this economically obsessed world?

I think of this approach as pull, not push, and I believe it key to theory development. Let yourself be pulled by an important concern out there, not pushed by some elegant construct in here. Take your lead from behavior in practice. And ask the big questions. In my experience, the problem in doctoral theses, and subsequent research people do, is not that they bite off more than they can chew, but that they nibble less than they should consume. Or to use another metaphor, I admire researchers who try to build cathedrals, not lay a few bricks. As Fritjof Capra put it in *Turning* Point**,** "If I ask it a particle question, [the electron] will give me a particle answer" (1982:77).

Second, I need to be stimulated by rich description. There are novelists who sit down with a blank pad and write—management theorists too, I suppose. I can't do that. I need to be stimulated by some body of rich inputs that I see right before me. Tangible data is best—the “thick” description that Clifford Geertz has described---not data all nicely ordered and systematically presented. (Robert Darnton has described Geertz’s work as “open-ended, rather than bottom-lined.”) And stories are best of all, because while hard data may suggest some relationship, it is this kind of rich description that best helps to explain it. So anecdotal data is not incidental to theory development at all, but an essential part of it.

But this needn’t be data per se. My favorite among my own books, *The Structuring of Organizations,* was written out of the theories, research findings, and descriptions of others—in other words, it was based mostly on existing literature. But even here, it was the thickest descriptive literature, closest to the data, most notably in Joan Woodward’s work (to which I shall return), that helped me most in the development of the theory. Highly structured descriptions, for example based on data collection around a couple of abstract variables, were far less useful. Think of the would-be theorist trying to swim in water as compared with a tank of shredded paper.

Third, and perhaps trickiest of all, I need to bootstrap an outline. That is, I must have an outline to write down my ideas, even if the object of writing down my ideas is to come up with an outline. This is the ultimate problem in creating theory (and, I suspect, doing interesting writing in general).

No matter how we *think* about our theories, ultimately we have to convey them to other people in linear order, and that means mostly in words. Mozart claimed about creating a symphony that the “best of all is the hearing it all at once.” (He also wrote about being able to “see the whole of it at a single glance in my mind.”) Wow! I wonder what that's like. But even Mozart had to convert it to linear order on paper so that others could play it.

The trouble with linear order, of course, is that the world we are trying to explain does not function in linear order. Now, if I don't start with a blank sheet of paper, but in my case all kinds of little papers scribbled with my notes, about findings and ideas, etc., plus neat sheets of paper printed with the findings and ideas of others, what am I to do with them? How can I order them when I don't have an outline (an order) to begin with? But how can I get an outline if I have no way to code the very inputs to the outline. And if I *do* have an outline, or theory, to begin with, how am I supposed to suspend disbelief to get to a better theory? Theory *is* belief.

There is no solution to this bootstrap problem except, I guess, something equivalent to how you lift yourself up by the bootstraps. A little bit at a time, using whatever you can get a hold of. (Climb on a stone? Tie a rope to a tree?)

Linear outlines are great. I still have the one I finally ended up with for *The Structuring of Organizations*—about 200 pages long! It was so detailed that I wrote the first draft of this 500 page book in three months. I never did such an outline again, and always pay the price. I used much sloppier outlines, and so have had to rewrite and rewrite and rewrite. Very messy to redo an outline in prose! *The Structuring of Organizations* came out faster—at least *once* I had that outline—and far more coherently, or perhaps I should say orderly, than any other book I have written. It literally integrates from the opening dedication to the final sentence. On the other hand, messy can sometimes be better, because it can be richer. To quote Voltaire, "Doubt is not a pleasant condition, but certainty is a ridiculous one" (in Seldes, 1983:713).

Fourth, linearity notwithstanding, I use diagrams of all kinds to express the inter-relationships among the concepts I am dealing with. *[Let's stop for a moment and considering what is happening here. I am writing, by hand, for reasons I'll explain in a later point. (See what I mean about linearity. I keep mixing up the order of my points. What a pain. If only, like Mozart, you and I could see the whole of this at a single glance in our minds.) I decided after the last point that I would start each new point on a new page, so I can easily go back and stick in points I hadn't thought about before, that should come between. This may seem like an awfully clever idea—I'm just kidding—but it wasn't really an idea. I happened to start the last point on a new page because the previous one finished at the bottom, and then I thought, hey, that's good, I should do that for all the points. You see, here too I am responding to what I see before me. I do have an outline—some scribbles about various points I want to make, based on having reviewed other papers I wrote about research (Mintzberg 1973 Appendix C, 1979b, 1982, 1991, 1993, 2002, 2004: 250-252, and 1984 with Danny Miller). But I haven't looked at this outline for awhile, because as I got into the first point, all kinds of other points occurred to me. It's the rendering of this on paper that really gets the ideas flowing in my head. Please take this as* **point #5**.]

My work is loaded with diagrams, seeking to express every which way how the ideas I am trying to make come together. Aristotle said that "The soul…never thinks without a picture." I try to help my soul think. Years ago, I wrote an autobiographical piece called

"Twenty-five Years Later…the Illusive Strategy" (Mintzberg 1993: see www.mintzberg.org) for a collection Art Bedeian put together. In looking over my own publications, tracking my own strategies as patterns in my writings, much like I have tracked the strategies of organizations, I found something interesting: there were distinct periods in the diagrams I did. Kind of like in painters' work (e.g., Picasso's "Blue Period"). I used rectangular boxes (flow charts, and like) in my earliest years (as in *The Nature of Managerial Work)*, blobs to depict organizations in the next period (as in *The Structuring of Organizations)*, and then everything went into circles of one kind or another. (See Figures 1a and b for an example of the first and last from my writings on managerial work.)

These diagrams really help me a great deal: I can see it all at a glance, even if outside my head. But not always into other heads. I have been puzzled to find that some people are puzzled by these diagrams. They don't think in such terms, nor are they even able to *see* it in the work of others. Even many of my own doctoral students, sometimes including the best, when urged by me to express their ideas in diagrammatic form, have not gotten past a 2X2 matrix or two. Maybe this has to do with my education as a a mechanical engineer—probably the only thing left of that—or at least my pre-disposition to do that kind of education, because I like to *see* things altogether, at a single glance, to quote a famous composer.

[Back to what's going on here. As new ideas are coming up, while I am writing down the previous ones like mad (maybe I'm just the medium?), I make little notes in the margins so as not to forget them. Now I am going to go back and look at them. Then I'm going to return to the outline to see if I am remotely on track. I know I am—remotely. But

first I should point out that I did not make a note about inserting this italic type in these square brackets, about these going ons here and now (as you are now reading), because it occurred to me to do that just as I started the fourth point (above), and so I did it straight away, although it is only now, in this second of these square brackets, that I realize what I am doing: I am using this *experience itself to figure out how I develop theory (if you can call these musings theory). Got that?! [If the above seems confusing—as I reread it, I can't blame you, so maybe you should reread it too!—then you should be getting an idea of what's really involved in the development of theory.] Think about how much richer is this writing experience itself as the basis for writing this paper than a book I did thirty years ago. How can I theorize about that, as compared with this, which is happening right here and now? What better to theorize about? So I have gone back and changed the title of this paper. It was "Sorry—No Theory for Theory." Now it is "Developing Theory About the Development of Theory."]*

[Now this is interesting. I have just gone back to my notes and found a note about what are the "sources of inputs" for theorizing. It said: "any and all—you never know what will work." Little did I realize how true that would prove to be here: how the best inputs for doing this paper have proved to be doing this paper!]

[Years ago, I heard about a well-known Australian potter approached by a researcher who wanted to study the creative process. He proposed to elicit protocols as the potter worked. But that didn't do it—the potter couldn't verbalize. Then he had a creative idea, consistent with his own creative process. He proposed to make a thousand pots in succession, each influenced by the last, so that the researcher would

have a visual record of the creative process. Nine hundred and ninety nine more articles like this and maybe we'll have an idea of how I develop theory. In the meantime, assuming the tolerance of the publisher will not stretch that far, you have one. [Afterthought: I will not do draft after draft after draft of this paper, as I usually do. Aside from cleaning it up for clarity, somewhat, I want to leave the outline and conceptual points more or less as they developed. What would be the use of showing you only the thousandth article? Too much theory about theory development is already like that—neat rationalizations of a messy process. Here you have the full messiness of the first effort!]]

Sixth *[going back to the original outline, which is why this may seem a little disconnected]*, **to develop good theory you have to connect and disconnect.** In other words, you have to get as close to the phenomena as possible in digging out the inputs (data, stories, and lots more), but then be able to step back to make something interesting out of them.

Too connected and you risk getting co-opted by the phenomenon—that, to my mind, is why so called "action research" has, with a few notable exceptions, not produced much interesting theory. The researcher cannot have his or her cake (of consulting income) and eat it too (with research publications—practical publications maybe, good research ones rarely). Researchers have to be able to step back.

But too disconnected and you cannot develop interesting theory either. As suggested earlier, the imagination is stimulated by rich description, nuanced exposure: stories and anecdotes are better than measures on seven point scales and the like. If you are going to measure, then measure as much as possible in real terms—close to how things actually happen in the world, for example the time managers actually spend on email instead of the time managers claim they spend on email (unless of course you are studying perceptions). This is what I believe to be the problem hounding economics today. This is one social science where researchers have nowhere to go to observe firsthand the behaviors they seek to describe. So they pile abstraction upon abstraction. (Sure there are fish markets. But ironically, what economists take to be markets today are fundamentally removed from the markets we can all go and see. These are places of community, where economic, social, and cultural factors all converge. The arm's length markets of today's economics overemphasize the economic at the expense of the social and cultural: they are antithetical to communities.)

Do we encourage our researchers to connect every which way? Hardly. In the case of doctoral students, we lock them in libraries for years and then tell them to go find a research topic. The library is the worst place in the world in which to find a research topic. Even students who were once in the world of real things have forgotten what goes on there.

The result is that a great deal of research is pushed by some theoretical construct or angle: game theory, networking concepts, beliefs about corporate social responsibility (yet again), whatever is fashionable in the world of academe. Under the scrutiny of such single lenses, organizations look distorted. Recall the "rule of the tool"—you give a little boy a hammer and everything looks like a nail. Narrow concepts are no better than narrow techniques. Organizations don't need to be hit over the head with either.

Seventh, to connect, you have to keep the research method simple, direct, and straightforward. For example, just go look (while of course recording carefully what you see). As usual, Yogi Berra said it best: “You can observe a lot just by watching.” Or in the more somber words of a Russian proverb: “Believe not your own brother—believe, instead, your own blind eye.”

The initial supervisor of my doctoral program told me that my thesis should be “elegant”. He meant methodologically elegant. I have always prided myself in the inelegance, or at least straightforwardness, of my methodology—I called it “structured observation” (written up in an appendix to *The Nature of Managerial Work*). I sat in managers’ offices and wrote down what they did all day. That, I believe, helped me get closer to elegant conclusions.

We are altogether too hung up on fancy methods in our field, and in much of the social sciences in general. All too often they lead to banal results, significant only in the statistical sense of the word. Elegant means often get in the way of elegant ends. People intent on being correct often go astray. What, for example, is the problem with a sample of one, at least for induction. Piaget studied his own children; a physicist once split a single atom. Who cares, if the results are insightful. Alternately, what is more boring than a bunch of academics arguing about statistical tests. Sure we need to get them right. But let’s not confound them, as did Popper, with scientific discovery.

Eighth, researching is detective work: you have to dig, dig, dig, for every scrap of information you can get. Don’t forget about that “you never know.”

Ninth, take prolific notes*. [I realize now that from the sixth point I have changed the sentence construction, to a more declarative form. But that just reveals* ***point #10*** *(not in my notes):* ***At early stages, keep it messy.*** *This paper, as noted, is at an early stage!]* I write down everything I can think of. When I am working on something, I have little scraps of paper coming out my ears. About anything, everything. Sometimes one is just a better way to word a particular idea I already recorded in another. In preparing for what I call my "Smith and Marx" pamphlet, there are ideas I have probably written down fifteen different times. Not because I forget the earlier versions: only because I think I have expressed it better each time. *[Which reminds me—bear in mind* ***point #11*** *(not in my notes either), that* ***it is not only having the ideas that make a successful theory but also expressing them engagingly.*** *William Schultz has made the engaging point that if you can't express your idea without jargon, maybe you don't quite have it: "When I look over the books I have written, I know exactly which parts I understood and which parts I did not understand when I wrote them down. The poorly understood parts sound scientific. When I barely understood something, I kept it in scientific jargon. When I really comprehended it, I was able to explain it to anyone in language they understood… Understanding evolves through three phases: simplistic, complex, and profoundly simple."]*

Twelfth, when possible as I go along I code the notes in terms of the outline. That is why I need the outline in advance of the writing. What can I possibly do with thousands of uncoded notes? Indeed, how could I even come up with these notes unless I have the sense of an outline? So I need the outline to think the thoughts and get the codes. But only after I have the thoughts can I really do the outline, and so the codes. This means I have to recycle back repeatedly to redo and flesh out whatever outline I do have, in order to enhance the codes and so to recode what has been coded. Got that?

Hopefully many of the codes stay the same—otherwise I can be at this for years. (Look for Smith and Marx in 2020. I did the first draft almost ten years ago!) All of this effort is to get everything in linear order, to get all those notes in one sequence, or at least in many little coded piles that are in one sequence. Then I can pull the piles out one by one, in order, to do sub-outlines of each section and then write. But what if in the thirty-second pile I pull out a note that should change the first thirty-one? Should I go on? Many people, I suspect, do. I also suspect you never heard of most of them. And that brings me to the most important point of all. If you can only retain one message from this paper, this is it!

Thirteenth, cherish anomalies. If you wish to get tenure within, say, fifteen years, you may be reluctant to constantly be recoding all your notes, let alone rewriting written text. But on some codes you must be in no hurry to close.

You are not going to make the great breakthrough from the note that fits. As you order the notes, it is of course quite nice when things fall into place. You proceed happily; parsimony is in your grasp, maybe tenure too. Also, perhaps, banality. And then comes this nasty note: some observation, idea, or example that simply refuses to fit. Weak theorists, I believe, throw such notes away. They don't wish to deal with the ambiguity. They want it all to be neat.

Keep these notes. Cherish them. Repeatedly return to them. Ask why? Why? Why? Be a bulldog ***(really point #14).*** Never give up trying to figure out what they mean. If you can come to grips with the anomaly, you may have something big. The poet W.B. Yeats captured this sentiment perfectly: “We made out of our quarrels with others rhetoric, but out of our quarrels with ourselves poetry.” Make poetry!

Anomalies are important because, to continue the text but not strictly the point, **fifteenth, everything depends on the creative leap.** And **sixteenth, that can be trivial**. So Fleming saw mould in parts of his sample. Big deal. Later he went back and found 31 footnotes (if I remember correctly some forgotten source) in the reports of other studies that experienced similar problems. For those researchers, it certainly was no big deal. They went on. Fleming stopped. Who knows what happened to the rest of his study, or all of theirs, but history records what happened to Fleming’s digression: we got penicillin, and eventually antibiotics in general.

What you set out to do doesn’t matter; it’s what you end up doing. Many of the best theses I have supervised ended up surprising their authors, and me. That is why I am not enamored of highly detailed research plans that leave no room for surprises. Back to Hans Selye for another wonderful quote, in remarks to the Canadian Senate on Science Policy: “I doubt that Fleming could have obtained a grant for the discovery of penicillin on that basis because he could not have said, ‘I propose to have an accident in a culture so that it will be spoiled by a mould falling on it, and I propose to recognize the possibility of extracting an antibiotic from this mould.’”

I get a great kick out of the fact that many of my doctoral students defend their thesis proposals well into their empirical work. After all, how can they know what they will do until they do it? I'm waiting for someone to defend the proposal in the morning and the dissertation in the afternoon!

Was Fleming a genius because of his insight? I'll bet many of those 31 other researchers were considered geniuses (then if not now). We have altogether too many geniuses in research and not enough ordinary, open minds.

I believe that there are not creative people in this world so much as blocked people. After all, every one of us has wild and wooly dreams. Only after we wake up do most of us stop being creative. (That is why the best of creativity so often happens at the interface, just as we wake up, when our more visually-inclined right hemisphere, where dreaming activity occurs, connects with our more analytically-inclined left, where speech takes place. That is when we are best able to connect those Mozart-like images with the linear order of words. To repeat, to be creative is not just to have the idea but also to express it.)

As the day unfolds, we hit the world as it is—fighting traffic to work, meeting an agitated boss, getting Propperian reviews of our latest journal submission—and that's the end of creativity. We get careful, or scared, either way blocked: we become *correct*. So much for those dreams.

I don't consider myself particularly creative. I certainly do badly in all those Mickey Mouse tests for creativity—like "Come up with 32 ways to …" See. I can't even invent one. On the other hand, I don't scare easily, not about ideas. Plus I have been lucky enough to fall into academic life, which is supposed to be about ideas, and at McGill, in Canada, which are particularly tolerant places. (I was on sabbatical in France when the dean called to say I got tenure. I didn't even know I was being considered! Times have changed, even at McGill.) So I have been able to respond to what I see before me—let it speak to me. The world is so rich and varied, that if you see it as it is, you are bound to appear creative. (And by the way I don't take seriously people who tell me that I am courageous. It doesn't take much courage to write words down on pieces of paper (unless, of course, you live in mortal fear that the Propperians will reject you). Yet I am amazed at how many colleagues are just plain scared to be different. That is not a good trait in an academic.)

My one hero in this world is the little boy in that Hans Christian Andersen story. Not because he *said* that the emperor was wearing no clothes: that was the easy part. Because he *saw* it. Amidst all those people who wouldn't let themselves see it, because they were afraid, he was open.

Fear is antithetical to theory development—fear of being different, fear of standing out, fear of not belonging, fear of being wrong, or subversive (if not obvious). Yet we have built fear into the whole process by which we do and assess research, especially in the tenure process. Open the journals and read the results.

I don't much care for regular doctoral students, the ones who have always done everything correctly—gotten the right grades, moved smoothly up some hierarchy, etc.—always as expected. As Paul Shepheard put it in *What is Architecture,* "The mainstream is a current too strong to think in." I cherish the ones who did the unexpected. (I should add that I have always prided myself in never having had a doctoral student who replicated any of my own research. If they couldn't break out on their own in their dissertation, when would they ever be able to do so? You can see the list of their topics in my C.V. on www.mintzberg.org.)

In other words, I prefer a bit of quirkiness in my doctoral students, which reflects no fear of being different. (Not too quirky, mind you: they still need the capability to get into the world and observe it firsthand, close-up.) Any kind of "correctness," even being a self-proclaimed "contrarian," impedes openness. In research, we have enough of people who see things as most everyone does; we desperately need ones prepared to step back and see the obvious as no-one else has. "Dare to be naïve," said Buckminster Fuller (1975:xix).

Theory development is really about discovering patterns ***[let's make this point #17],*** recognizing similarities in things that appear dissimilar to others, i.e., making unexpected connections. Theory is about connections, and the more, and the more interesting, the better.

In my first study, my doctoral research, I found that managers got interrupted a lot. That their work was largely oral. That they spent a lot of time in lateral relationships. I just wrote it down. All this had to be patently obvious to anyone who ever spent time in a managerial office, behind the desk or in front of it. (It is our great discredit that too few scholars of "management" ever did, or do.) These findings just didn't jive with the then (and largely still) prevalent view of managerial work, dating back to Henri Fayol's book of 1916: "planning, organizing, coordinating, and controlling." (Four words for controlling.) Where are the lateral relations here? What room does this leave for interruption? Sure managers control (this is our flat earth theory), but they do much that is evidentially quite different. I just wrote it down, and so managed to parlay some rather obvious observations about the emperor (not being naked so much as attired in a different suit) into an academic reputation. Lucky me. Not so difficult. Nobody ever said: “Are you kidding?” Somebody at least should have said: “They must all have been kidding for fifty years,” at least for what they failed to see. (Back to that emperor again—I guess you don’t mess with people in uniforms.)

When I wrote *The Structuring of Organizations*, just about the best research I read for it was that by Joan Woodward (*Industrial Organization: Theory and Practice*, 1965). But I couldn’t reconcile her findings about process industries with my outline to that point. Without thinking about anomalies, etc., I just kept coming back to my notes about this. When it finally hit me, when I figured out how to reconcile her description of the structures used in highly automated industries with my description of “adhocracy,” or project organization, used in fields like R & D and consulting, I had a breakthrough for my own framework. I realized that Woodward was in effect describing post-bureaucratic processes, ones that were so formalized, so perfectly machine bureaucratic, that they no longer needed human beings. That freed up the human beings to design and maintain the equipment, working in project teams, much like in those other fields. So beyond machine bureaucracy was adhocracy.

Eighteenth, once you have all those notes coded, and those anomalies messily set aside, you have to weave it all together. My example above about Woodward, and the story of Fleming, may have left one wrong impression, at least for the social sciences. It is rarely *the* insight that makes for an interesting theory. That usually comes from the weaving together of many insights, many creative leaps, most small and perhaps a few big. It's all in the weaving. And that comes, for me at least, in the writing, whether of the text itself (as I hope you have been able to see here) or of the detailed outline. And this leads, for me at least, to the **nineteenth point: clear the decks of technology: write, literally.**

Nothing impedes integrating more than that damn keyboard. It pushes everything away. It's just you and all those keys, etc.; everything else, all those glorious notes you may or may not have written, all those anomalies you should be cherishing, are pushed aside—you can barely find them.

There are apparently great poets who just wrote down their great poems. They could have used a keyboard. It all flowed out of their heads. But what about the poet who revised his great poem 91 times before he got it right? In those days, he had to use a flat desk. Would he have come up with that poem on a slanted keyboard and a vertical screen?

I write on a flat desk [*as I am doing* now] with my papers around me. I can pull them in every which way. I am comfortable with a keyboard (as a student, I was Sports Editor of the McGill Daily, although reporting on a hockey game is admittedly different from coming up with a theory). I even used word processing before almost anyone else, because a professor at McGill had a very early system. (That nearly drove us mad—for example, we had to correct from the end in!) But then, as now, I *write* and my P.A. types (now Santa, a gift from that other Santa). Indeed, I correct too on paper, still needing to spread out—and Santa retypes. Shall we ever understand what damage the keyboard has done to theory development?

And finally, twentieth (as mentioned already)**, iterate, iterate, iterate.** I write draft after draft after draft. I keep correcting, fixing, adjusting, reconceiving, changing, until it all feels right (for then; as noted about my work on managerial work, I came back to it later, seventeen years later—see my 1991 article "Managerial Work: Forty Years Later"). I am simply my own harshest critic. Nobody tears my work apart like I do. Too bad you can't see the thousand of pages of rewrites of my latest book, *Managers not MBAs.* It is 462 pages long, and every part of it was redone at least five times. One very long chapter, which was eventually spit into five chapters (3-6), was redone at least nine times. I just kept coming back until it felt right.

But not here. It feels right to keep this messy, like theory development itself, so as better to make my points. The first of a thousand articles, the rest never to be written.

So, do twenty points on theory development a theory of theory development make? And am I describing the flat earth of theory development, or the round earth, or the vertical face of some mountain that I am taking to be the whole earth? Who cares. If you have learned something helpful, that is what matters.

And if you haven't, then at least I leave you with a testable hypothesis. Right here. If theory creation really is replicable, then the author of another chapter of this book must have come up with the same theory about theory development. (Unless, of course, mine is not true.) So go test that hypothesis, all you Propperians.

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