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Methodology and research in the science of management

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Science, usefulness and methodology

To say that the goal of research is to produce scientific knowledge seems uncontroversial enough. And yet, we need to define what we mean by scientific knowledge, as confusion is rife. Ever since the end of the 18th century, science has proven enormously successful in improving people's physical living conditions. As a result, its standing has risen to the point where the strongest argument in favor of almost any proposition nowadays is that it is "scientific". If something is "scientific", then it is "good"; and if something is "good", then it must be "scientific".

It is easy enough to demonstrate the absurdity of such reasoning, but the fact is that people often mistakenly assume that "scientific" equals "good" in the sense of "useful". As we shall see later, it would be naïve to expect science to be immediately applicable. In a business school, it should be equally obvious, however, that a good deal of the knowledge that is useful to managers is not in the least scientific. Managers need to know the particular circumstances of the market in which they work, who's who in that market, where the opportunities lie, which of the employees' skills are unique and should be nurtured, etc. This is not something a person learns in school, but in daily contact with the world of the company or institution concerned.

Curiously, while science is overrated as a source of useful information, and useful information is overrated as regards its scientific credentials, the general public has come to somewhat distrust scientists. Only 50 years ago, the authority of an expert in any given area was unlikely to be questioned. Authority was somehow taken for granted. Since the protest era of the 1960s and 1970s, however, respect for experts in any field has declined considerably. In part, that may be because the general public has had its fingers burned, having earlier trusted expert opinion on certain matters (U.S. citizens learned to distrust what experts told them about Vietnam in the mid-'60s, for example). In part, it may be because society has become more complex, making it increasingly difficult to tell a "real" authority from a self-proclaimed expert, particularly in today's more sophisticated professions.

Sowell [1980] illustrates this last point with a very graphic example. It is very easy to find out whether a man knows how to milk a cow: you give him a cow and a bucket, and if the bucket comes back full, he knows; if not, he doesn't. In contrast, if a person claims to be good at designing marketing plans, there is no similar, obvious way to test his claim. Waiting to see what happens is not usually an option: you generally want to know before the plan is put into effect. What's more, it is impossible to measure the results without a degree of ambiguity or arbitrariness. That is why peer opinion is often the only test we have.

So here we have a first criterion for deciding whether a thing is scientific or not: a thing is scientific if scientists agree that it is. Yet, while this may be a good criterion for judging a particular piece of research, it is still worth asking what makes one statement scientific while another is not. That is where methodology comes in.

According to Christenson [1983], most researchers today (he is concerned about researchers in the field of accounting, but the observation applies to practically any other field) have a thorough grounding in research methods, but practically none in methodology. Making a plea for sanity in the use of language, Machlup [1963] explains the distinction between methods and methodology:

“Is it necessary that semiliterates get away with their misuse of words, and that through sheer repetition the misuse becomes respectable and legitimate by virtue of the authority of a new edition of Webster? Methodology, in the sense in which literate people use the word, is a branch of philosophy, or logic, though some logicians prefer to regard logic as part of methodology. Semiliterates adopt the word when they are concerned with neither philosophy or logic, but simply with ‘methods.’ Instead of ‘statistical techniques,’ they would say ‘statistical methodology,’ and instead of ‘research methods,’ they love to say ‘research methodology.’ They do not understand that the same method may be justified on very different methodological grounds, and that from the same methodological position one may defend very different methods of research.”

Thus, methodology is useful because it allows us to reflect on the foundations of our statements about the world.

Aristotle was probably the first person to give a definition of science and establish the conditions under which knowledge may be said to be scientific. In Book I of his *Posterior Analytics*, he says that “... by demonstration I mean a syllogism productive of scientific knowledge, a syllogism, that is, the grasp of which is eo ipso such knowledge”. Not all knowledge can be based on reasoning (syllogism), as knowledge requires premises that are “true, primary, immediate, better known than and prior to the conclusion. Syllogism there may indeed be without these conditions, but such syllogism, not being productive of scientific knowledge, will not be demonstration”. Somehow, then, Aristotle's basic criterion is this: science is everything that can be demonstrated (with reasoning) from initial truths which need no demonstration because they are self-evident.

Implicitly or explicitly, this has remained the most widely accepted definition for centuries, leading to an unjustified bias in favor of analytic propositions and supposed absolute certainties that Aristotle probably would not have liked. Thus, Descartes thought that any knowledge that can be questioned should not be called science, and an 18th century English encyclopedia declared that “science is a clear and certain knowledge of anything, founded in evident principles or demonstration” [Chambers, 1738]. Kant was less categorical; he saw that, alongside science properly speaking, which contained propositions that cannot be refuted, we have to include any other body of knowledge systematized according to certain principles.

Empiricism and positivism changed this situation, with their insistence (worthy of a better cause, let it be said) on the observability and empirical verifiability of any statement that claims scientific status. This brought about a cultural change in the abovementioned bias. While for many years it seemed as if any statement that was not deductive was not scientific, now almost the reverse is the accepted truth: anything that does not have an immediate empirical referent is unscientific. It goes without saying that neither of the two biases has a sound basis. It was clear even in Aristotle’s original conception that all science must have both a formal (deductive) structure and an empirical basis; and that both must be well founded, that is to say, rigorous. Sadly, “scientific” nowadays is often taken to mean “empirical”, while “rigor” is thought to come from the use of statistical methods when, in fact, the way the discipline of statistics is used nowadays is often far from scientific, methodologically speaking. At the same time, the obsession with the observability or verifiability of conceptual constructs can all too easily lead to disdain for potentially useful concepts and constructs. This has happened even in physics¹, while Godfrey and Hill [1995] have recently shown how this happens in the field of strategic management too.

Rigor, defined in the Webster’s Dictionary as “scrupulous or inflexible accuracy or adherence”, is what gives a statement the right to be considered scientific. In recent years, mainly in the context of economics, there has been a debate about “rigor versus relevance”, as if there were necessarily a tradeoff between the two. This debate is clearly misguided. As Bunge [1988] says,

“Science is useful: because it seeks the truth, science is effective at providing tools for good and evil. Ordinary knowledge usually concerns itself with obtaining results that can be applied immediately: as a result, it is not sufficiently true, which means it cannot be sufficiently effective. Given a true knowledge of things, it is possible to manipulate them successfully. Science is useful because it is objective: without actually aiming for applicable results, science does in fact provide them, sooner or later. ... Therefore, urging scientists to produce applicable knowledge is redundant: they cannot do otherwise. It is the job of technicians to put scientific knowledge to practical use, and it is the job of politicians to ensure that science and technology are used for the benefit of humanity.”

What could be more “relevant” than astrology? It concerns our health, our success in business, our safety while traveling, and our relationships. These are all vitally important matters in a person’s life. The only problem is that there is no good reason to believe what astrology tells us. Astrology has no foundation, no “rigor”, though that did not stop a president of the United States, Ronald Reagan, from having an astrologer on his payroll.

Herbert Simon [1979], in his acceptance speech for the Nobel Prize, put it differently:

“It is a vulgar fallacy to suppose that scientific inquiry cannot be fundamental if it threatens to be useful, or if it arises in response to problems posed by the everyday world. ... There is, however, a converse fallacy that deserves equal condemnation: the fallacy of supposing that fundamental inquiry is worth pursuing only if the relevance to questions of policy is immediate and obvious.”

Science and truth

There are many misconceptions about science, perhaps the most common being that some theories are false while others are true, when in fact “true” and “false” are not adjectives that can be applied to scientific theories.

Scientific theories are often compared to maps (see, e.g., Polanyi, 1958; Christenson, 1980). A map can be said to be a “theory” of the terrain it is intended to represent. It is not difficult to see that a map can never be “true” in any sense. All maps are to some extent false, as there are always details of the terrain that they do not show. Maps may or may not be useful for finding one’s way around, but in no way are they a “realistic” representation of the world. The only possible “realistic” representation would be a perfect duplicate of the original, on exactly the same scale and with the same features and details. Obviously, that would make it perfectly useless as a map. A map is useful precisely because it is a simplification of the terrain it represents (much smaller, flat rather than rugged, brightly colored unlike the original, etc.), one that we can use to see where we are and where we are going.² It cannot and should not reproduce the original in every last detail. It merely needs to show what is relevant for the purpose at hand. A road map needs to show the roads and the towns, but not necessarily the landforms; in contrast, the landforms will be crucial to an engineer planning a road; he will need a very different kind of map from a motorist.

Science must *abstract* from reality. That means concentrating on particular variables, namely the ones that are judged relevant to the type of problem at hand, and omitting the rest. That is why science is never “realistic”. The most successful scientific theory ever produced, Newton’s mechanics, starts from three axioms that are not at all realistic, and are even somewhat counterintuitive (the principle of inertia, $f = ma$, and

the principle of action and reaction).³ This is not to say, however, that science doesn't have to respect some rules regarding its hypotheses. As in map making, there are certain rules that must be respected if we want the maps to be useful and relatively complete.

Naïve inductivism

A very common view of science is that it starts from unbiased observation of facts and then generalizes, by induction, to formulate universal laws about those facts, which in a subsequent step are generalized to build theories of a more general nature. The laws and the theories are then verified by comparing their empirical consequences with reality, including the initial observations. According to Mark Blaug [1980], this view, which (following Chalmers, 1976) we shall call "naïve inductivism", was the standard view of the philosophy of science in the mid-19th century. And yet, as early as the end of the 18th century, Kant and Hume had shown that this conception did not hold water. It is a sad fact of human nature that a point of view can survive (and not only until the mid-19th century, but on into the late 20th, albeit less widespread) even after someone has shown how ill-founded it is.

Let us show why its foundations are so shaky. Essentially, it boils down to the fact that none of the three premises of naïve inductivism (that science starts from observation, that it generalizes by induction, and that it then verifies) rests on a solid foundation.

We start with the first premise, which says that science starts from observation and that observation is a solid base from which to derive scientific knowledge. A historical anecdote will illustrate this point. The anecdote concerns Galileo's first observations of the planet Jupiter and its system of satellites, using his recently invented telescope. For Galileo, the satellites were real; his opponents, however, had serious doubts, and some even maintained that the satellites were "produced" by the telescope, which instilled more skepticism than confidence. They could not see what Galileo saw (through what were admittedly very rudimentary and unwieldy telescopes). In a way, they had the same difficulty seeing what we now consider obvious as any untrained person would have seeing what a trained radiologist can see in an X-ray. What for some was a fact, for others did not exist. Galileo himself, on discovering the rings of Saturn, went from initial incredulity to thinking that God was playing a joke on him, before he finally "saw" what we take today as "proven". In other words, in order to be able to look through a telescope and take what we see as an observation, we need to have and accept a certain theory of optics and telescopes, just as seeing the "facts" that appear in an X-ray requires a certain training (that is, a whole theory) about what X-ray machines do.

Kant [1964] expressed this idea in general terms in the “Critique of Pure Reason”:

“There can be no doubt that all our knowledge begins with experience... but it does not follow that it all arises out of experience ... for it may well be that even our empirical knowledge is made up of what we receive through impressions and of what our own faculty of knowledge (sensible impressions serving merely as the occasion) supplies from itself. If our faculty of knowledge makes any such addition, it may be that we are not in a position to distinguish it from the raw material, until with long practice of attention we have become skilled in separating it.”

The second premise is that science can use particular cases to formulate a general law. This has even less foundation than the previous one, as Hume [1874] showed a few years before Kant. The fact is that the principle of induction can have only two types of foundation: logical-deductive, or empirical-inductive. The logical-deductive option can be ruled out immediately, as only strong or complete induction, as used in mathematics, is acceptable. The empirical-inductive option, on the other hand, would require using precisely the principle we want to prove. That is, if we justify it by arguing that the principle of induction has been used on some occasions and has worked, we are taking for granted that it will always work, which obviously is not a solid foundation.

Russell [1959] illustrates this point with the story of the inductivist chicken. A chicken on a farm observes that the farmer appears at 8 a.m. on the dot every day to scatter some corn for him and his feathered friends. Having made a large number of such observations, the inductivist chicken decides he can generalize that the farmer will come every day at 8 a.m. to feed them. Since he has seen this happen every day since he was born, his hypothesis has been confirmed in 100% of cases. One day, he realizes there is a hole in the wire and thinks he might go out and see the world. But when he checks his watch, he sees it's five to eight, and decides to wait for the farmer to come and feed him first. But alas! It is December 24, and when the farmer comes, it is not to feed the chicken, but to wring his neck.

Russell notes that the chicken would have benefited from a more refined view of the uniformities of nature. That is, unless we understand why there is a certain constancy (or, to put it in terms more appropriate to management, unless we know the farmer's decision model), merely to know that he appears every day and scatters chicken feed is to know very little. And such slight knowledge may even be dangerous. Obviously, if that is all the information we have, it may be rational to use it; but we should be aware of how unreliable it is, even though it has been confirmed in 100% of cases.

Hence the Popperian rule of falsification, sadly ignored by many management researchers, as Christenson [1983] has clearly shown. To falsify is to try to disprove a hypothesis; it is done simply by presenting a counter-example (that is, an example that contradicts the theory). If a hypothesis is reasonable (only reasonable hypotheses advance our knowledge), falsifying it (which is logically possible) tends to be rather difficult, whereas “confirming” it (which is logically impossible) is extraordinarily easy, if by “confirm” we simply mean to add more examples of what the hypothesis assumes. It was extremely easy

for the inductivist chicken to add more and more data that “confirmed” his hypothesis (data accumulated automatically with every day that went by). But he would have found it substantially more difficult (he would have needed to change his system of observation, we might say) to add data that rejected the hypothesis. Yet that is precisely what would have been most useful to him.

A much less anecdotal illustration of the same issue is to be found in the development of astrology. The Egyptians actually knew quite a lot about astronomy. They had found an empirical relationship between the early morning appearance, just before sunrise, of the star Sirius (the brightest object in the firmament, apart from the Moon and a few planets) and the annual flooding of the Nile, on which their agriculture depended. Basically, a few days after Sirius appeared, the water levels would start to rise and the bountiful floods would begin. I do not know whether they actually established the causal link between the two phenomena (Sirius appeared at the beginning of summer, which is when the ice and snow on the mountains in Central Africa melts, feeding the Nile), but the inductivist reasoning that gave credibility to astrology is in any case easy to imagine. If the appearance of Sirius can predict an important event such as the flooding on which an entire people depends for its food, how can the stars not be able to predict trivialities such as the character of an individual, or his success in love? Throw in a few inaccuracies (the floods were obviously not predicted entirely accurately, given the unpredictability of seasonal temperatures) and already we have an excuse to believe practically anything that comes from the stars.

Sensible people know that astrology has no foundation. Counter-examples are easy to find for any of its propositions, even allowing for variations and inaccuracies. But it is important to bear in mind that very many people still read horoscopes regularly. Most newspapers, magazines and television channels (even the “serious” ones) publish them. Some astrologists have even become quite famous and presumably make a decent living out of it. If the criterion for scientific status were the amount of money a discipline attracts, astrology would come off quite well.

The third principle of naïve inductivism is the belief that, once formulated, theories can be verified. From the above analysis of induction it follows that, as a general rule, verification is impossible as it would mean, once again, accepting the principle of induction. Popper’s condition for something to be accepted as knowledge of reality (always provisional and capable of improvement) is that it be falsifiable, and that “serious” attempts to falsify it have failed.

Other naïve conceptions

Besides naïve inductivism (or naïve empiricism, as he calls it), Christenson identifies two other naïve conceptions that are common in the world of management: naïve pragmatism and naïve rationalism.

Naïve pragmatism is the excessive haste to obtain applicable results, and the conviction that the sooner the results are put to use, the better. Christenson agrees with Pareto that this obsession with premature practical applications hinders the progress of science, as does the insistence on telling people what they should do instead of observing what they actually do. And he adds that, obviously, observing what people do without a theoretical structure as a basis is naïve inductivism.

Lastly, naïve rationalism consists of thinking that the aim is to explain known phenomena, when that goal has not been achieved even in the natural sciences. Sophisticated rationalism, by contrast, considers the observed state of the world as only one of a number of possible states, and aims to discover the complete set of possible states and their logical relations, that is, the logical structure of the set of possible worlds.

What should research in management be like?

So far, I have tried to indicate some of the conditioning factors of “good” research. However, I recognize that I have been more negative than positive, in the sense that I have spent more time talking about what not to do than what to do. That is quite deliberate, of course. There are two very good reasons for it.

First, it is much easier in this area to say what is to be avoided than to recommend immediate courses of action. If research is essentially about generating ideas, knowing what needs to be done is almost as good as having done it; the initial idea is always the most difficult part. In certain fields of research, doctoral students sometimes take longer to find a suitable subject for their thesis than to actually develop it. Finding what to research and how is an integral part of the researcher’s task. In research, there are no easy recipes, though there are certainly dangers to be avoided.

Secondly, real research is something that must come from each individual researcher. One should “let the spirit blow”. Imposing a method, or even a subject, tends to be counterproductive. People are only capable of researching what interests them. I remember having read somewhere in Samuelson that in his youth he undertook an empirical research project; but when he found that the result of his efforts was that the marginal propensity to consumption was negative, he resolved never again to devote himself to this type of research. When I read it, I wasn’t sure whether it was serious or a joke, but in any case it makes no difference. Asking Samuelson to carry out empirical research is probably asking the impossible, although he is undoubtedly one of the (if not *the*) least questionable Nobel Economics laureates ever.

Nevertheless, there are some positive things that can be said, though undoubtedly we have to be more cautious. That is what I shall attempt to do in what follows, somewhat changing the tone to become more direct and personal and less academic.

First, we need to consider the purpose of research. Broadly speaking, research has two possible results: one is long term and uncertain, while the other is short term and certain. The first one is the result of the research itself. Sometimes there is a result, and sometimes there isn't. Sometimes it is important, and sometimes it isn't. And if it is important, it always comes after many years' work. The second, more short-term and more certain result is that research (if it is "good") improves the researchers themselves. It makes them think about their subject at a non-elementary level, it brings them into contact with critical colleagues, it makes them aware of their limitations, it encourages them to formulate their ideas more clearly, and it helps them to be more precise. First, they must discover for themselves what others have already discovered before them (reading what others have thought is not the same as *re-thinking* it yourself). And second, they must realize how difficult it is to keep walking when there is no road, when the road is made by walking. The humility to accept that our "brilliant" new idea is one that many others have had before us is something we acquire only by systematically putting things down in writing and showing them to someone else. It would be a serious mistake to think that of the two purposes of research just stated, the first is the most important. On the contrary, the second is more important than the first. If human progress is not as fast as it might be, it will simply have to be slower. That is nothing to worry about. What is important, however, is that every academic behaves professionally, and that consists essentially in keeping abreast of what everybody else thinks. And there is only one way to do that: to be forced to write down what one thinks, in a structured manner.

Obviously, that is a very demanding task. One of the basic reasons for learning is to satisfy demands. The international academic community has its standards, and the formal requirements are laborious. In an article on organizational research in Europe and America, Koza and Thoenig [1995] caricatured the image that many American researchers had of their European counterparts:

"For some US scholars, the profile of an average European researcher is of an individual who is not rigorous, wasting time with general ideas and unable to deliver cumulative knowledge outside very complex monographic studies. ... The lack of tenure pressure on the continent is responsible for parochial training and poor scholarship which could not pass traditional peer review."

This situation has changed a lot in the past few years, and Europe has adopted the American ways to a great extent. But Koza and Thoenig also presented the other side of the coin, and from there we might be able to reach some synthesis. In effect, they said that the European cliché is that US research is somewhat immature, due to the pressure for tenure, and that it suffers from the worst perversions of "normal science".⁴ This is probably true on the whole. That is precisely why we need to pay attention to methodology, which is the reason for the first part of this paper.

Unfortunately, in the last decades, supposedly rigorous research has lost a lot of relevance and the “perversions of normal science” have become worse, to the extent that Haack [1998] points out that nowadays researchers are often “fake researchers” or “sham researchers”. The fake researcher “is not primarily concerned to find out how things really are, but to advance himself by making a case for some proposition to the truth-value of which he is indifferent” – which is likely to happen given the incentive system that is applied to research (“publish or perish”). And the sham researcher is not “primarily concerned with finding out how things really are, but to make a case for some immovably held preconceived belief”.

Ghoshal [2005], in a well-known posthumous article, accused “bad management theories” of “destroying good management practices”. “Bad management theories” are those that are based on supposed rigor (good methods, perhaps), but on bad methodology: “the pretense of knowledge” [Hayek, 1989] of quantitative-econometric studies on the one hand, and the pessimistic view of human nature that implies that people are guided only by their self-interest on the other. This is bound together with the fact that the conventional academic research results are often unrelated to the “folk wisdom” that real-world managers are interested in for executive education [Pearce, 2004].

Thus, to make real progress in research, we would need an approach that is based on better methodology, one that cannot be qualified for instance as naïve empiricism, which is probably the problem that occurs most often. A mix of rationality, empiricism, and pragmatism is needed. In a nutshell, research should have, on the one hand, the standards of scholarship demanded in the United States, and on the other, the solid methodological foundations that American research sometimes lacks.

Notes

¹ In the late 19th century, some physicists, adhering to positivist principles, refused to accept the existence of subatomic particles, as they were not directly observable and were not available to immediate experience.

² This statement is not to be confused with the very well known statement by Milton Friedman [1953]. Friedman says that the less realistic a theory’s “assumptions” are, the better the theory; thereby espousing an instrumentalist position quite alien to the position of this author. What I argue here is that a theory cannot (and need not) take all the details of reality into account. Unlike Friedman, however, I contend that the closer a theory comes to including all the variables that are relevant to the type of problems it is supposed to resolve, the better it will be.

³ It is a remarkable fact that this hypothesis is still useful today, even though we know that it is not a good approximation to reality under certain circumstances (specifically, at very high speeds).

⁴ I assume they are referring to “normal science” in the Kuhnian sense.

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Abstract

The paper attempts to derive the conditions for obtaining good research in the field of management from a methodological point of view, i.e., from the point of view of the logical and empirical foundations of knowledge and the reasons to believe in the truth of any purported “scientific” proposition. After reviewing the essential concepts coming from classical philosophy and recent philosophy of science, the paper tries to apply them to management research. The paper draws heavily from the work of Charles Christenson (1976, *Proposals for a Program of Empirical Research into the Properties of Triangles*, *Decision Sciences*, 7, 631–648; and 1983, *The Methodology of Positive Accounting*, *The Accounting Review*, LVIII No.1, 1–22).

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