From counting in management to counting on management: making social science research matter

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Making social science research matter

Abstract

We have an unwavering faith in research substantiated by numbers. In the popular imagination, quantitative methods are still seen as the most robust and reliable means to inform decision making. The hegemony exercised by mathematical reasoning is succinctly captured in the statement, "If it can't be counted, then it doesn't count!" In this paper, I'd like to explore the assumptions underpinning the 'knowledge claims' made by mathematically informed reasoning. By teasing out the reasoning processes through which quantitative analysis proceeds, I shall circumscribe the explanatory boundaries of the knowledge claims it can make. I then reflect on the knowledge contributions of techniques reliant on mathematical reasoning towards management and speculate on how the loose ends within such research programs can be strengthened.

1.0 Introduction

It is an open secret that management research and scholarship, nurses a ‘physics envy’ (Ghoshal, 2005). By this, I refer to a disposition among a large swathe of management academics to value the pursuit of general, context-independent theoretical knowledge over the concrete, practical, context-dependent knowledge (Flyvbjerg, 2001, p. 66). Since Frederick Taylor’s persuasive call for a ‘scientific approach’ to management, the field has witnessed a proliferation in the use of mathematical modelling; and more recently computer simulations as tools to strengthen our knowledge on management. In their quest for predictable theories within management, modern descendants of Archimedes are still looking for that fulcrum on which they can rest the lever that is to move the whole world. Much like physicists, who armed with the certitudes of ‘laws’, undertake predictions of a natural phenomenon, social scientists crave for exactitude within their craft which would then allow them to predict and control a social phenomenon. The trouble within the social sciences, however, as Herbert Simon (1983) reminded us in a lucid commentary encapsulating the challenges, is the truism “no conclusions without premises” (p. 5). Put differently, within the ‘politics of meaning’: one of the objectives of social sciences after all is to explain, the meaning is context bound while the context itself is boundless.

The above truism requires a little more unpacking. For this I turn to a wonderfully illuminating ‘Metalogue’ authored by the intellectual maverick Gregory Bateson (1972, pp. 48-49). A metalogue is usually an imaginary dialogue between father and daughter where innocuous questions are used as vehicles to achieve transportation in thinking about seemingly simple ‘concepts’.

Metaloque: What Is an Instinct?

Daughter: Daddy, what is an instinct?

Father: An instinct, my dear, is an explanatory principle.

D: But what does it explain?

F: Anything—almost anything at all. Anything you want it to explain.

D: Don't be silly. It doesn't explain gravity.

F: No. But that is because nobody wants "instinct" to explain gravity. If they did, it would explain it. We could simply say that the moon has an instinct whose strength varies inversely as the square of the distance. .

D: But that's nonsense, Daddy.

F: Yes, surely. But it was you who mentioned "instinct," not I.

D: All right—but then what does explain gravity?

F: Nothing, my dear, because gravity is an explanatory principle.

D: Oh.

D: Do you mean that you cannot use one explanatory principle to explain another? Never?

F: Hmm . . . hardly ever. That is what Newton meant when he said, "hypotheses non fingo."

D: And what does that mean? Please.

F: Well, you know what "hypotheses" are. Any statement linking together two descriptive statements is an hypothesis. If you say that there was a full moon on February 1st and another on March 1st; and then you link these two observations together in any way, the statement which links them is an hypothesis.

D: Yes—and I know what non means. But what's fingo?

F: Well—fingo is a late Latin word for "make." It forms a verbal noun fictio from which we get the word "fiction."

D: Daddy, do you mean that Sir Isaac Newton thought that all hypotheses were just made up like stories?

F: Yes—precisely that.

The crux of Bateson’s argument is that all knowing is predicated on a speculation of the world. A speculation based on our *personal assumptions* of how we imagine our world. It is these imaginations which underpin the social science we build. As March (1999) writes, “The major claim to legitimacy by a social scientist is the claim that his procedures systematically evaluate the quality of his models and, thus, that his speculations are good ones.” (p. 307). For the most part, I think the claim is reasonable; but we need occasionally to examine the problems of evaluating our imaginations and the biases that confound our efforts to produce good speculations Put differently, we need to constantly re-mind ourselves of the ‘owing’ in our ‘knowing’.

Someone who understood the profound role played by imagination within theoretical speculation was the poet William Blake. Caught between theological dogma dispensed by the church and rational dogma which emerging science espoused, Blake was resigned to the necessity of Newton's artificially simplified concept, gravity, a seminal idea around which physical theory could take shape. But equally, he also understood its deceptiveness. In a letter to his patron and friend Thomas Butts, Blake wrote:

Now I, a fourfold vision see
And a fourfold vision is given to me
'Tis fourfold in my supreme delight
And threefold in soft Beulah's night
And twofold Always. May God us keep
From Single vision & Newton's sleep

—William Blake (1802)

Imagination therefore has a key role to play in revitalising the sciences by challenging the assumed ideas of an age. Whitehead describes such assumptions as the "assumptions which appear so obvious that people do not know that they are assuming them because no other way of putting things has ever occurred to them." (quoted in Trilling, 1976, p. 190). To scholars, then fall the responsibility of helping good ideas forged by their predecessors, find a new life in the imaginations of their successors (Cohen, 2012, p. 19). This paper is written, precisely in such a spirit. The remainder of this paper is organised into four sections. In section two, I venture into the meta-theory which underpins the 'technology of mathematics'. The goal here, is to examine some of the core assumptions underpin mathematical reasoning and in so doing, to circumscribe the knowledge claims it can make. Section three explores how mathematical thought has been translated and applied within management research. Here, the goal is to contextualise the impact of mathematical thought on management research and scholarship and to better illustrate the contributions as well as limitations of this analytical technique. In section four, I consolidate the insights which emerge from the analysis to argue for a switch in our understanding of rigour in research. Traditionally, rigour in research has been understood as precision in measurement and accuracy in data gathering. Here, my core argument is that whilst these are important, it is even more important to have 'precision' in thought. It is this reflexivity, I argue, that is key to more insightful research. I finally conclude by summarising the core arguments articulated along with its implications for the practice of research.

2.0 Speculation in Mathematical Thought

The management scientists, who are enthusiastically committed to their quantitative methods and to their principles, make the mistake of believing that, being scientists, they do not deal in assumptions, preferences and conclusions. There is a prevailing thought within this school that the lack of precise laws which are universal and timeless is because of the inadequate sophistication is quantifying data. Therefore, the prescription has been more mathematical models and more computer simulations to analyse systematic experimenting. What is not prescribed is to be out there within the thick of the action and to experience the unfolding of the social phenomenon under investigation. Those of us like me, (in the interest of full disclosure, I'm a trained engineer with a bachelor's degree in Electrical Engineering) emphasis was always placed on the reliability of mathematical formulations and systematic experimentation.

Social science in this sense, was perceived as a particular laggard. This was primarily attributed to the lack of computing capacity available to analyse the overwhelming complexity of data produced within the social phenomena. Now clearly, the argument goes, with rapid strides made within computing, it should be easier for us to crunch numbers and develop extremely dependable models of the social phenomena we are investigating. Such an approach would confer the legitimacy of the 'scientific bases' to our knowledge claims which we so deeply crave. This, as I shall argue in the sections which follow, is based upon a naïve understanding of what I'd like to call the 'technology of mathematics'.

The technology of mathematics is a beautiful and dignified abstraction. Its originality consists in the fact that within the mathematical sciences, connections between things are exposed, which besides agency in human reason, are extremely unobvious (Whitehead, 1925, p. 19). But it is a mistake to treat the technology of mathematics as inviolable to that ineradicable element of arbitrariness prevalent in human reasoning. Wiser people have said wiser things on this subject already and so my limited purpose here is to remind you of the significance of their ideas and to then identify some possible domains in which our current instincts might be prejudiced in significant ways. Here, I borrow from the writings of the mathematician turned philosopher Alfred North Whitehead (1925, pp. 19-37).

The technology of mathematics evokes in our minds, a science devoted to the exploration of numbers, quantity, geometry and in contemporary times, investigation into yet more abstract concepts of order, causality and into analogous types of purely logical relations. Consider for example the numbers seven and five. We could have seven cows, seven sandwiches, seven mangoes, seven colours and so on. Similarly for the number five, we have five fingers, five toes, five senses and so on. While the mere thought of a number, here the number seven or the number five does not evoke anything, paring it with an entity, cows, sandwiches, colours or senses, makes thinking about things possible. So while applying the technology of mathematics, we always get rid of particular instances and particular sorts of entities. So even though cows are very different from say, our senses, in the realm of mathematics, and here I mean pure mathematics, five in cows *is the same* as five in senses with the number five referring impartially to any group of five entities. In other words, no mathematical truths apply merely to cows, or colours or sandwiches or senses. Mathematical operations applicable to cows are the same as that can be administered on the senses, sandwiches or mangoes. In Whitehead's (1925) words,

> "So long as you are dealing with pure mathematics, you are in the realm of complete and absolute abstraction. All you assert is, that reason insists on the administration that, if any entities whatever have any relations which satisfy such and such purely abstract conditions, then they must have other relations which satisfy other purely abstract conditions" (p. 21).

The certainty of mathematics therefore, depends on its 'complete abstract generality' (Whitehead, 1925, p. 22). But we can have no a priori certainty that we are right in believing that the observed entities form a particular instance of what falls under our general reasoning. Therefore, in order to make an intelligible use of the technology of mathematics, three processes must be kept in mind.

First, the purely mathematical reasoning must be thoroughly scanned to make sure that there are no causal slips in it, no causal illogicalities due to failure in reasoning. This particular criterion is a vital premise underpinning mathematical reasoning but is particularly difficult to adhere to within the social sciences where causal relations are at best ambiguous. This point is succinctly illustrated in this conversation between a doctor and a management scientist which I've reproduced from an essay by William Starbuck (2004).

> "I told this doctor that I had been trying to create a computer program to make medical diagnoses because I wanted to improve medical care.
>
> He responded, 'But, diagnosis is not important to good medical care ... Good doctors do not rely on diagnoses.'
>
> 'But, medical schools teach doctors to translate symptoms into diagnoses, and then to base treatments on diagnoses,' I protested.

> 'That's right. Medical schools do teach that,' he conceded, 'but the doctors who do what they were taught never become good doctors. There are many more combinations of symptoms than there are diagnoses, so translating symptoms into diagnoses discards information. And there are many more treatments than diagnoses, so basing treatments on diagnoses adds random errors. Doctors can make more dependable links between symptoms and treatments if they leave diagnoses out of the chain.
>
> 'However, the links between symptoms and treatments are not the most important keys to finding effective treatments. Good doctors pay careful attention to how patients respond to treatments. If a patient gets better, current treatments are heading in the right direction. But, current treatments often do not work, or they produce side-effects that require correction. The model of symptoms–diagnoses–treatments ignores the feedback loop from treatments to symptoms, whereas this feedback loop is the most important factor.
>
> 'Doctors should not take diagnoses seriously because strong expectations can keep them from noticing important reactions. Of course, over time, sequences of treatments and their effects produce evidence that may lead to valid diagnoses.'" (pp. 1249-1250).

Such causal ambiguities therefore need to be kept in mind before mathematical reasoning can proceed. This brings us to the second process within mathematical reasoning. This is to "make quite certain of all the abstract conditions which have been presupposed to hold" (Whitehead, 1925, p. 22). In other words, the abstract premises from which the mathematical reasoning proceeds must be determined. This means that inferring through the principle of induction from mathematical reasoning cannot guarantee infallible general laws, without risk of error, from specific facts, even myriads of them. "No number of viewings of white swans can guarantee that a black swan will not be spotted next. Whether even a definite probability statement can be made about the colour of the next swan is a matter of debate, with the negatives, outnumbering the affirmatives" (Simon, 1983, p. 6).

The third process is that of "verifying that our abstract postulates hold for the particular case in question" (Whitehead, 1925, p. 23). This is problematic, even within science where 'facts' are gathered using instruments that are themselves permeated with theoretical assumptions. Take for example a microscope which is used to make observations. Is it possible to construct one without at least a primitive theory of light and optics?

Mathematical reasoning therefore gains its generalizability from the "fact that they are expressible without reference to those particular relations or to those particular relata which occur in that particular occasion of experience" (Whitehead, 1925, p. 24). But it is this very generalizability facilitated by the technology of mathematics which robs it of its relevance on that particular occasion of experience, a point of grave sensitivity to any social science. Let me illustrate this point using an example, I borrow from the writings of William James (1909/2011).

> "Consider a chemist who exercising his/her knowledge in mathematics tells us that two atoms of hydrogen and one of oxygen combine themselves of their own accord into the new compound substance 'water'. This is but an elliptical statement for a more complex fact. That fact is that when hydrogen and oxygen, instead of keeping far apart, get into closer quarters, say into the position H-O-H, they affect surrounding bodies differently, they now, wet our skin, dissolve sugar, put out fire, etc; which they didn't in their former positions. 'Water', is but our name for what acts peculiarly. But if the skin, sugar and fire were absent, no witness would speak of water at all. He would still talk of the hydrogen and oxygen distributively, merely noting that they acted now in the new position H-O-H." (p. 63).

Quality has thus been expressed in terms of a numerically determined quantity, two hydrogen atoms and one oxygen atom. This simultaneous generalization and abstraction of a particular property within an experience always more or less deforms the property by the extension it gives to it (Bergson, 1912/1999, p. 29). Such is the operation of mathematical reasoning. In sum, ensuring the elimination of causal slips and causal illogicalities due to failure in reasoning, belief in the prior supposition of the logic which drives inductive reasoning and ensuring that the quality of this reasoning process remains more or less stable within the particular case being investigated are all preconditions necessary for the triumph of abstract mathematical reasoning.

3.0 Mathematical Thought and Management Research

The assumption that the general conditions transcend any one set of particular entities is the ground for the entry of the notion of 'variable'. Also, just as numbers are abstracted from reference to any one particular set of entities, algebra allows abstraction from the notion of any particular number. In other words, algebra serves as what the Californian painter Robert Irwin would call 'a compounded abstraction' within the technology of mathematics. Its essence is best captured in one of Irwin's favourite maxims: 'seeing is forgetting the name of the thing seen' (quoted in Weick, 2006, p. 1726). Put simply, a concept used to explain a phenomena within science is nothing but an analytical abstraction performed by the scientist. The purpose of such an exercise is to 'black-box' a stream of experience for convenient later use. However, the indiscriminate use of one black box to explain another is a major intellectual vice within social science. Let me try and illustrate my point using an example narrative by Bateson (1972),

> "In ancient Rome, a candidate was once asked during his oral doctoral examination by the learned doctors on the "cause and reason" why opium puts people to sleep. The candidate triumphantly answers, "Because there is in it a dormitive principle!" Such reasoning is not uncommon to the statistical disciplines within management studies where a systematic record of the interaction between man and opium is observed, recorded and then given a fictitious cause, namely the "dormitive principle". The dormitive principle here, is now a compounded abstraction. Either the opium contains a reified dormative principle, or the man contains a reified need for sleep, which is "expressed" in his response to opium. And characteristically, all such hypotheses, generated through a blunt use of mathematical reasoning are "dormative"; in the sense that they put our critical faculty to sleep" (p. 17).

In order to create a mathematical model, one states a set of assumptions and then uses algebra to extract some implications of these assumptions. One can experiment with different assumptions until the model exhibits the properties one desires. Likewise, when one creates a computer simulation, one states a set of assumptions and the computer generates some implications of these assumptions. Since computers do nothing on their own initiative, simulations can only reveal the logical implications of what researchers believed before they created the simulations or what they assumed during the process of creating their models (Starbuck, 2004). "One does computer simulation because one does not know how to model one's theory mathematically" writes Startbuck (2004). He continues, "This might occur because one has little knowledge of mathematics, but it can also occur because mathematics is not capable of providing answers." (p. 1237). Because of this, problems must be quantified in a manner amenable to mathematical techniques and this poses a serious limitation on the applicability of operations research and management science techniques to social science problems (Simon, 1983, p. 91).

A second feature of quantitative reasoning is that a multitude of nonlinear, discontinuous, interacting assumptions has the potential to generate outputs that appear mysterious, even magical. Because simulations are process oriented, researchers have to specify activity sequences *apiori* even when they lack information about them thereby violating the "no conclusions without premises" tenet in general reasoning. Large, complex simulation models are virtually impossible to validate in detail. Even though such models are used within decision making for their predictive power, predictability is not the same as understanding and it is quite possible that it is possible without understanding (Boisot & Mckelvey, 2010). Boisot and McKelvey (2010) cite the Nobel laureate Richard Feynman who is said to have famously quipped 'Despite its remarkable predictive achievements, no one really understands quantum mechanics' (cited in Boisot & Mckelvey, 2010, p. 419). Lansing (2003) further emphasises this point when he writes:

> 'But if we shift our attention from the causal forces at work on individual elements to the behaviour of the system as a whole, global patterns of behaviour may become apparent. However, the understanding of global patterns is purchased at a cost: The observer must usually give up the hope of understanding the workings of causation at the level of individual elements. "The statistical method," wrote physicist James Clerk Maxwell in 1890, "involves an abandonment of strict dynamical principles" (Vol. 2, p. 253)' (p. 185).

Anderson (1999) too has cautioned against this tendency within mathematical modelling where scholars abstract away nonlinear interactions for the sake of analytical tractability, even though the emergence of pattern depends on such interactions.

Stated differently, computers generate outputs without explaining their reasoning. Researchers can add instructions to their programs that record calculation sequences but simulation programs typically incorporate so many microscopic steps that the explanations themselves pose serious data-analysis challenges. As a result, researchers are likely to end up with simulated behaviours that they cannot understand, a phenomenon termed the Bonini's Paradox. Starbuck (2004) elaborates the Bonini's Paradox as follows:

> 'As a model grows more realistic, it also becomes just as difficult to understand as the real-world processes it represents.' (p. 1238)

Since a model is built to demonstrate a causal understanding of organising processes; complexification of a model undermines the interdependences between subroutines by making it more, thereby rendering the model no easier to understand than the original causal process. Named after Charles Bonini's model which represented a hypothetical firm's detailed decision making as it decided how much to produce, what prices to charge, and so forth; within a short time, the model could generate many years of decision making, and allowed the researcher to vary elements of both the decision processes and the environment of the firm.

> "But as Charles's (1963, p. 136) wrote: 'We cannot explain completely the reasons why the firm behaves in a specific fashion. Our model of the firm is highly complex, and it is not possible to trace out the behaviour pattern throughout the firm ... Therefore, we cannot pinpoint the explicit causal mechanism in the model" (quoted in Starbuck, 2004, p. 1238).

In other words, compounding abstractions leads to a barrenness of understanding. In a tradeoff between understanding and prediction, good theories should always favour understanding.

A third often misguided critique offered by practitioners of quantitative techniques is their criticism of subjectivity in non-quantitative studies. This often takes the form of inadequate sample size, success bias in sampling, a 'mere case study', to state a few. Yet their own subjectivity is sometimes extreme. In the nature of our enterprise, a degree of subjectivity is inevitable. Intellectual safety would then seem to lie, not only in increasing the number of mechanical checks or in more rigorously examining those assumptions which had been brought to conscious formulation, but also in straight-forwardly admitting that subjectivity was bound to appear and inviting the reader to be on the watch for it (Trilling, 1976). This rarely happens. Take for example, reports by social scientists which routinely overstate the generality of their observations. In particular, researchers often conceal the ambiguity in their observations by focusing on averages and using hypothesis tests about averages to convert ambiguities into apparently clear conclusions.

Thus, instead of characterizing statistical findings by stating percentages such as '70 percent of adult men have dark hair,' researcher's state, test, and do not reject the hypothesis: 'Men have dark hair.' Then they describe such findings, since the measure is statistically significant by saying 'Men have dark hair' as if the description describes everyone or every situation (Starbuck, 2004, p. 1245).Thus truly absorbing studies in data and quantities may have the unfortunate effect of strengthening the 'validity claims' still more with people who are by no means trained to invert the process of abstraction and to put the fact back into the general life from which it has been taken. Mathematical systems have a greater sensitivity to initial conditions, which make their dependency and hence uniqueness, stronger making generalizations in social science extremely difficult. Furthermore, chaotic systems often exhibit recursive symmetries at different scale levels, which, as Tsoukas (2005) observes, mathematicians surprisingly approach with qualitative analysis methods (Voelpel & Meyer, 2006, p. 1566).

Finally I speculated that a similar phenomenon might occur with cross-sectional data for five broad reasons:

- First, a few broad characteristics of people and social systems pervade psychological data sex, age, intelligence, social class, income, education, or organization size. Such variables correlate with many behaviour and with each other.
- Second, researchers' decisions about how to treat data can create correlations between variables.

- Third, so-called ‘samples’ are frequently not random, and many of them are complete subpopulations even though study after study has turned up evidence that people who live close together, who work together, or who socialize together tend to have more attitudes, beliefs, and behaviours in common than do people who are far apart physically and socially.
- Fourth, some studies obtain data from respondents at one time and through one method. By including items in a single questionnaire or interview, researchers suggest to respondents that they ought to see relationships among these items (Starbuck, 2004, p. 1244).
- Fifth, and crucially, the role of time is underplayed in such models. While we all have to act in time, such models judge actions by timeless standards. The attitude is best summed by Bateson (1979, p. 63) when he quipped “if [t]he if…then of causality contains time”, then how can the “if… then of logic” be timeless?”

In sum, the technology of mathematics, as used within management, allows us to argue inductively from data to hypothesis but seldom do we treat the hypotheses against the knowledge derived by deduction from the fundamentals of science or philosophy (Bateson, 1972, p. xxv). “We seek scientific rationality because it pleases our minds”, writes Starbuck (2004), “but what gives our minds pleasure may not give us insight or useful knowledge” (p. 1239). Such reasoning as described, usually consists in passing from concepts to things, and seldom from things to concepts. This I believe is the Achilles heel of developing theories with mathematical reasoning.

4.0 From Precision of Measurement to Precision of Thought

Any research exercise involves a trade-off between simplicity, accuracy and generalizability (Weick, 1979). Any theory can therefore simultaneously be simple and accurate, simple and general or general and accurate but not all three at once. Our fixation with the need to be general has had an adverse impact on the simplicity and accuracy of the phenomena we theorise about within organisations. The result of mindlessly sticking to these formulaic notions of ‘rigour’ is what Chia (2014), in a recent article calls ‘a resultant *rigor mortis’*, by which he means, ‘an intellectual ‘stiffness’ of the mind that discourages any kind of speculative conjecturing including especially the initial capacity to gloss over long stretches of incomprehension and to focus on only those aspects that appear immediately appealing or promising’ (p. 684).

In order to arrest this *rigor mortis* which is setting into management research, where theorists are more keen on tracking tractable rather than relevant problems (Weick, 1989), I suggest we move to a more simple and accurate mode of theorising.

A misguided anxiety about the need to be general takes the form a scornful quip about "What insights might be gained from a 'mere case"?" Van Maanen puts it well when he writes "The smart-ass but wise answer to this hackneyed but commonplace question is 'all we can'" (Van Maanen, 2011, p. 227). "If we are concerned about the imprecision of single site studies as research data", then as Simon (1991) remarked, "we can console ourselves by noting that a man named Darwin was able to write a very persuasive and perhaps even correct book on the origin of species on the basis of precisely such a study of the Galapagos Islands and a few other cases. To the best of my recollection, there are no statistics in Darwin's book" (p. 128).

Cultivating empirical sensitivity through detailed, theoretically informed, philosophically grounded, reflexive research, in other words, is crucial. Specificity of findings in management research, therefore is as, if not more important than generalizability. Is there any point at all in pursuing mathematically generalizable findings on a phenomena which is statistically significant across several organisations but is not applicable (at least without severe modifications) within a single one of these organisations? Several years ago, when Erving Goffman, was criticised for being too specific and too ready to wrap a concept around every situation he analysed, his blunt yet eloquent response was 'it is better perhaps (to have) different coats to clothe the children well than a single, splendid tent, in which they all shiver' (Goffman, 1961, p. xiv).

The need of the hour is more coats and lesser tents. As regards validity, such immersed research is based on interpretation and is open for testing in relation to other interpretations and other research. However one interpretation is not just as good as another, which would be the case for relativism. Every interpretation must be built of claims of validity, and the procedures ensuring validity are as demanding for such detailed research as for any other activity in the social sciences (Flyvbjerg, 2001, p. 130).

5.0 Conclusion

In this essay, I have attempted to explore the meta-theory of mathematical reasoning. The main inference I draw is that the social sciences are being inundated with statistically significant, but meaningless noise, supposed ‘findings’ that say nothing of lasting value, but enable researchers to publish a multitude of articles (Starbuck, 2004, pp. 1245-1246). I’m sure that this conclusion “will disconcert both those who reflect on the social sciences without practicing them and those who practice them without reflecting them” (Bourdieu, 1977/2002, p. vii). Mathematical reasoning is both necessary and useful as a technique to speculate *if* a phenomenon is happening. It is a rather blunt tool to infer *how* and *why* a phenomenon unfolds. Consider a simple word "democracy," for example. It is an abstraction from the experience of what it means to lead a free life within a nation state. We could now attempt to explain democracy by operationalising a scale of freedom constituted by dependent and independent variables and their variant relationships. Or we could explain it by describing its elements such as a free press, independent judiciary, due process, elected representatives and so on. Which of these would be a more meaningful explanation?

In pursuing our purpose, and making our abstractions, we must be aware of what we are doing; we ought to have it fully in mind that our abstraction is not perfectly equivalent to the infinite complication of events from which we have abstracted (Trilling, 1976, pp. 188-189). Mathematics is only the science of magnitudes, and mathematical processes are applicable only to quantities, but as Henri Bergson (1912/1999, p. 52) reminds us “it must not be forgotten that quantity is always quality in a nascent state; it is, we might say, the limiting case of equality.”

I’d like to conclude with yet another anecdote, narrated by James March (1999),

> “Several years ago, there was a well-known Californian child psychologist who at the end of each of her talks would invariably be asked: “Mrs Gruenberg, do you really mean that we should never spank our children?”; to which she would reply, “Well, I suppose, if you keep reminding yourself every moment that you should never ever spank your children, you will end up spanking them just about the right amount.” (p. 359).

Before I’m accused of endorsing physical child abuse, let me distance myself from the notion of spanking children. But like Mrs Grunberg’s precepts, it is my hope that with this brief essay, I’ve been able to convince you all to commit to a more sparing, yet rigorous and reflexive use of the ‘mathematical technology’ within organisational studies. Such a renewed focus, I believe would not just arrest but potentially reverse the ‘publish as we perish’ (Alvesson & Gabriel, 2013, p. 246) trend in management research, by making it relevant to practise.

References

Alvesson, M. & Gabriel, Y., 2013. Beyond Formulaic Research: In Praise of Greater Diversity in Organizational Research and Publications. *Academy of Management Learning & Education,* 12(2), p. 245–263.

Anderson, P., 1999. Complexity Theory and Organization Science. *Organization Science,* 10(3), pp. 216-232.

Bateson, G., 1972. *Steps to an Ecology of Mind.* San Francisco: Chandler Publishing Co.

Bateson, G., 1979. *Mind and Nature.* New York: Dutton.

Bergson, H., 1912/1999. *An Introduction to Metaphysics.* Indianapolis/Cambridge: Hackett Publishing Company.

Blake, W., 1802. Letter to Thomas Butts. In: *Letters of William Blake.* s.l.:s.n., p. 79.

Boisot, M. & Mckelvey, B., 2010. Integrating Modernist and Postmodernist Perspectives on Organizations: A Complexity Science Bridge. *Academy of Management Review,* 35(3), pp. 415-433.

Bonini, C. P., 1963. *Simulation of information and decision systems in the firm.* Englewood Cliffs, NJ: Prentice Hall.

Bourdieu, P., 1977/2002. *Outline of a Theory Practice.* Cambridge: Cambridge University Press.

Chia, R., 2014. Reflections on the distinctiveness of European management scholarship. *European Management Journal,* Volume 32, p. 683–688.

Cohen, M. D., 2012. The Garbage Can Model of Organizational Choice: Looking Forward at Forty.. In: *Research in the Sociology Vol 36.* Bingley, UK: Emerald Books.

Flyvbjerg, B., 2001. *Making Social Science Matter: Why social inquiry fails and how it can succeed again.* Cambridge: Cambridge University Press.

Ghoshal, S., 2005. Bad Management Theories Are Destroying Good Management Practices. *Academy of Management Learning and Education,* 4(1), pp. 75-91.

Goffman, E., 1961. *Asylums.* New York: Anchor.

James, W., 1909/2011. *A Pluralistic Universe.* London: CreateSpace Independent Publishing Platform.

Lansing, J. S., 2003. Complex Adaptive Systems. *Annual Review of Anthropology,* Volume 32, pp. 183-204.

March, J. G., 1999. *The Pursuit of Organizational Intelligence.* Malden, Massachusetts: Blackwell.

Simon, H., 1983. *Reason in Human Affairs.* Stanford, California: Stanford University Press.

Simon, H. A., 1991. Bounded Rationality And Organizational Learning. *Organization Science,* 2(1), pp. 125-134.

Starbuck, W. H., 2004. Why I stopped trying to understand the Real World. *Organization Studies,* 25(7), pp. 1233-1254.

Trilling, L., 1976. *The Liberal Imagination: Essays on Literature and Society.* New York: Charles Scribner's Sons.

Trilling, L., 1976. The Sense of the Past. In: *The Liberal Imagination.* New York: Charle Scribner's Sons, pp. 181-197.

Tsoukas, H., 2005. *Complex Knowledge: Studies in organizational epistemology.* Oxford: Oxford University Press.

Van Maanen, J., 2011. Ethnography as Work: Some Rules of Engagement. *Journal of Management Studies,* 48(1), pp. 218-234.

Voelpel, S. & Meyer, J., 2006. Haridimos Tsoukas: Complex Knowledge: Studies in Organizational Epistemology. *Organization Studies,* 27(10), pp. 1563-1569.

Weick, K. E., 1979. *The Social Psychology of Organizing.* 2nd ed. s.l.:Addison-Wesley.

Weick, K. E., 1989. Theory Construction as Disciplined Imagination. *Academy of Management Review,* 14(4), pp. 516-531.

Weick, K. E., 2006. Faith, evidence, and Action: Better Guesses in an Unknowable World. *Organization Studies,* 27(11), pp. 1723-1736.

Whitehead, A. N., 1925. *Science and the Modern World: Lowell Lectures,1925.* Cambridge, MA: The Free Press.