

The triumph of nonsense in management studies

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TITLE:**The triumph of nonsense in management studies****ABSTRACT**

More management scholars than ever are expressing concern about the state of our field. Complaints include the suggestion that many publications are written purely to further our careers rather than to advance knowledge. In this article, I argue that our discipline is in crisis. We neglect really important issues in favour of bite sized chunks of research that are more likely to find quick publication in leading journals. Given that such journals insist on the primacy of theory development, we also increasingly resort to pretentious and long-winded prose to at least create the illusion of ‘theory development.’ The result is writing that can only be understood by those already on the inside of the debates it references. As opposed to ‘imposter syndrome’, in which qualified people doubt their suitability for a given job, I suggest that many of us have become ‘genuine imposters’ in that we pretend to be doing more important work, and more competently, than we really are. Finally, I propose changes in our mind-sets and journal practices to restore some sense of deeper purpose to what we do.

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INTRODUCTION

In this paper, I criticize how management scholars write and what we write about. I argue that the growing emphasis on publishing in ‘top’ journals has reinforced the already strong fetish of theorizing for its own sake (Hambrick, 2007). While there is obviously much to be said for good theory, the need to ‘develop theory’ has become a condition of publication by our elite journals to a greater extent than can be found in other disciplines. There is no parallel expectation that we should address important phenomenon, write well, address multiple constituencies or seek to impact on practice. To a large extent, work produced by this mind-set ‘benefits no one but the authors’ (Lambert, 2019, p. 383). For example, our key journals have published very few papers that address the role of management in the Great Recession of 2008.

Rather, we have a profusion of theorizing that sidelines major issues and which only those already familiar with the issues discussed can understand. Since developing new theory is difficult, and therefore rare, many papers project only the *illusion* of theory development. More us are becoming what I term ‘genuine’ imposters in that we pretend to be doing something we are not – that is, developing compelling new theories that matter. This requires authors to adopt a tortured writing style characterized by vagueness, euphemisms, and long words where shorter ones are readily available. I suggest that this represents a crisis of understanding about the purpose and value of our work that we should urgently address. While many of these problems have been with us for a long time, there is a growing sense that they are getting worse. I also argue that the prolonged and excessively critical nature of the peer review process intensifies the problem. Finally, I make some suggestions about what we can do to restore more meaning and purpose to management research.

DEVELOPING THEORY AND ‘THE ANXIETY OF INFLUENCE’

To a far greater extent than can be found in other disciplines, major management journals insist that papers must make a contribution to theory. The *Academy of Management Journal* is

typical in its insistence that: ‘All articles published in the *Academy of Management Journal* must also make **strong theoretical contributions**. Meaningful new implications or insights for theory must be present in all *AMJ* articles, although such insights may be developed in a variety of ways (e.g., falsification of conventional understanding, theory building through inductive or qualitative research, first empirical testing of a theory, meta-analysis with theoretical implications, constructive replication that clarifies the boundaries or range of a theory).’¹. The status of *AMJ* and journals like it means that they have a high desirability for authors, who of course desperately follow editorial injunctions in the hope of being published. In addition, other journals who covet the same status are tempted to copy the standards of elite journals in the usually forlorn hope of joining them. These isomorphic pressures exert enormous pressure on our publishing practices.

For authors, this means that if you use an existing theory to explain an interesting phenomenon your work will be rejected because it ‘doesn’t develop new theory,’ however suitable it is. This has numerous unfortunate effects. Here is one. Bloom (1973), a literary critic, pointed out that poets are inevitably inspired by reading the works of others, but argued that the more such work influences them the more likely it is that their own will be derivative. There is, simultaneously, the need to do something new, but a sometimes paralysing fear that this is impossible. He termed this ‘the anxiety of influence.’ In our field, researchers face similar dynamics from the outset of their careers. We are confronted with a mountain of existing theories, but are still ‘encouraged’ to create new ones. Many of us seek to cover up our anxiety about this by writing overblown nonsense, frequently in the style of some authority figure whose work is held up as a master lens that can be used to understand all the world’s problems. Amateur philosophising often substitutes for genuine critical thinking.

¹ See <http://aom.org/Publications/AMJ/Information-for-Contributors.aspx>, accessed 25th July 2019. The emphasis is in the original.

We thus have a three-fold paradox – insistent calls for more theory, big claims that major theoretical advances are being made, and simultaneously a growing recognition that in fact really radical new theories are rarely forthcoming (e.g. Clark and Wright, 2009). The pressure to produce more theory interacts with the career ambitions of academics to produce a great deal of faux theorising. As a result, our journals are clogged with the endless elaboration of minor issues that claim to be building theory, but which are of little interest to most readers and that are incomprehensible to the wider public (Gabriel, 2010; Alvesson and Gabriel, 2013). As Hopwood (2009, p. 517) witheringly puts it:

‘... so much of the theorising seems to be a repetition of existing theoretical work, summarizing time and time again what is already known and usually doing more to add to the publication listings of the authors than to advance wider understandings of the managerial craft and its organizational and societal contexts.’

I am not disputing that good theory is important. Without it, we would be left with a series of observations but lack explanations for how they relate to each other. However, I am arguing that theory development has become an unhealthy obsession in our discipline. It is rare to find a convincing rationale for this. Elite journals, and those that covet elite status, take it as self-evidently important that developing theory should take pride of place in the long list of things that a journal article can do. The result is some dreadful writing, seemingly intended to disguise the pointlessness of what is going on.

NONSENSE WRITING DISGUISED AS BUILDING THEORY

The famous American sociologist, C Wright Mills’s (1959) influential book *The Sociological Imagination* included a chapter that was highly critical of what he called ‘grand theory.’ Mills characterised this as ‘the associating and disassociating of concepts’ (p. 26), and wrote of it as follows:

‘The basic cause of grand theory is the initial choice of a level of thinking so general that its practitioners cannot logically get down to observation. They never, as grand theorists, get down from the higher generalities to problems in their historical and structural contexts. This absence of a firm sense of genuine problems, in turn, makes for the unreality so noticeable in their pages. One resulting characteristic is a seemingly arbitrary and certainly endless elaboration of distinctions, which neither enlarge our understanding nor make our experience more sensible. This in turn is revealed as a partially organized abdication of the effort to describe and explain human conduct and society plainly.’ (p. 33)

To illustrate his case, Mills took various passages from the influential work of Talcott Parsons, then the doyen of American sociology, and translated them from academese into English. He suggested that the 555 pages of his book *The Social System* could be turned into about 150 pages of straightforward prose. The effect was like watching a hot air balloon slowly deflate. In Mills’s view, the faults he highlighted weren’t just a personal peccadillo. Rather, it was a means of avoiding having to deal with such issues as power, since it was more comfortable to retreat into a world of arcane abstractions that created the impression of intellectual substance simply because few understood it.

The quality of much academic writing is equally terrible today. Most papers in mainstream journals are formulaic, cautious, dull and unreadable. Writing by critical management scholars is little better, and indeed is often worse. As Grey and Sinclair (2006, p. 445) put it:

‘Much of the writing in our field is tendentious, jargon-ridden, laboured. Partially, this seems to us to stem from attempts to make our writing academically legitimate, as if describing and explaining organizational lives must invest itself with a language quite different to those lives. As if, in rejecting positivism, we feel obliged to adopt an equally abstracted vocabulary so as to show that we are, after all, still serious scholars.’

In such work, bonus points are awarded if you can find a French philosopher that no one has ever heard of, the deader the better, and fashion a claim that the implications of their writings for management studies has been ‘unjustly neglected.’ Some examples are useful. Consider the abstract of ‘Strategy as practical coping: A Heideggerian perspective’ (Chia and Holt, 2006):

‘Specifically, we argue that the dominant ‘building’ mode of strategizing that configures actors (whether individual or organizational) as distinct entities deliberately engaging in purposeful strategic activities derives from a more basic ‘dwelling’ mode in which strategy emerges non-deliberately through everyday practical coping. Whereas, from the building perspective, strategy is predicated upon the prior conception of plans that are then orchestrated to realize desired outcome, from a dwelling perspective strategy does not require, nor does it presuppose, intention and purposeful goal-orientation: strategic ‘intent’ is viewed as immanent in every adaptive action’ (p.635).

So far as I can understand it, this means that instead of sticking to rigid plans managers often wing it on the hoof. True – but we know that already. The authors attempt to show that Heidegger is relevant to their view of strategy as practice, and in doing so segue into Bourdieu’s notion of habitus:

‘By introducing Heidegger’s distinction between the building and dwelling modes of engagement to the strategy-as-practice literature, we are able to begin to explore further why it is that individual and organizational practices may exhibit sufficient consistency for constituting identities and strategies without recourse to *intentionality* and the presumption of the existence of conscious mental states. Yet, in emphasizing the primacy of dwelling, we do not imply a blind obedience to orthodoxy, or conformity to internalized structural forces; actors have more than adequate room for manoeuvre, because what the dwelling mode of engagement precipitates are unconscious *predispositions* to act in a manner congruent with past experiences but to do so in a manner that ensures the smooth and uninterrupted

adjustment to local situations. It was this question of how to account for a kind of ‘absorbed intentionality’ from within practices that led the French sociologist Pierre Bourdieu to extend Heidegger’s work to the study of the material structuring of human action within socio-cultural contexts. Bourdieu’s work must therefore be understood within the context of Heidegger’s insights’ (p. 645).

With all due respect to our eminent authors, I still don’t think this really adds anything to the notion that managers often improvise rather than stick to rigid plans. Mintzberg (1994) made essentially the same point in his award winning book *The Rise and Fall of Strategic Planning*, and in which we find not a single reference to either Bourdieu or Heidegger. It seems that many of us feel compelled to make our theories more complex than they really are to show off how clever we are. Such intellectual exhibitionism might just be achieving the opposite.

Thus, a bombastic style, starved of metaphor, wit or irony has become commonplace. *Administrative Science Quarterly* published a paper entitled ‘Imprint-environment fit and performance: How organizational munificence at the time of hire affects subsequent job performance’ (Tilcsik, 2014). ‘Munificence’, not a word I encounter every day, simply means largesse, benevolence or generosity, among a variety of other synonyms. But to use a more familiar word would immediately make the title more comprehensible and weaken the paper’s claim to be developing theory. Now read its abstract:

‘The core hypothesis, supported by the results, is that the more similar the initially experienced level of organizational munificence is to the level of munificence in a subsequent period, the higher an individual’s job performance. This relationship between what I call “imprint–environment fit” and performance is contingent on the individual’s career stage when entering the organization and the influence of secondhand imprinting resulting from the social transmission of others’ imprints. A possible implication of the core hypothesis may be a “curse of extremes,” whereby both very high and very low levels of

initial munificence are associated with lower average performance during a person's subsequent tenure. One mechanism underlying these patterns is that employees socialized in different resource environments develop distinct approaches to problem solving and client interactions, which then lead to varying levels of imprint– environment fit in subsequent resource environments' (p.639).

I would translate this as follows: 'When managers behave well with new employees, and continue to do so, they work better, and when they behave badly the opposite happens. This is partly because already existing employees also behave either well or badly and so role model attitudes and behaviour for others.' 'Imprinting' seems to mean the impact on us of our experiences with others. But since this doesn't sound sufficiently theoretical, we read instead of 'imprint–environment fit.' As Billig (2013, p. 51) observes, sometimes big words are used 'not to identify a discovery, but to cover over a lack of discovery.' Note also the reference to 'supported by the results.' This means that some hypotheses which stated the obvious were developed, and, in the manner of someone hypothesising that happy people are happy because they are happy people, they were duly confirmed.

It is hard not to agree with Van Maanen (1995, p. 139): 'I am appalled at much of organization theory for its technocratic unimaginativeness. Our generalizations often display a mind-numbing banality and an inexplicable readiness to reduce the field to a set of unexamined, turgid, hypothetical thrusts designed to render organizations systematic and organization theory safe for science.' This is still the state of theory in our field today. I think it is even worse than when Van Maanen reached the dismal conclusion that I have just quoted.

Weick (1989, p.516) suggested that 'Theorists often write trivial theories because their process of theory construction is hemmed in by methodological strictures that favour validation rather than usefulness.... These strictures weaken theorizing because they de-emphasize the contribution that imagination, representation, and selection make to the process, and they

diminish the importance of alternative theorising activities such as mapping, conceptual development, and speculative thought.’ He is highlighting how the norms of writing a journal article for publication violate the actual processes whereby valuable insights are really generated. Most papers follow what is now a very tired formula – Introduction, literature review, methods, results and discussion, written to fairly standard lengths. There is little room for the reporting of inspiration, luck, happy accidents or the exercise of what Weick (1989) has called ‘disciplined imagination.’ An impersonal writing style is adopted, in which we find expressions such as ‘this paper argues’ rather than ‘I think.’ One might imagine that they are written by a computer rather than a human being. Come to think of it, when you consider how much of quantitative papers consist of tables auto-generated by SPSS, and how many ‘critical’ papers seem to just cut and paste obligatory sets of references, this might not be so far from the truth. This helps authors achieve their primary goal – publication. What it doesn’t do is produce meaningful theories that offer useful insights into the world of organizations.

These problems seem to become stronger the more that researchers draw on grand theory taken from the likes of Jacques Lacan, the deceased French psychoanalyst. Billig (2013, p. 4) is among many critics of Lacan’s famously impenetrable style of writing, arguing that he ‘was an obscure writer, who seemed to delight in making things difficult to grasp, offering few examples to illustrate his allusive points.’ Chomsky described him as ‘a total charlatan, just posturing before the television cameras the way many Paris intellectuals do. Why this is influential I haven’t the slightest idea.’² But influential he is, including within our field. I have found forty-four papers in the journal *Organization Studies* and seventy-four in its more critically oriented counterpart, *Organization*, that favourably cite his work. The latter felt that Lacan’s influence was important enough to justify a special issue devoted to his influence on our field. Its introductory editorial begins thus:

² See www.openculture.com/2013/06/noam_chomsky_slams_zizek_and_lacan_empty_posturing.html, accessed 23 July 2018.

‘Lacan, of course, does not exist. By this we do not mean to say that he died, in 1981, but rather to say that when we say ‘Lacan’, we are using a language, or more precisely a signifier, and that signifier is caught up in a chain of other signifiers. This might be obvious to us now, but this has not always been so.’

Obvious is not an adjective that I would use at this stage. Ploughing on, we read as follows:

‘This is perhaps only obvious to us *because* of Lacan, or because of the things that we here condense by the name Lacan. This calling into being of a single signifier that stands in the place of a subject is one of the key illusions that Lacan shows us, again and again. It is also the kind of stupidity that we have started to sense, throughout the 20th century, that results from any effort to say ‘I’ (Contu, Driver and Jones, 2010, p. 307-8).

Nor is the editorial from which I have just quoted the most impenetrable example to be found. I picked a Lacan themed paper at random from *Organization Studies*, and the first sentences that caught my eye were these:

‘According to him, one of the key functions of fantasy (belonging to the imaginary) is to veil the lack constituted by the failure of symbolization (in the symbolic) in order to sustain the illusion of wholeness and to avoid the anxiety resulting from the emergence of the real. Fantasy is a narrative structure whose content can greatly vary, but which rests on the imaginary promise of recapturing what has been lost’ (Vidaillet and Gamit, 2015, p. 992).

This is a shame, since the paper deals with an important issue – how employees in a factory responded to the threat of closure. It contains many pieces of brilliant writing and has fascinating data. But the good writing appears when Lacan isn’t on stage. When invoking The Master, his disciplines seemingly feel obliged to mimic his writing style, thereby showcasing ‘the anxiety of influence’ in our own field.

It is initially baffling that people choose to write this way. Pinker (2014a, p. 59) suggests that much of it derives from what he terms ‘the curse of knowledge’: that is, the ‘difficulty in

imagining what it is like for someone else not to know something that you know.’ A much less flattering explanation, and one that I find more plausible, is given by Gross and Levitt (1994, p. 73). Describing the arcane mysteries of postmodernism, they argue that such writing offers ‘the possibility of becoming an initiate, part of an elect whose mastery of a certain style of discourse confers an insight unobtainable elsewhere and authorises a knowing (and often smug) attitude.’

However, this isn’t only a personal failing on the part of academics. I now suggest that the vagaries of the peer review system that predominates in the management field make these problems worse.

HOW THE REVIEW PROCESS DAMAGES OUR WRITING

The process of submitting one’s work to many journals now forces us to traverse a formidable obstacle course. As Moosa (2018, p.127) has argued, referees tend to believe that for their reviews to ‘count’, ‘they must find something wrong in the papers they review.’ This means that no paper, however brilliant, will be accepted outright for publication. Even if they have already appeared in a top tier journal, the chances are that if they were anonymously resubmitted they would be expected to go through more revisions, or might even be rejected. Peters and Ceci (1982) demonstrated as much in an admittedly small but famous study where they resubmitted twelve psychology papers to journals that had previously accepted them. While three of the papers were recognised, the rest went out to peer review. This time, sixteen out of eighteen reviewers recommended rejection. They provided devastating critiques of the methodological and theoretical shortcomings of the papers in question. The editors agreed, and rejected eight papers that their journals had already published. I see no reason why we should expect different outcomes in our field.

We are required to dodge the bullet of desk rejection, overcome the hurdle of reviewer comments, and crawl through a long process of multiple revisions – all the while profusely

thanking the editor and reviewers for ‘their invaluable feedback.’ Papers become bloated by lengthy, pointless and boring literature reviews. Authors try to anticipate every possible demand from every conceivable reviewer; reviewers are asked to suggest improvements, whatever the quality of the paper in front of them, so they do; and editors rarely call time when a paper is good enough to publish. Many of us resort to ever more convoluted writing in order to convey the impression of methodological rigour and theory development.

Does this sound like over-statement? If so, consider the testimony of Seibert (2006, p. 203), discussing a paper that he co-authored in the *Academy of Management Journal*. It won the journal’s outstanding paper of the year award for 2004. Seibert reports:

‘...the first set of editor-reviewer comments covered 13 pages of single-spaced text. The R&R was characterised by the action editor as “high-risk.” Our reply consisted of 31 pages of single-spaced text. The second set of editor-reviewer comments covered 10 pages; our second reply was 13 pages. I can literally say that we invested as much work in the replies to reviewers as we did in writing the original manuscript.’

In one way, the dedication of Seibert and his co-author to the task in hand is admirable, and it important to note that he framed his commentary on the process in wholly positive terms. The subtitle of his reflective piece says it all: *Reviewers are an author’s best friend*. From another standpoint, a covering letter of 31 pages is insane, albeit a form of insanity that is coerced. Submitting a paper to a top journal seems akin to becoming a hostage, with rejection (i.e. termination) the ultimate sanction for disobedience. Authors are desperate to escape this fate, but can only do so if they bow to each and every idiosyncratic reviewer request/ demand, while expressing fulsome thanks for the pain that is being inflicted on them. They can either seethe in silence or fall victim to Stockholm syndrome, where the review process starts out badly but by the end they quite like it. I am not denying in the least that peer review often adds value. But

I am arguing that when it becomes this prolonged and ferocious it can destroy all life, passion and individuality in a submission before it sees the light of day. Authors may feel that they have ended up writing what they don't want to write, including claims to be creating theory, in order to satisfy reviewers and build their careers.

Cederström and Spicer (2017, p. 708) give a vivid account of how common this has become. They describe working on a paper that was submitted to several journals and underwent multiple revisions. It was eventually published - ten years after they began working on it. Rather than feeling satisfaction at this achievement they write as follows:

'Was it better than our first version? We couldn't tell. All intellectual excitement had been beaten out of us. It no longer felt like our own words. It was now just an assemblage of what the reviewers wanted us to write, thinly disguised as an academic article... It felt like some form of double captivity. Authors feel as if they're trapped, destined to write things they don't find interesting. And reviewers and editors feel the same way, forced to comment on papers they find pointless. Hundreds of hours were spent writing, and dozens of hours in reviewing and editing. For what?'

Inevitably, these practices distort how we write. Cornelissen et al (2012) corresponded with authors who had published qualitative papers in prominent US journals. Many reported that there was often 'a disconnect between the actual conduct of qualitative research and the way the study is framed, written up and represented in published articles' (p. 192). While this was often related to the reporting of reliability, validity and inter-rater reliability it suggests that reviewers and editors pressurise authors to conform to norms of writing that are currently in vogue and that they actually dislike. These norms include a penchant for over-theorising. Behaving pre-emptively, many of us now feel that we have to write in a convoluted style when submitting articles to journals, in the generally futile hope of easing reviewer criticism.

Overblown writing has become a Pavlovian response to our perverse system of rewards and punishments. I offer the following example:

‘Essentially, organizations do not exist independently of communication since it is through communication that they find entitative form... it is primarily leaders who claim the entitative status for those organizational structures that institutionalize their role as leaders. But this only works to the extent that the entitative claim is recognised and responded to by others. Anyone can claim leader status in an organization, but this claim will only acquire agency if it is granted, however reluctantly, by others... Leadership is therefore a communicative process whereby agents claim entitative status for emergent social structures. Moreover, without such claims being made, negotiated and formalised there would be no over-arching organizational entity within which leaders emerge from leadership processes.’

My translation of this is as follows:

‘Unless people communicate clearly with each other they can’t form organizations. As organizations emerge, some people assert their right to become its leaders. But they must negotiate with others to see if this is accepted. If they don’t persuade enough people to regard them as a leader then they can’t become one.’

‘Entitative’ simply means something that has now become real. Academia trains us to make simple ideas more complex than they are in an attempt to be taken seriously, during the peer review process and after. In this example, that is precisely the author’s strategy. I should know, because the author is myself (Tourish, 2014, p. 86). Worse still, I did not need to write in such a pretentious way for the journal concerned³. But by this stage a noisy style had become my default position, like a dog trained to bark for attention. In self-defence, I would argue that this isn’t the worst piece of academic writing that you can find. But it is now common. Few of us would blink if we encountered a sentence such as the following: ‘The ontological and

³ Full disclosure: I am now its editor.

epistemological orthodoxies of functionalism and positivism need to be problematised.’ All this means is that ‘Many of the truth claims made by management researchers are false.’ Yet if you or I were asked to write such a statement I suspect that we would come closer to my first sentence than the second. Increasingly, we build barricades to keep readers out rather than open doors to invite them in.

As more of us are socialised into the production of such discourse (to call it *writing* would be an abuse of the English language), a ‘perversion of natural selection’ is likely to occur (Edwards and Roy, 2017, p. 53). Younger academics have no experience of an alternative academic environment to that of the present. Those of them who conform to the sterile writing norms of our journals will prosper. As they advance in their careers the status quo seems completely natural to them – why should they challenge it? No wonder that we hear more and more scholars, young and old, talk of publishing as a ‘game.’ Devoid of any wider purpose, this is precisely what it can become.

Theory as tautology

Another manifestation of this problem is the profusion of tautologous hypotheses in our work (Spoelstra et al., 2016). For example, a study of workplace loneliness offers the following hypotheses, among many others (Ozcelik and Barsade, 2018, p. 2348):

‘Hypothesis 1. Employees who experience higher levels of workplace loneliness will be less affiliatively approachable towards their coworkers (employee approachability).’

My translation: ‘Lonely employees don’t talk much to their fellow workers.’

Hypotheses 2 reads as follows: ‘Employees who experience higher levels of workplace loneliness will be less affectively committed to their organization.’

My translation: ‘Lonely employees dislike workplaces where they feel lonely.’

It is little wonder that empirical studies designed to test such hypotheses almost invariably report that they have been ‘confirmed.’ It is difficult to imagine why anyone would suggest that

lonely employees talk more to their co-workers than those who are less lonely, or that loneliness might make them more emotionally attached to the places where they work. We have reached a stage where much of our research has moved beyond a bias in favour of publishing positive results. Rather, it is often designed so that *only* positive results can be found, even if the results tell us next to nothing about real phenomena. This may help explain why the volume of positive findings in our literature is so high. For example, Russell Craig and myself analyzed fifty randomly selected papers published in *Administrative Science Quarterly* that offered hypotheses. 90 per cent of the hypotheses proposed were either partially or fully confirmed. This problem is exacerbated by the reluctance of journals to publish negative findings.

Look where it has led us. Edwards and Berry (2010) tried to assess how precise management theories are. They examined papers published in the *Academy of Management Review* over the twenty-five year period between 1985 and 2009. Ultimately, they settled on twenty theories that offered 183 propositions. In terms of the magnitude of relationships that the propositions predicted, they report that:

‘19 (10.4%) simply stated that a relationship would exist, 164 (89.6%) described the direction of the relationship, and none of the propositions predicted a point value or range of values. With respect to the form of the relationship, 177 (96.7%) of the propositions were silent on this issue... For the most part, the theories in our sample developed propositions that predicted the direction of a relationship but said little about the form of the relationship or conditions that might influence the relationship. Rather, the majority of the propositions essentially stated that, if one variable increases, another variable will increase or decrease. It follows that, as the methodological rigor of studies designed to test these propositions increases, the likelihood of finding support for the propositions and their associated theories will likewise increase, putting the theories at progressively lower risk’ (p.670).

This is a polite way of saying that the theories they studied had little real predictive value. It also means that that they are hard to falsify, a common problem in organizational studies. The term that perhaps best describes this is *pseudo-science*.

'Genuine' Imposter Syndrome

There has been some discussion in the field of 'imposter syndrome', where high achieving individuals come to feel that their success is due more to luck than anything else, and that their own relative lack of ability is about to be exposed (Knights and Clarke, 2014; Bothello and Roulet, 2019). But I think another dynamic is also now at work. If you begin to believe that publishing papers is mainly a performative game played to boost your career, then one option is to *really* become an imposter. That is, you could pretend to be a scientist conducting rigorous research but cut corners, use big words to create the illusion of theory development, produce tautologous hypotheses and pray that outsiders will be too baffled and bored by your prose to see through what you are doing. Given enough practice, you may even fool yourself. In short, and while I acknowledge that there are many academics to whom this does not apply, a great number of us have responded to the performative expectations of academia by becoming what I term 'genuine imposters.' The imperative placed on theory development by our journals offers those of us who take this road a well-lit stage on which they can showcase what I think of as their 'talent to confuse.' As a result, the academic firmament is populated with theoretical bandwagons that are often built of tinsel, and held together by little more than hope and hype.

We need to call time on the kind of nonsense I am criticising in this paper, and which I myself have perpetrated. In an article appropriately entitled *Why academics stink at writing*, Pinker (2014b) argues that 'Our indifference to how we share the fruits of our intellectual labours is a betrayal of our calling to enhance the spread of knowledge. In writing badly, we are wasting each other's time, sowing confusion and error, and turning our profession into a laughing stock.' I agree. Such theorizing is ultimately vacuous, even if it appears in so-called

top journals. It is worth remembering that ‘research has no value if no one reads it, remembers it, or gets inspired to take the ideas further’ (Meier and Wegener, 2017, p. 193).

Fortunately, there are other approaches that we can adopt, and I offer the example here of a paper that I particularly admire. It is highly accessible and beautifully crafted. Anyone can understand it, without needing a dictionary at their elbow, alongside some analgesic substance to dull their pain.

WRITING DIFFERENTLY

Starbuck (1992) published *Learning by knowledge-intensive firms* in the *Journal of Management Studies*. One indication of its impact is that *JMS* republished it in a booklet of Classic Articles some years later. It made a key contribution to our understanding of what knowledge intensive firms are, how they are organised, and how they are managed. Starbuck starts with a story: ‘The General Manager of the Garden Company (a pseudonym) invited John Dutton and me to advise him about he called their ‘lot-size problem.’ He was wondering, he said, whether Garden was making products in economically efficient quantities.’ We then move to a new paragraph consisting of one sentence: ‘We had no idea what a strange but memorable experience this would be!’ (p.713). Starbuck develops the ‘story’ of how he and Dutton had a guided tour of the plant. It is a rich, detailed account of the manufacturing process, the attitudes of the workers, and the inefficiencies that were observed. Then, like many good stories, there is a twist. Despite all the inefficiencies that were plain to see, it turned out that the company was in fact highly profitable. Starbuck writes: ‘John and I had received several lessons in business... and the General Manager had not even charged us tuition!’ (p.714) The narrative has been inverted: the client turns out to be the teacher, and the teacher the client. From there they identify the major factor in the Garden Company’s success – the expertise of its workers: ‘It was a knowledge-intensive firm (KIF)’ (p. 715).

Now notice what is missing in my summary so far. There are no research objectives, no methods, no details of interview protocols – and so far no literature review. What could be called a literature review only begins some three pages into the paper, under the heading ‘What is a KIF?’ It could have begun like this, but look at the advantages of not doing so. The real experiences described now illuminates the literature in a way that would normally be impossible. Starbuck also places himself as a central character in the story: ‘Debates about how KIFs differ from other firms persuaded me to focus on firms that would be knowledge-intensive by almost anyone’s definition. As a starting point, I defined an expert as someone with a formal education and experience equivalent to a doctoral degree, and a KIF as a firm in which such experts are at least one-third of the personnel’ (p.719). Eschewing any pretence of omniscience, he frequently says things like ‘To my surprise’ and ‘To our amazement.’ The paper then builds to what I think is a beautifully written conclusion:

‘To appreciate such currents’ beauty and intricacy, social scientists need to stop averaging across large, diverse categories... In the social sciences, broad patterns over-simplify and capture only small fractions of what is happening. They leave scientists in worlds that **look** random. Broad patterns also tend to emphasize what is consistent with the past and to overlook subtle changes. There is also a world of bright colours, sizzling days, exceptional firms, rare experts, and peculiar KIFs’ (p. 738).

It is a paper that is both intellectually stimulating and aesthetically pleasing, and I have absolutely no doubt that it would be desk rejected by *JMS* if it were submitted to the journal today.

WHAT IS TO BE DONE?

Above all, I think we should stop describing publishing as a ‘game.’ Doing so legitimises the performative mindsets that have taken root within our field. It can downgrade or displace entirely the desire to generate interesting ideas, improve the world or address important

problems (Biggart, 2016). This is as pointless as it is ultimately self-defeating. When we cease to love the *process* of academic creation and instead become fixated only or mainly on outputs we can end our careers feeling that all our achievements are built out of sand. It takes more than a high h-index to have a meaningful academic life (Marx, 1990; Elangovan and Hoffman, 2019).

However, it is within our remit to change our language and what we write about. I can think of no better purpose than addressing the grand challenges that confront our species, and which at present we mostly ignore. Individually and collectively, we need to do work that matters and commit to what Courpasson (2013, p. 1244) calls ‘passionate scholarship.’ Despite the institutional pressures that are exerted on us, we still have a great deal of agency in what we research and how we write. We should exercise it more and conform less, for our own sakes and more importantly for the benefit of the societies in which we live (Tsui, 2019).

I have also been critical of how the peer review process now works in management journals. The ‘outputs’ that emerge from multiple rounds of revisions – bloodied, misshapen, overlong, and engorged with references – have not always been improved by it. We need to regain some sense of proportionate effort. Maybe it is time to heed Spiegel’s (2012) advice and, as authors, editors and reviewers just *do less*. I agree with the suggestion that two rounds of revision on a paper should be enough before a firm decision can be made on whether to publish it or not. As Alvesson et al (2017, p. 103) argue, ‘This would forestall ‘the escalation of commitment’ dynamic which locks reviewers and authors into a process usually ending up in tepid or overlong articles or bitter and costly rejections.’ It might even help everyone to actually enjoy the process, to at least some extent.

In addition, I suggest that journals should rethink the pole position they accord to theory development. It is absurd to insist that every paper must do this, in the absence of any similar emphasis on relevance, good writing, and the need to address interesting questions. If a paper

utilizes an existing theoretical framework to explore important issues (such as, say, the Great Recession, the emerging technological revolution in work, or climate change), I fail to see why that should be a problem. It might even make some of our journals – heretical thought - more fun to read. We need more of what McKiernan and Tsui (2019) describe as *responsible management research*: that is, research that tackles important issues and seeks to make a difference⁴.

This requires much greater variety in how we write. The world is too rich and diverse to be captured by jaded formats, and a prose style that accommodates only shades of grey rather than the full spectrum of the rainbow. We can do better.

CONCLUSION

In 1859, Charles Darwin concluded *The Origin of Species* with one of the most famous passages in all of the scientific literature:

‘It is interesting to contemplate a tangled bank, clothed with many plants of many kinds, with birds singing on the bushes, with various insects flitting about, and with worms crawling through the damp earth, and to reflect that these elaborately constructed forms, so different from each other, and dependent upon each other in so complex a manner, have all been produced by laws acting around us... from the war of nature, from famine and death, the most exalted object which we are capable of conceiving, namely, the production of the higher animals, directly follows. There is grandeur in this view of life, with its several powers, having been originally breathed into a few forms or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being, evolved’ (p. 425).

⁴ See also the following website, promoting Responsible Research in Business and Management: <https://www.rrbm.network>.

How vivid, inspiring and memorable this is. I can't immediately think of an equivalent in our writings about organizations. Yet organizations are teeming with as much life as any rural field or hedgerow. They have tribes and micro-tribes, outbreaks of insanity, struggles for existence, mass extinctions and the flourishing of new forms of organizational life. They are full of love and hatred, optimism and despair, and all the emotions in between these extremes. Organizations are maddening to study, absorbing, often funny and rarely boring. Our line of work gives us the privilege of writing about the human beings who inhabit them, the endless mistakes that they and we all make, and countless opportunities to discuss how they could be improved. Seemingly unimpressed, most of our writing has drained all life and colour out of the fantastic spectacle in front of us. It is not an approach that will win a wide audience for our ideas, nor does it deserve to.

We need to be astonished afresh by the ordinary, for the ordinary is not ordinary at all. Perhaps it is time to write about management and organizations with less obscure theorising, with more variety, and with a little more humour, curiosity, and passion.

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