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ON CRISIS, GENUINE IMPOSTERS AND COMPLACENCY IN MANAGEMENT STUDIES

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I am grateful to all those who responded to my original ‘provocation.’ In this short response I will mostly take our areas of agreement as read. However, I will clarify some issues that have arisen, and then focus on where disagreements exist.

It is worth reiterating what has brought this debate into being. Fundamentally, many of us are concerned about our publishing practices, including what we write about and how we do it. Audit, accountability and ranking systems have proliferated, bringing with them escalating demands about how much we need to publish (Warren, 2019) and where we must publish it (Tusting, 2019). This also affects how we write and what we need to write about in order to do secure publication in a prized elite of ‘A’ journals (Frémeaux, et al, 2019; Yoshikawa, 2019; Aguinis et al, 2020). Since these journals stress theory development above all else, most of us focus ever more on developing theory as our main goal. Other objectives become less important or disappear altogether.

Given that developing theory is actually rather difficult, many of us just pretend that we are doing so. This requires us to write in pretentious language, develop hypotheses that are actually tautologies (e.g. that lonely employees don’t talk much to their fellow workers) and report in triumphant prose that these have been supported by our data. We have become ‘genuine imposters.’ Jean Bartunek, Laura Empson, Kai Peters and Howard Thomas, Catherine Cassell and Gavin Schwarz seem to broadly agree, while making important additions to the argument.

Moreover, since many journals covet ‘A’ status they are tempted to mimic the priorities of already established journals in the mostly hopeless quest to join them. There is only so much room at the top. These isomorphic pressures affect us at all levels, from individual academics to journals, scholarly associations, business schools, Universities and national education
systems. PhD students are increasingly socialized into the expectation that they must publish even before they complete their PhDs, and do so in what are regarded as top tier journals (Gabriel, 2019). Prasad (2013) reports that PhD students in elite US business schools often change their original topics when confronted with feedback that their original idea wasn’t in favour with elite journals. Maybe these changes are sometimes for the better. But I can’t think of much worse criteria for selecting a PhD topic than what is flavour of the month with a handful of outlets. The modes of writing that these journals encourage reproduce themselves with the willing connivance of elite schools.

POINTS OF AGREEMENT AND CLARIFICATION

Now to clarify some issues that have arisen. Like Catherine Cassell, I also do not believe that ‘the entirety of our discipline is morally bankrupt and populated by imposters’ As I said in my original piece, there are a great many of us who resist the pressures that I am critiquing. But I do believe that superficial theorising, bad writing and a focus on relatively trivial topics have become endemic. Nor are these problems confined to a few ‘top’ journals. I cited examples of such practices from European organizational theory journals, such as *Organization Studies* and *Organization*. A critical orientation does not immunise authors from pretentiousness. Indeed, it often seems to demand it.

To be clear, I am not criticizing the development of new theories or the development of existing ones. I am simply complaining that theory development has been given too much priority in our writing. Theory is a very good thing. But you can have too much of anything, including physical exercise, dieting and building theory. Taken to an extreme, means can become ends, overwhelming the goals you originally had in mind. We have reached this stage with theory development. Peter Bamberger argues that ‘The constant development, updating and fine tuning of a comprehensive yet parsimonious statement of principles allows us to provide practitioners with frameworks and models allowing them to adopt practices and lines
of action with reasonably predictable outcomes.’ I disagree. The Evidence Based Management movement has arisen precisely because its advocates complain that we do not synthesise important findings and present them in the manner that Bamberger argues we do. It is the exception rather than the rule. The data on this issue are not encouraging. Pfeffer and Fong (2002: 88) concluded that ‘less than one third of the tools and ideas that companies are paying money to implement came out of academia and those that originated in universities were used less often and were abandoned more often.’ The unpleasant truth is that business school academics write mostly for each other. Managers rarely read our work and generally find it alienating when they do.

**Grand challenges**

I agree with Jean Bartunek that theory has many advantages which go beyond merely addressing grand challenges. These include the intrinsic interest of the theory concerned. I don’t wish to replace one stifling orthodoxy (‘you must develop theory’) with another (‘you must only address really big issues’). That said, it is astonishing how rarely our work engages with issues that really matter. Rather, ‘researchers seem to focus more and more on short-term and low-risk projects to secure publications, thus bringing about intellectual stagnation’ (Pianezzi et al, 2019: 1). The result is a ‘proliferation of incremental and theoretical work that contribute little and reduce the relevance of management research overall’ (Billsberry et al, 2019: 120). Encouraged to exaggerate the extent of their contribution, authors habitually claim to float like a butterfly and sting like a bee. In truth, their feather light punch is generally forgotten as soon as it lands.

Thus, climate change is possibly the greatest existential crisis to face humanity since the second world war. A search of the *Academy of Management Journal’s* abstracts shows that the

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1 While I have some criticisms of the EBM movement (Tourish, 2013), I agree with its argument that our work should do more to address important issues, and explain the relevance of our theories and findings for a much broader constituency than ourselves.
journal has published maybe five papers that deal with it. My search using terms such as ‘Great Recession,’ ‘financial crisis’ and ‘banking crisis’ suggests that it has published none dealing with the events of 2008. This is astonishing, bizarre and disgraceful. I suggest that our irrelevance to the world beyond the scholarly community more than merits the adjective ‘crisis.’

OPTIMISM, OR COMPLACENCY?

All of which brings me to the most critical contribution here, that of Shaw and Baer. I normally admire optimism. But my admiration turns to concern when it denies inconvenient truths and becomes complacency. Shaw and Baer don’t quite say that all is for the best in the best of all possible worlds, but apart from an occasional scuffmark on the bodywork they seem to imagine that our publishing juggernaut is in pristine shape. Attempts to argue otherwise are dismissed as ‘wanderings.’ For example, they describe as ‘trivial’ my criticism of the word ‘munificence’ in a paper, instead of its author using a more familiar term that would be more readily understood. My point was that obscure language has become a default position by authors increasingly desperate to convey the impression of theory development. The result is a growing number of papers that only those on the inside of the debates they reference can understand. The point may be right or wrong, but it is scarcely ‘trivial’.

To clarify a misunderstanding on the part of Shaw and Baer (and also Bamberger), my complaint is not with theory development, novel or otherwise, but with the absolute priority that this has assumed in our writing. I agree that an entirely new theory is not required for every paper. I should have stated more clearly that if authors use an existing theory or don’t develop new implications for theory their work will likely be rejected. But this scarcely improves matters. We remain in a position where the development of new insights for theory, however this is defined, has become the ultimate criterion for deciding on publication (Miner, 2003).
The resultant stress on novelty means that we are increasingly bombarded with papers that aim not just to make incremental advances in theory but impulsively claim to be revelatory – that is, they purport to offer a radically new way of thinking on some issue within our field (Ferris et al, 2012). As Lindebaum (2016: 537) has pointed out, this means that ‘… accelerating demands for novel theories in management studies imply that new methodologies and data are sometimes accepted prematurely as supply of these novel theories.’ He offers the example of neuroscience in management research. Put bluntly, ‘Largely because our journals expect it, authors forcefit theory to phenomena that are still emerging’ (Wood et al: 405).

My book (Tourish, 2019) offers a chapter length example from my own sub-field – leadership studies. This is the rapid growth of research into Authentic Leadership Theory (ALT). I argue that ALT differs little from the already established theory of transformational leadership, and shares its weaknesses. The theorizing is shallow, the empirical data behind it (resting mostly on analyses of results obtained from a flawed survey instrument) are highly questionable, and the hypotheses that the research tests are generally tautologies so designed that only positive results are possible. This is what we would expect to find when we have normalized and naturalized the practice of publishing primarily to advance our careers. It is not the hallmark of a healthy field.

**The value of peer review**

I am puzzled by Shaw and Baer’s claim that I have suggested peer review is ‘a relatively new process that journals now force authors to endure.’ They then defend the value that peer review adds to the publishing process. But so did I! There is nothing in my contribution to suggest that I think the review process should be ‘abandoned or curtailed’. My point about peer review was that ‘when it becomes (so) 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, to
satisfy reviewers and build their careers.’ It is surely perverse to deny that, despite its manifest benefits, the peer review process often assumes such dysfunctional forms.

Shaw and Baer are unfazed by my discussion of Seibert et al’s award-winning paper in *AMJ* from 2004. Let us recall that it received a first set of editor-reviewer comments covering 13 pages of single-spaced text, resulting in a reply of 31 pages of single-spaced text. To which I say: wow! What is missing from Shaw and Baer’s narrative is any sense of the power dynamics involved in the review process. While it is technically true that authors don’t have to accept all of the feedback from editors and reviewers, they are pressured to do precisely that, and many feel compelled to do so. Bedeian (2003) surveyed 173 lead authors of papers published in *AMJ* and *AMR* between 1999-2001. While he found much that was positive (after all, these authors had successfully navigated the review process), 34.1% also reported that they had felt pressure to conform to the personal preferences of the editor or reviewers in order to get their work published; 25% made changes to their papers that they felt were wrong; 34.1% felt treated as inferiors rather than peers by an editor or reviewer; 56.1% felt that editors regarded a reviewer’s knowledge about original research as more than important than the authors.2

In a blow to any assumption of reviewer superiority, 54.7% of the authors said they had been asked to review a manuscript that fell beyond their competence, but 36.6% reported that despite this they submitted a review anyway. As Gabriel (2010: 764) concluded:

‘Publishing is now a long process, involving numerous revisions, citing authors one does not care for, engaging with arguments one is not interested in and seeking to satisfy different harsh masters, often with conflicting or incompatible demands, while staying within a strict word limit. Most authors will go through these tribulations and the drudgery of copious

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2 In a delicious irony, for my purposes, it appears that *AMJ* rejected Bedeian’s critical paper because it did not develop theory.
revisions, accepting virtually any criticism and any recommendation with scarcely any complaint, all in the interest of getting published.’

Almost as an afterthought, the Shaw and Baer concede that there are ‘certain areas of concern’, including ‘faux theorizing, over-theorizing and intellectual exhibitionism.’ Having defended the existing state of peer review it is curious for them to acknowledge that many flawed papers somehow manage to evade its scrutiny. It doesn’t seem to occur to them that, in many instances, such defective work appears not in spite of the peer review process but because of it. Reviewers know that journals expect theory development much more than, say, insights for practice. They therefore often ‘encourage’ authors to load their work with more references to the work of hallowed authorities, more theorizing, and more jargon so that they can become more academically credible. No one wants to make our work unreadable. But, too often, this is the unintended consequence of our fetish for building theory.

Shaw and Baer suggest that I favour only a particular kind of research – namely, ‘qualitative studies written with an obvious flair for the dramatic.’ Not so. I favour papers that are interesting, address important issues, are well written, and can be understood by as wide an audience as possible. I object to papers that are trivial, badly written and comprehensible only to those who are already fully submerged in the topic concerned. Consistent with this, I object to the pursuit of novelty for its own sake. The point is that most of us now do such things as a matter of routine, and I offered an example from my own work in my original paper. There is something systemically awry when so many people enter academia with high hopes and positive intentions, only to end up spending much of their time ‘doing and publishing research primarily for other researchers, not for the broader practice community’ (Aguinis et al, 2020: 142). This is like staging a play where the only spectators are those participating in the performance. It is pointless.

NEXT STEPS
What is the way forward? Shaw and Baer identify several positive initiatives with which I am familiar, as does Bamberger, and which I support, including the development of the Responsible Research in Business and Management initiative (rrbm.network). While promising, these are at an early stage. There remains scope for action at multiple levels. As individuals, there is nothing to stop us from committing to doing work that matters. We can, for instance, show a much greater concern for such issues as justice, ethics and purpose (Contu, 2019), and make more of an effort to do research that at least attempts to help solve the world’s problems (Bartunek and Rynes, 2010). Institutional constraints are not absolute. We still have great freedom to determine what we write about and how. If this sometimes means passing up on the chance to publish in what purport to be top tier journals then that may be a choice worth making.

At field level, it is time to rethink the priority that we place on theory development. If we can use an existing theory to explain interesting phenomena, without always subordinating what we are analysing to desperate attempts at developing the theory, I don’t see why that should be problematic. We can also elevate the importance of other things that an academic paper might accomplish, such as identifying implications for practice and addressing multiple audiences, including policy makers, the public, and trade union organizations. I see no reason to define implications for practice only in terms of managers. Many stakeholders are affected by the world of organizations, and we can do a better job of acknowledging their interest in what we research.

Of course, we don’t have to do any of this. We can, like Shaw and Baer, express overall satisfaction with the status quo, and continue to play ‘the publishing game.’ Many of us have benefitted from it, at least in terms of building our careers. Beyond the quality of our writing, this mind-set facilitates the acceptance and normalisation of many Questionable Research Practices (Aguinis et al, 2020). If our work is only a ‘game’, then why not bend the rules, just
a little (at least to begin with), if it helps us to achieve publication hits? Ultimately, I think this approach is unsustainable. Our stakeholders won’t wear it indefinitely. Moreover, the despair and cynicism it engenders among ourselves may make academic life too meaningless to bear (Fleming, 2019).

We can, and we must, aim higher and do better.
REFERENCES


