Seeing and Being Seen as a Management Learning and Education Scholar: Rejoinder to "Identifying Research Topic Development in Business and Management Education Research Using Legitimation Code Theory"

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We greatly appreciate the editors' invitation to respond to Arbaugh et al's (2016) thought-provoking article about the current state of business and management education (BME) research. As incoming and current co-editors of *Management Learning*, it is an excellent opportunity to contribute to a discussion about how we in the management learning and education community see ourselves and want to be seen by others, both within the academy and beyond.

The distinctiveness of *Management Learning* is illustrated by the journal's new strapline, *The Journal for Critical Reflexive Scholarship on Organization and Learning*. Articles in the journal share two defining characteristics – they engage in critique, and are intentionally thought-provoking. Critique is valued not for its 'own sake', but because it is often a source of creativity and innovation. Being 'critically reflexive' means being explicit about the philosophical assumptions and theoretical perspectives that fundamentally shape our research practice and the knowledge that is generated through it. As former editor-in-chief Chris Grey (2009) puts it, contributors to the journal have a 'license to think' differently.

In responding to Arbaugh et al we want to reflect critically on two assumptions they make about what constitutes a successful academic field. First, they suggest such fields have taken-for-granted knowledge which forms the foundation that future research can build upon; and second, that they have a high degree of scholarly impact. By holding these assumptions up to critical scrutiny, those of us who undertake BME research might think differently about how we engage with existing knowledge in the field. This could have implications for how we think about research currently and in the future.

Arbaugh et al note that:

> "the lack of a common foundation among topical areas may reveal a relatively immature field, as each topical area seeks to find its own basis for existence versus a more mature field where many areas recognise their common educational research foundations, and thus agree on foundational roots and research questions" (p.5).

This belief is shared by Rynes & Brown (2011) who, writing in *Academy of Management Learning & Education*, note that "one thing that *does* differentiate high from low-legitimacy fields is the extent of taken-for-granted knowledge upon which to build current research" [576: emphasis in original]. Arbaugh et al find, based on

their citation analysis, BME to be "somewhat immature in its development" (p.20) and call on scholars to build on the field's foundational works to progress its development.

It seems to us that the desirability of taken-for-granted knowledge is itself taken-for-granted. If we reflect critically on the ontological and epistemological commitments underpinning this conception of legitimacy, we see it is based on a belief in the existence of a 'real world' that we can use to generate truthful (valid, reliable) knowledge though scientific research. This is a 'building block' conception of an academic field, where agreed foundations are taken to be objective representations of what constitutes BME research. If we understand legitimacy in this way, 'progress' is possible only by accepting the past and seeing the future as unproblematically building upon it. We, instead, see BME is a pluralistic field that does not rely on paradigm consensus based on a positivistic philosophy of science for its future development.

We suggest that knowledge production in our field does not proceed in such a hierarchical and incremental fashion. This is because BME research is produced in the context of, and through direct engagement with, social practice and application-based problems that take into account the values and perspectives of practitioners as well as the researchers who study them. Hence, BME research is a form of mode 2 knowledge production (Gibbons et al, 1994), the value of which arises through interactions between theory and practice. BME is also, we suggest, characterised by lack of consensus regarding what questions need to be asked and what constitutes legitimate knowledge. Issues also tend to be returned to recursively, resulting in the production of situationally specific, ideographic knowledge (Bell & Willmott, 2014). This 'soft' approach to knowledge production is quite different from the one Arbaugh et al (2016) appear to promote, which seems to us to be based instead on mode 1 assumptions (Gibbons et al, 1994).

In addition to the absence of philosophical assumptions about the nature of the knowledge that informs BME research, Arbaugh et al (2016) do not reflect on the methodological values that determine what is seen as legitimate knowledge, and the narrowness sometimes entailed in these definitions. The legitimacy of BME as a field of study is tied to notions of methodological rigor as a means of assessing the quality of research (Bell, Kothiyal & Willmott, forthcoming). However, the kind of rigor that tends to be implied in assessing the quality and legitimacy of BME research is often linked to a positivist philosophy of science and promotes an objectivist criteriology, e.g. through unproblematic application of notions such as 'bias' (e.g. Currie & Pandher, 2013). As Bell, Kothiyal & Willmott (forthcoming) argue, this focus on methodological technique can lead to the narrowing and displacement of research goals, erasure of context, and devaluation and marginalization of alternative methodological genres.

Arbaugh et al's (2016) mention of rigor focuses on the "prominence" of the journal that an article is published in, this being "associated" with certain "indicators of rigor and quality" (p.4). We are concerned that this promotes an elitist and self-reinforcing view of research quality - through implying that "highly prominent journals" *necessarily* produce high(er) quality research. This does not take into account the politicized socio-cultural processes through which the prominence of North American

journals is constructed (Grey, 2010) in ways that disadvantage scholars from other parts of the world (Murphy & Zhu, 2012). It also overlooks the academic 'gamesmanship' that goes into publishing in such journals, practices that are reinforced by the use of journal ranking systems in building academic careers and business school reputations (Mingers & Willmott, 2013).

We agree with Arbaugh et al (2016) that consideration of an academic field's foundations is crucial for its future development. But we would like to see more critical reflexivity about those origins because, by taking these for granted, we may be missing opportunities to enrich BME research. By way of illustration, we draw on the field of change management, which has as its foundational framework Kurt Lewin's 'changing as three steps' (unfreeze-change-refreeze) model (Lewin, 1947). Lewin's contribution is an important source of legitimacy for the change management field, offering academic respectability through connection to a famous psychologist as well as a set of questions that are explored by researchers today. The model also provides legitimacy with practitioners by underpinning many change models widely used in organizations. However, while this common foundation might be seen as a source of strength and maturity, it might also be viewed as a weakness. Cummings, Bridgman & Brown (2013) show how 'change as three steps' is more a product of others than Lewin himself *and* something far more fundamental and instrumental than Lewin intended it to be. Change management scholars are unlikely to be aware of this because, while they generally cite 'Lewin, 1951 *Field Theory in Social Science*' as the source, most would never have read what Lewin had to say about it because it occupies just two paragraphs buried deep in a 338-page book. Cummings et al (2013) conclude that 'change as three steps' "is a solid foundation only in the sense that it has hardened through a series of interpretations that have built upon each other, and this sedimentation may now repress other ways of seeing or organizing thinking about change" (p.18).

This dynamic can also be seen within BME. The case method of teaching developed at Harvard Business School. With its emphasis on training students to solve business problems, case method is seen by its advocates as well as its critics as offering both the origin and unwavering clarity of purpose for business education. However, Bridgman, Cummings and McLaughlin (2015) demonstrate through a historical examination of the emergence of the case method at HBS that the case method's 'past' is more contested than is acknowledged in its 'history'. For example, in response to the social and economic crises of the 1920s and '30s, the narrow, instrumental conception of the Harvard case method that we take for granted today was broadened to incorporate a more philosophically-informed, critical reflection on the role of business in society. Rethinking these foundations (rather than accepting and seeking to build on them) would enable us to think afresh about the case method and the purpose of business schools.

The second aspect of Arbaugh et al's (2016) article we want to reflect on is their assumption that successful, mature, progressive academic fields have a high level of scholarly impact. This, they suggest, is achieved primarily through the influence of a small group of active and well known scholars whose work is highly cited. They conclude that while there are encouraging signs, BME research is relatively immature on this measure, possessing few scholars who have developed a reputation first and foremost as BME scholars. They also find BME research to be rather insular, with

few articles published in BME journals having a significant impact on other fields. While we share Arbaugh's et al's (2016) recognition of the importance of scholarly impact, and also wish to see established and emerging researchers regard BME research as a legitimate field of study through which to frame an academic career, we are concerned about their reliance on citation patterns, which we see as a fundamentally problematic measure of scholarly activity and value.

The value of citation analysis as a proxy for scholarly value can be affected by 'coercive citation' (Wilhite & Fong, 2012), where authors are, either implicitly or explicitly, pressured by editors to cite other articles in the journal in order to enhance a journal's impact factor. Studies indicate that self-citation practices are widespread and more pronounced in the business disciplines than in economics, psychology or sociology. Wilhite and Fong further suggest that authors in our field are more likely to continue submitting their work to journals where coercive citation is the norm (Wilhite & Fong, 2012). Yet Arbaugh et al (2016) do not take self-citation into account in their analysis. Nor do Arbaugh et al (2016) take into account gender as a variable that affects citation patterns. In other disciplines, including sociology (Davenport & Snyder, 1995) and international relations (Maliniak, Powers & Walter, 2013), it has been found that women are systematically less cited than men, even when a range of other variables including institutional affiliation, tenure status, substantive focus, and methodology, are taken into account. Women scholars may be drawn to BME because they see it as more closely aligned with their identities as teachers and administrators (Thomas & Davies, 2002). It would therefore have been interesting for Arbaugh et al (2016) to explore whether BME is a field that is prone to the social dynamics through which articles authored by women are constructed as less central to the development of a field than those authored by men, in ways which cannot be attributed to differences in the quality of their scholarship.

As these examples illustrate, the idea that citation analysis is an objectively neutral measure of the quality of BME scholarship (Currie & Pandher, 2013) is, we suggest, fundamentally erroneous. Such analyses are also excessively reductive; by focusing on what can be easily measured and counted there is a danger that they overlook 'softer' yet, we suggest, more significant evaluations of the value of knowledge, including a focus on the ethical, practical and political implications of BME research. We are also mindful of other dimensions of research impact which are of importance to BME scholars that cannot be captured through citation analysis. As Paul Adler noted in his presidential address to the Academy of Management, if we consider the extent to which our research has made a difference to the lives of our readers, and compare that with our influence in the classroom, most of us would conclude that our greatest impact (whether positive or negative) is on our students (Adler, 2016). The same could be said for BME research – that its primary impact is in shaping the learning of management educators and students, as well as influencing management learning processes within organizations. Consequently, while some academic fields might justifiably attract criticism that research is an end in itself that is not sufficiently relevant or related to practice, this seems less likely for BME research.

We share Arbaugh et al's (2016) desire for BME to attract new scholars, be seen as a worthwhile area of research, and influence other academic fields. Their analysis will potentially be seen as a useful guide for researchers considering the viability of BME as a career path. However, given the nature of BME research, we continue to expect it

to attract a high proportion of contributors for whom BME is not their dedicated, or even their primary, field of study (Arbaugh, 2016). Many will be attracted to our journals because of the opportunity to reflect on their practice as educators and, in doing so, increase the likelihood of them having a positive impact on their students – rather than as the result of a calculated career move.

In offering this critique, our intention has been to ask some critical questions about whether 'legitimacy', as commonly understood, is a standard against we should judge ourselves. Where Arbaugh et al (2016) want to celebrate foundational works, we want to engage critically with them. Where they value consensus and taken-for-granted knowledge, we value contestation and challenge to existing assumptions and established ways of thinking within the field. Whilst we agree that 'innovation' and 'progress' are desirable, we understand these terms rather differently.

In a world characterised by social, environmental and economic crises, there is an urgent need for management learning and education that offers a stronger critique of established modes of thought and action. We suggest that *Management Learning,* and other journals within the BME field, are well-positioned to respond to these challenges. While discussions about scholarly impact are worthwhile, we want to continue to see BME as a means to a greater end – enhanced management learning and education. We need not feel inferior to other fields within management and organization studies. Indeed, we have good reason to believe we might be viewed with envy by them.

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