

Has Management Studies Lost Its Way? Ideas for More Imaginative and Innovative Research

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ABSTRACT Despite the huge increase in the number of management articles published during the three last decades, there is a serious shortage of high-impact research in management studies. We contend that a primary reason behind this paradoxical shortage is the near total dominance of incremental gap-spotting research in management. This domination is even more paradoxical as it is well known that gap-spotting rarely leads to influential theories. We identify three broad and interacting key drivers behind this double paradox: institutional conditions, professional norms, and researchers' identity constructions. We discuss how specific changes in these drivers can reduce the shortage of influential management theories. We also point to two methodologies that may encourage and facilitate more innovative and imaginative research and revisions of academic norms and identities.

Keywords: interesting theories, problematization, research methods, research problems, research questions, theory development

INTRODUCTION

The enormous expansion of the management field has over the last decades resulted in a vast growth of academic articles published. This expansion has been coupled with a disproportional increase in the rejection rate due to limited journal space. Given the huge growth of management articles combined with an intensified competition for getting published, one could perhaps expect a significant boost in innovative and high-impact papers, but no, rather the reverse. Despite all the good and rigorous research being produced, there is a broadly shared sense of a troubling shortage of novel ideas and really strong contributions within management studies (e.g., Bartunek et al., 2006; Clark and Wright, 2009; Daft and Lewin, 2008; Grey, 2010; Starbuck, 2006, 2009).

In this paper we give support for this view, point at major problems in the field contributing to the sad state of affairs, and offer suggestions for what can be done to reverse the situation. The paper consists of two parts. In the first part, we describe the

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perverse situation that, at the same time as more journal articles than ever are being published, there is a broadly shared assessment of an increasing lack of more interesting and influential studies. Contrary to the common knee-jerk reaction that more rigour in theorizing and in methodological procedure will fix the problem (Donaldson et al., 2012), we contend that a major reason for this paradoxical shortage is the almost total dominance of incremental gap-spotting research within management studies. The strong prevalence of gap-spotting research is even more surprising, as it is widely recognized that a theory attracts attention and becomes influential if it challenges our current assumptions, rather than reinforcing them by thoroughly and systematically filling a gap in existing literature. In the second, and main part of the study, we investigate why most researchers seem to favour incremental gap-spotting research. We point to three broad and interacting drivers: institutional conditions, professional norms, and researchers' identity constructions. Finally, in order to turn our field from one preoccupied with piecemeal additions and confirmations of what we think we know – to more frequently coming up with more novel and high-impact contributions – we suggest a series of key changes in institutional arrangements, professional norms, researchers' identity constructions, and our methodologies for theory development.

A DISTURBING SHORTAGE OF INTERESTING AND INFLUENTIAL WORK

The number of management articles published has increased drastically during the last three decades for several reasons (Gabriel, 2010). Numerous new business schools have been established globally, at the same time as existing business schools have increased in size (Engwall and Zagagni, 1998; Spender, 2007). The more frequent use of research assessment reviews in many countries (e.g., RAE/REF in the UK, and ERA in Australia) for evaluating research performance are also central drivers behind the rapid growth of articles published. Those 'publish or perish' reviews demand that academics publish on a regular basis – ideally in top-tier journals – as it will improve the academic rankings for the business school in question (and the status of the researcher).

Not only has the number of journal articles increased substantially, but so, too, has the competition for getting published. The acceptance rate has been steadily shrinking in most journals and is now as low as 5 per cent in our top-tier journals. As many have noted (e.g., Gabriel, 2010; Tsang and Frey, 2007), publishing in these journals is a long and tedious process, involving numerous revisions before getting the final verdict, which usually is a rejection. Given all this, one would expect a relative increase in high-quality research, leading to more interesting and influential theories being published over the years. Paradoxically, this is not the case. Quality may have in some respects gone up, but hardly the number of interesting and influential contributions.

Instead, several prominent scholars (e.g., Grey, 2010; Oswick et al., 2011; Starbuck, 2003, 2006, 2009) and editors for our leading journals have frequently been raising strong concerns about the lack of more innovative and influential studies. For example, in his examination of knowledge production within the social sciences, Starbuck (2006) expresses disappointment and disillusionment with theories developed within the management field, noting that 'years pass with negligible gains in usable knowledge;

successive studies of topic appear to explain less and less' (p. 1), and 'too much effort goes into generating meaningless research "findings", and the flood of meaningless "contributions" probably obscure some discoveries that would really be useful'. Similarly, in his overview of the relationship between European and North American management journals, Grey (2010, p. 691) concludes that the research published in 'both elite US journals and those European journals that seek (with, as I have suggested, inevitable failure) to join that elite have become increasingly formulaic and dull'. And in their wide-ranging review of theories utilized within organization and management theory (OMT), Oswick et al. (2011) complained that almost all influential theories within OMT have been brought in from the outside, not developed within OMT.

Several leading journal editors make similar assessments. Reflecting back on the years since launching *Organization Science*, Daft and Lewin (2008, p. 177) conceded that their original mission to reorienting organizational 'research away from incremental, footnote-on-footnote research as the norm for the field' (Daft and Lewin, 1990, p. 1) had not been realized. They re-emphasized the need not to prioritize incremental research but, instead, 'new theories and ways of thinking about organizations' (Daft and Lewin, 2008, p. 182). Similarly, the outgoing editors of *Journal of Management Studies* – based on their review of more than 3000 manuscript during their six years in office (2003–08) – noted in their concluding editorial piece that while submissions have increased heavily '... it is hard to conclude that this has been accompanied by a corresponding increase in papers that add significantly to the discipline. More is being produced but the big impact papers remain elusive ...' (Clark and Wright, 2009, p. 6). Equally, the editors of *Academy of Management Journal*, Bartunek et al. (2006, p. 9), argued that while *AMJ* is publishing 'technically competent research that simultaneously contributes to theory ... [it is] desirable to raise the proportion of articles published in *AMJ* that are regarded as important, competently executed, and *really interesting*'.^[1]

These and other editors are hardly inclined to exaggerate the problems. Normally, journal editors are likely to point at the progress, strengths, and success of their journals. A widespread perception of a shortage of high-impact papers can therefore be seen as a strong indicator of a deeply dissatisfying state of the art. Innovative and influential writings like those that emerged in the late 1970s are rarely seen nowadays. It is difficult to come up with recent contributions of such magnitude as the paradigm study by Burrell and Morgan (1979), the root metaphor idea (Morgan, 1986), and institutional theory (Meyer and Rowan, 1977). Similarly, impressive in-depth case studies like those of Jackall (1988), Kunda (1992), Pettigrew (1985), and Watson (1994) have hardly been seen during recent years. Instead, incremental research rather than innovation and creativity seem to dominate the hard work of all the diligent people in our field.

What Makes a Theory Interesting and Influential?

But why does incremental research rarely seem to generate high-impact theories? In order to answer that question we need first to understand what makes a theory interesting, i.e., that it attracts attention from other researchers and, thus, becomes influential. Although different people may find different studies and theories interesting, interestingness is hardly just a matter of idiosyncratic opinions. Collectively held assessments of what counts

as interesting research are much more profound than purely subjective views, even though the collective can be restricted to a sub-community rather than an entire field, such as management studies. During the last four decades, originating with Davis's (1971) seminal study, a large number of researchers within management and the social sciences have shown that – quite different from what Donaldson et al. argue – rigorously executed research is typically not enough for a theory to be regarded as interesting and influential: it must also challenge an audience's taken-for-granted assumptions in some significant ways (e.g., Astley, 1985; Bartunek et al., 2006; Black, 2000; Corley and Gioia, 2011; Weick, 1989, 2001; Wicker, 1985). In other words, if a theory doesn't challenge some of an audience's assumptions significantly, it is unlikely to receive attention and become influential – even if it has been extremely rigorously developed.

Gap-Spotting: The Key Problem to the Paradoxical Shortage of Interesting Studies

Although it could be argued that every scientific inquiry involves some form of questioning, contemporary studies bear little witness of deliberate and systematic attempts to challenge the assumptions underlying existing theories (Barrett and Walsham, 2004; Bartunek et al., 2006; Clark and Wright, 2009; Johnson, 2003; Locke and Golden-Biddle, 1997; Sandberg and Alvesson, 2011). Instead, the most prevalent way of theory development in management studies appears to be *gap-spotting* (Alvesson and Sandberg, 2011; Sandberg and Alvesson, 2011). It is by identifying or constructing gaps in existing literature that most management researchers formulate their research questions and develop their theories. In gap-spotting, researchers refer positively or mildly critically to earlier studies with the purpose of 'extend(ing) this literature' (Westphal and Khanna, 2003, p. 363), to 'address this gap in the literature' (Musson and Tietze, 2004, p. 1301), to 'fill this gap' (Lüscher and Lewis, 2008, p. 221), and to rectify the oversight that '[n]atural languages have . . . received very little explicit attention by organization scholars' (Vaara et al., 2005, p. 597). Such gap-spotting research means that the assumptions underlying existing literature for the most part remain unchallenged in the formulation of research questions.

It is important to note also that assumption-challenging research needs to be connected to established literature in order to be seen as meaningful (McKinley et al., 1999). As Cornelissen and Durand (2012) remark, a theory is seen as novel and counterintuitive only in relation to what we already know, that is, existing literature. But building mainly positively on earlier work within a school or area and identifying gaps that have not been addressed as starting points and rationales for studies are fundamentally different from the idea that underlying assumptions are crucial, often problematic, and in need to be critically addressed.

Gap-spotting is of course not something absolute but varies in both size and complexity. It can vary from an incremental extension of an established theory to the identification of more significant gaps in existing literature (Colquitt and Zapata-Phelan, 2007). It can also sometimes involve a strong degree of questioning existing literature (Locke and Golden-Biddle, 1997). Neither is it simply about finding a gap in a given body of literature. Instead, it often involves constructing gaps by bringing together different bodies of literature in complex and sometimes creative ways (Golden-Biddle and Locke,

2007; Locke and Golden-Biddle, 1997; Sandberg and Alvesson, 2011). Nor do we deny the possibility that gap-spotting research can lead to important contributions. However, because gap-spotting research does not deliberately and ambitiously question the assumptions underlying established literature, it rarely leads to the development of new high-ranking theories. In other words, gap-spotting is more likely to reinforce or moderately revise, rather than challenge, already influential theories (Sandberg and Alvesson, 2011).^[2]

WHY THE STRONG DOMINANCE OF GAP-SPOTTING RESEARCH IN MANAGEMENT STUDIES?

That incremental consensus-confirming work is much more common than consensus-challenging contributions is unsurprising. High impact studies are per definition very rare. What is surprising – at least for the editors and other commentators cited above – is that the number of consensus-challenging studies is disappointingly low. The dominance of incremental gap-spotting research is even more puzzling, as it is well known that it is consensus-challenging, not consensus-seeking theories that tend to receive most attention and become influential. We think there are three broad and interacting drivers, offering explanations to this paradoxical behaviour amongst management researchers: institutional conditions, professional norms, and researchers' identity constructions.

Institutional Conditions

Institutional conditions refer to how institutions (e.g., governments, universities, business schools, funding bodies) and their policies regulate the conduct of research, especially the production of research reports. Universities and researchers in many countries across the globe are increasingly governed by various assessment formulas introduced by governments for evaluating academic research performance, such as RAE/REF in the UK and ERA in Australia (Bessant et al., 2003; Leung, 2007; Willmott, 1995, 2011). A key performance indicator in those assessment formulas is the number of articles published in high-ranking journals within a *designated journal list*. This has meant that practically the only research performance that counts in many business schools today is publications in A-listed journals.

As noted by many across the entire scientific field (e.g., Adler and Harzing, 2009; Lawrence, 2008; Macdonald and Kam, 2007), the use of such journal lists is likely to encourage researchers to concentrate on publishing articles in particular journals rather than trying to develop more original knowledge by identifying and challenging the assumptions underlying existing literature. In management studies, Macdonald and Kam (2007, p. 702) observed that: 'All but forgotten in the desperation to win the game is publication as a means of communicating research findings for the public benefit.' And in science Lawrence (2008, p. 1) noted that the use of journal lists for evaluating academic research performance has meant that 'scientists have been forced to downgrade their primary aim of making discoveries to publishing as many papers as possible'.

The pressure to publish in highly ranked journals does not necessarily in itself reduce innovative work, but as we will discuss below, these journals tend to emphasize

incremental gap-spotting research more than innovation and intellectual boldness, at least this seems to be what they publish (Bouchikhi and Kimberly, 2001; de Rond and Miller, 2005; Pfeffer, 2007; Starbuck, 2006, 2009).

Professional Norms within the Management Field

Journals, editors, and reviewers are the main professional norm setters for how research is conducted and what research is published (Baruch et al., 2008). Incremental gap-spotting research is strongly encouraged by the ‘*adding-to-the literature*’ norm within leading management journals (e.g., Johanson, 2007; Pratt, 2009) as the primary evidence for research contribution. For example, based on her 26 years as *Administrative Science Quarterly*’s managing editor and her reading of more than 19,000 reviews and more than 8000 decision letters, Johanson (2007, p. 292) firmly advises authors to adhere to the adding-to-the literature norm because ‘if you can’t make a convincing argument that you are filling an important gap in the literature, you will have a hard time establishing that you have a contribution to make to that literature’. The prevalence of the adding-to-the literature norm is also evident in Miller et al.’s (2009, p. 278) observation that our top-tier journals increasingly force researchers into incremental gap-spotting research as they ‘encourage work on topics that fit neatly within today’s popular theories and allow the development and tweaking of those theories’. Similarly, in a recent special issue on theory development in *Organizational Research Methods*, the guest editor argued that ‘[i]n the interest of theory development, management and organizational research would make better progress if we devoted more attention to theoretical refinement, conducting research that identifies the boundaries and limitations of theories, stages competitive tests between rival theories, and increases the precision of theories so they yield strong predictions that can be falsified’ (Edwards, 2010, p. 615), an argument also proposed by McKinley (2010) and Donaldson et al. (2012).

The strong adding-to-the literature norm in our leading management journals does not necessarily mean that challenging dominant assumptions is excluded or directly discouraged. However, its emphasis on carefully relating one’s own study to existing literature tends to encourage researchers to find gaps and not to move that far away from the established body of work in their specific subfield. Relating one’s study to existing bodies of knowledge in a much more sceptical and consensus-challenging way, perhaps getting inspiration from outside a (sub)field, breaks with the adding-to-the-(sub-specialized)-literature norm.

The demand to meticulously relate one’s study to existing literature is also underpinned by a specific kind of *rigour* upheld within many management journals and strongly advocated by Donaldson et al. in this *JMS* issue. It typically means: (a) a requirement of a systematic and overly pedantic vacuum cleaning of existing literature, as a way to show how one’s own study contributes to existing literature; and (b) an emphasis to carry out empirical research through detailed codification procedures or statistical treatment without asking questions if there is something more fundamentally problematic with existing literature or whether the data really are valuable indicators of the phenomena supposedly addressed. As the at the time outgoing editors of *Journal of Management Studies* observed in their final editorial note: ‘The emphasis on improving the rigour of

theorizing and of empirical method . . . may have led to more incremental research questions being addressed' (Clark and Wright, 2009, p. 6). Donaldson et al.'s push for using rigour methodologies, such as mathematical and causal modelling in theory development is therefore likely to further amplify, rather than reduce, the shortage of high-impact research in the management field.

Incremental gap-spotting research is further driven by the increasing tendency amongst academics to *pigeonhole* themselves (and their subject matter) into narrow and well-mastered areas. Such pigeonholing helps to boost their productivity and to meet academic performance criteria in the sense that: one knows the literature, goes to the right conferences, cultivates a network of people that matters, is familiar with the norms and rules of the journals in the sub-area, and therefore is capable of successfully publishing incremental contributions regularly. But the likelihood of generating frame-bending and high-impact research through such pigeonholing is typically low. In particular, there are often (a) strong expectations (amongst reviewers and editors) that people working within a specific sub-field, cite a significant part of all the work within it, and (b) limited space, energy, and tolerance for bringing in literature from outside the sub-field, as a way to open up new areas of inquiry (Bourdieu, 2004; Starbuck, 2003). Sometimes this pigeonhole thinking comes through very strictly. One of us received the following reason from a reviewer for why his paper should be rejected: 'I'm just not convinced that this paper works as a piece of leadership research that can be satisfactorily situated within existing approaches and debates.' But perhaps innovative research does not easily situate itself within the existing literature in a specific sub-field but breaks out of and challenges it.

Gap-spotting research is also promoted by the strongly-held *accumulation* norm in social science that knowledge is supposed to advance through incremental accumulation within a particular field. As Litchfield and Thomson, the founders of *Administrative Science Quarterly*, state in their vision of the field of organization studies: 'scholars should build a cumulative, comprehensive, general body of theory about administration' (Palmer, 2006, p. 537). This accumulation norm continues to dominate. For example, in its criteria for publication, *Academy of Management Journal* stipulates that 'submissions should clearly communicate the nature of their theoretical contribution in relation to the existing management and organizational literatures'. Similarly, *Journal of Management Studies* says that its main criterion for publication is that a submitted paper should contribute 'significantly to the development of coherent bodies of knowledge'.

The accumulation norm tends to reinforce the gap-spotting logic by requiring researchers to adopt a systematic, analytical, and often narrow focus, which makes them unable to ask more fundamental and sceptical questions that may encourage some significant rethinking of the subject matter in question. The accumulation norm also gives an impression of a collective project signalling reason, progress, and may work as an antidote to a lurking feeling that social research has strong elements of subjectivity, arbitrariness, and relativism (Pfeffer, 1993). Hence, gap-spotting research may not only be used to legitimate a specific piece of research but also the scientific project itself and, thus, preserve and reproduce knowledge accumulation as a fundamental scientific ideal; despite it being untenable as shown by Kuhn (1970) and scholars emphasizing the multi-paradigmatic and contested nature of social science (Burrell and Morgan, 1979; Delanty, 2005).

Closely related to the accumulation ideal is the *crediting* norm, which stresses the need to build on and acknowledge the work of other scholars. Although citation is vital in research publications, there is an increasing expectation to vacuum-clean a narrow field and cite almost everything within it. This is the case even if it makes the text more disrupted and harder to read and the references do not add anything. As Gabriel (2010, p. 764) observed:

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 revisions, accepting virtually any criticism and any recommendation with scarcely any complaint, all in the interest of getting published.

It is also a strong requirement from journals to cite work that has been published by them as a way to increase their impact factor and ultimately for getting published. For example, both authors, who recently published articles in a top-tier journal, discovered in the proofs that the journal editor had inserted references from their own journal without permission from the authors. While this kind of ‘coercive citation’ is occurring across the board it is considerably more common in management journals (Wilhite and Fong, 2012, p. 543).

Journals function as a strong disciplinary regime – as such it is a mixed blessing. On the whole, the quality-reinforcing elements are the most prominent and journals also encourage and demand a degree of innovation and novelty. Clearly, papers benefit from reviews and revisions many times, and journal articles have probably been improved in some respects over time as a consequence of stricter journal regimes. But increasingly detailed monitoring and expectations that authors must comply almost fully with the demands of reviewers and editors are sometimes counterproductive. To develop original ideas and engage in independent thinking is counteracted by a demand to ground everything that is being said in ‘existing literature’ in a specific subfield. In principle, it is possible to do both. But time, effort, intellectual focus, and text space typically means that there is a conflict between the norms that demand everything should be tightly connected to literature, data and methodological rules on the one hand, and more imaginative and innovative research efforts on the other. Imaginative efforts often call for less detailed focus on what exists and more discretion to the researcher. In particular, the current focus in many review processes on fault-finding and compliance to reviewers’ and editors’ comments with the aim of making the submitted paper more tightly related to existing literature is likely to produce incremental research rather than encourage the development of novel and challenging ideas (see Bedeian, 2003, 2004; Tsang and Frey, 2007).

Researchers’ Identity Constructions

The above institutional conditions and professional norms exercise a strong normative control over the way research is conducted and reported in research texts. Through a

long and extended socialization into the field, most researchers internalize those norms and conditions and develop what can be called a *gap-spotting habitus* (to partly borrow a term from Bourdieu). By following this habitus, we reproduce its dominance and force others to comply; and because of our doing so, gap-spotting achieves the status of the proper or 'right' way of generating research questions and developing theories within management studies. In other words, we become gap-spotters doing incremental adding-to-the-literature research.

This gap-spotting identity is further reinforced by the fact that many (most?) management researchers seem to take very seriously the demand to publish regularly in the 'right' journals. At least this is what is expressed at conferences and other social interactions amongst researchers. People report that they feel the pressure to publish, otherwise their school may fall back a step or so in the rankings, or their promotion may not be as rapid as otherwise. For many, a strong responsiveness to expectations has become natural and self-evident. Compliance dominates. Management academics are turning themselves into gap-spotting sub-specialists eager to pump out as many journal articles as possible rather than into more genuine scholars, wanting to do really novel, challenging and significant research.

Identity constructions seem to be more about where and how much is being published rather than about original knowledge and unique contributions. Who am I? I am a person who has published in this or that journal. We see indications of this identity construction all the time in author presentations in journals. Here, many people mention affiliation and then emphasize where they have published. As identity markers publication outlets are apparently central. A particularly problematic effect of constructing an identity based on where you publish is that it can easily lead to what Willmott (2011) labelled 'journal fetishism', that is, researchers start to care more about the publication outlet than the actual research contribution (see also Tourish, 2011).

This somewhat perverse and excessive focus on journal publication as identity markers further drives researchers to embrace incremental gap-spotting research and simultaneously downplay more genuine scholarly research – where extensive reading coupled with a familiarity with, and interest in, a wide set of ideas are central. As Barnett (2010) poignantly observed, if a colleague peeks into your office and sees you are reading a book you almost feel embarrassed and guilty; you are supposed to write papers not reading books. Similarly, Gabriel (2010, p. 762) observed that the majority of his colleagues 'read mostly the abstracts and spend relatively little time carefully assimilating detailed arguments, which suggests to me that, for many reading (with the notable exception of reading for the purpose of writing a peer review) has become a less important activity than writing'. This leads to the possibility of academics writing for fellow writers, which are only interested in 'casting their eyes on whatever promotes their own writing agendas'.

It is important not to exaggerate here. Publications in the right journals are not contradictory to broader scholarship and a strong intellectual interest, including curiosity, openness, and a willingness to take some risk and trying to be imaginative and creative. By no means is all work characterized by the latter easily compatible with contemporary journal publications and, in particular, with the requirement to constantly publish papers in prestigious journals. Many intellectual projects require something

broader that does not easily fit into the standard journal format of 8000–10,000 words (although some journals like *JMS* do give extra space for qualitative papers). They also call for an engagement with a broader literature than having a strong focus on a narrow area. One can just think of a Foucault or a Habermas, trying to adapt to the format and criteria of our leading contemporary management journals. Also Burrell and Morgan's (1979) groundbreaking book would be impossible to squeeze into a research career where journal publication is central. We note however that Morgan also published an influential article (Morgan, 1980), partly based on his book with Burrell.

The problem is twofold in that (a) contemporary journal format is not being optimal for all kinds of research and scholarly orientations, and (b) contemporary professional norms are giving too much priority to incremental gap-spotting research, which taken together, foster a journal publication technician rather than a 'genuine' scholar. In particular, incremental gap-spotting research with its typically narrow and instrumental approach contradicts problematization and assumption-challenging and, thus, makes the generation of more novel and influential work difficult. The gap-spotting mode is further reinforced when management researchers together – in reviews, in promotion committees, in career advising, in pub and conference talk – do identity regulation of others and themselves (Alvesson and Willmott, 2002), naturalizing and normalizing publications in 'top journals' (only). A too strong focus on journal outlets partly goes in a direction that undermines the chances of more interesting work getting produced. We, as a research community, apparently foster a gap-spotting mode – not a scholarship mode – as the key ingredient in researchers' identity constructions. Researchers eager and capable to use a broad set of intellectual resources and are imaginative and challenging are a rarity – at least when it comes to appearing in leading journals. As Adler and Hansen (2012, p. 5) noted, 'all too many scholars so reduce the scope of their research that it means little to them beyond its use as a vehicle for getting published and advancing their career. What keeps so many professors from doing the research that would matter the very most to them, personally and professionally?'

The Relationship between Institutional Conditions, Professional Norms, and Researchers' Identity Constructions

The victim-of-the-system explanation. It is possible to see the interplay between institutional conditions, professional norms, and researchers' identity constructions as a tight system highly difficult to break away from. One line of argumentation would thus be to emphasize the connections and mutually reinforcing effects of the three key drivers behind the prevalence of incremental gap-spotting research. Institutions emphasize rankings, journals and academics eager to be successful (otherwise facing material and symbolic consequences) strive for ranking improvements. The identity projects (and narcissism) of academics are strongly reinforcing the effects of instrumental pressures and material incentives. People (we) are increasingly caught up in the rankings and differentiations: to be a good academic means to publish in A-listed journals, and you do whatever it takes to get published in those journals.

For most people, such a tightly regulated system makes it almost impossible to spend several years writing a really innovative book (or even a set of papers). Instead, academics

are furiously trying to publish in A-listed journals, whose grip over the researcher's time, focus, and self is being reinforced. Performing less well on this one-dimensional scale means researchers are jeopardizing their academic career possibilities – and perhaps their egos. Many (most?) researchers struggle to meet even a modest level of success. At some places, performance monitoring and resource allocation simply means that without a steady flow of journal publications, tenure may be at stake and/or teaching load will increase, money for conferences and books will dry up, and it will be difficult to find time and support to do ambitious research. In order to survive (or at least succeed) in such a tightly regulated system the researcher is more or less forced into incremental gap-spotting research in a highly specialized area.

While the 'victim of the system' explanation for the shortage of interesting and influential theories in management studies intuitively makes sense, it is quite partial and in many places the system is neither so tight nor so constraining. It is perhaps surprising how little protests there have been of this 'evil' system, despite frequent complaints in conversations among people. The situation seems to persist, even if it seems undesirable, partly because of more or less voluntary reproduction of it but also because it has many winners who are reluctant to change it. As Starbuck (2006, p. 94) noted, when such a perverse situation persists, it is almost always when 'someone is benefiting from the situation. So who are the major beneficiaries of non-progress in the development of knowledge?'

In the management field, it seems to be almost all involved, as long as they are on the successful end of the scale. It provides deans, at least at research-oriented and reasonably successful schools, with a powerful tool to control and monitor the research performance of faculty. Leading journals receive increased submissions and status through high impact factors. The careers of some successful researchers are boosted. PhD students get clear rules for how to operate their careers. At the same time, all suffer in various ways from the constraints.

The strongest winners are probably advocates of a neo-positivistic research agenda (such as the one promoted by Donaldson et al., arguing for more rigorous incremental studies). Here the use of the conventional journal format fits nicely. A standardized format, map-and-fill-the-gap and aim for knowledge accumulation by positively adding to earlier work without too many complications, are important components of such a research paradigm. Once again, journal format or standards for how to write do not exclude other contributions, but working within other traditions, like rich ethnographic studies, are not easily packed into the 8000–10,000 words format.

We are in-the-charge-of-the-system explanation. Above, we made a case for how the three ingredients (institutional conditions, professional norms, and researchers' identity constructions) can be seen as a tightly coupled system forcing management researchers (as victims or beneficiaries) into incremental gap-spotting research. But one can also argue for a less deterministic view and much looser connection between the three ingredients. Governments and central university administration are not in themselves particularly preoccupied with specific forms of research – and would probably applaud signs on great innovations and high impact results – but are more concerned with getting value for money, aid in resource allocation, and creating an impression of rational control over the spending of the tax payers' money and so on.

If professional groups decided to upgrade assumption-challenging studies and down-play consensus-seeking adding-to-the-literature research, this would not go against regulatory bodies' need for finding ways of spending resources in a reasonable way and getting some indicators of how various universities, schools, and research groups are performing. In the UK, for example, the research assessment review committees are made up by academics, which have high discretion in evaluating institutions. Similarly, journal editors have extensive discretion about what publication policies the particular journal should embrace. They could therefore make policies that encourage imaginative studies rather than merely incremental, consensus-based research. And most researchers have often considerable discretion when it comes to how they can shape their career. For example, not all people are striving to get tenure at a very prestigious university. Even those who do are only subordinated to do 'whatever it takes' for a short period of time before they get tenure (or move to another place). Research active academics are tenured during most of their working life, and many have more or less guaranteed time for research in their contracts. Some researchers are also diligent and gifted enough that they, without too much effort, can reach the minimum number of publications required and can therefore afford spending extensive time on more innovative projects.

One could actually reverse the top-down logic and argue that it is *not* institutional arrangements – rankings, funding, performance pressure from the top – that drives the process downwards, but that it works the opposite way. It is academics – through their choices and priorities – that establish and revise norms, and form journals (as authors, reviewers, editors, members of associations running the journals), and probably have the strongest impact on how universities and professional institutions actually do their assessments. Researchers as individuals and collectives are in significant ways in charge of how research should be conducted and decide what research counts as valuable and should be published. The major problem is hardly that, as one often hears, writers are good and evaluators are not. (We) writers are as bad as (we) reviewers and editors – they (we) are the same persons (although the reviewer position may sometimes pull out the worst in people (us), as Gabriel, 2010 remarks).

We are exercising concertive control over ourselves, voluntarily building our own constraining (and seductive) rules and norms, and willingly giving up a lot of possible discretion (cf. Barker, 1993). After all, who are producing the research texts? Who are giving the feedback and the recommendations and decisions for what papers and books should be published and how research texts should look? We all do. As researchers we decide about our own journals. And as a collective we control the norms for good research and, thus, to a considerable degree, form, bend, and translate how governments and others institutions' policies influence the research practice.

There are of course limits to our discretion and there is a complicated structure–agency set of relations involved. Institutionalized arrangements have strong reproductive tendencies and established rules of the game are not always so easy to change from below. Similarly, centralized moves, such as a highly differentiated research funding based on quantitative output performance sometimes have drastic effects (Adler and Harzing, 2009). But institutional policies in themselves do not mean discrimination of imaginative, consensus-challenging work as long as this is carried out productively. Nor is consensus-challenging work necessarily more time-consuming to carry out than

consensus-confirming studies. But it is difficult to come up with and develop good ideas if there is a strong focus on getting all technicalities right, associated with adding-to-the-literature research. It is difficult to fully master a narrow sub-speciality *and* read broadly and variedly in order to get new ideas and break out from the sub-speciality box.

What above all the ‘being-in-charge-of-the-system’ explanation suggests is that – if only researchers want – there are ample opportunities to put management studies back on track again. Below we point to how specific changes in institutional arrangements around governance of research, professional norms, researchers’ identities, and research methodologies can reduce the serious shortage of interesting and influential studies.

PUTTING MANAGEMENT STUDIES BACK ON TRACK: WAYS OF ENCOURAGING INNOVATIVE AND INFLUENTIAL RESEARCH

The near omnipresent requirement to continuously publish in ‘high-quality’ journals has meant that most management researchers have lost sight of, or strongly downplayed, the most overriding goal and ultimate purpose of management studies, namely to create and produce original knowledge that matters to organizations and society. In other words, it is not paper production per se that is most important but the creation and production of knowledge that is important and influential. Journal publication is a *means* for facilitating the development, quality assurance, and communication of new knowledge, not an end in itself. It can be an excellent means with many advantages, but as emphasized here, there are currently significant problems. Therefore, the most important issue for getting management studies back on track is to shift away from the current focus on paper production to the production of more innovative and influential ideas and theories that can make a significant difference to both theory and organizational practice. Encouraging such work requires a substantial rethinking and reworking of institutional conditions, professional norms, researchers’ identity constructions, and methodologies for theory development.

Revising Institutional Conditions

Governments. The primary way in which governments influence research is through their specific research assessment reviews and their focus on the number of publications in A-listed journals during a specific period of time. However, using such an assessment formula as the chief indicator for academic research performance and quality is marred with difficulties, particularly as it strongly encourages incremental gap-spotting research. There is also a weak relationship between influential studies in the sense of citation impact and where they are published (Adler and Harzing, 2009; Glick et al., 2007; Pfeffer, 2007; Singh et al., 2007). As Pfeffer (2007, p. 1342) noted, the research on citation counts ‘illustrates that a shockingly high proportion of papers, even those published in elite journals, garner *zero* citations, with an even larger percentage obtaining very few’. However, governments can rectify most of the above problems (and better support a more scholarly research mode) by changing and broadening the criteria for assessing academic research performance. One of the most important changes would be

to put a significantly higher emphasis on *citation count* as an indicator of research performance. This would stimulate stronger efforts to produce more innovative and influential studies, even if productivity would suffer.

Using citation count as a performance indicator has of course its own problems (e.g., Adler and Harzing, 2009; Grey, 2010; Starbuck, 2009). For example, there are many exclusive clubs of authors who mainly cite each other and rarely other authors outside the specific club (Macdonald and Kam, 2010). Writings on fashionable topics may get undeservedly much attention. Method and review papers are sometimes cited more than theoretical and empirical studies. But still, citations say a lot of what is viewed as interesting and significant.

Another important step that governments can take to encourage more innovative and influential work is to *broaden the publication outlets*. Instead of primarily relying upon a designated journal list, other outlets can also be included, such as books, book chapters, and practitioner oriented journals and magazines. This would take away the emphasis to (only) publish frequently in prestigious journals and allow for less narrow and standardized work.

Revising universities' and business schools' policies. The abovementioned ideas could also influence what is being done within universities and schools. For example, hiring, tenure, and promotion committees could put a stronger emphasis on citation impact, and on research that has been published not only in a designated journal list but also in other outlets, such as books and book chapters.

One can also reconsider the often too *narrow time frames* in which academics are expected to publish a certain amount of articles. For example, in the Australian business school context, it is not uncommon that researchers are supposed to publish at least two articles in prestigious journals over a two-year time interval. Such short time intervals further encourage researchers to engage in safe and predictable gap-spotting research. As many have pointed out, such productivity measures tend to encourage incremental research and 'too often restating the obvious (Bedeian, 1989; Boyer, 1990; Denning, 1997)', while hampering more innovative research (de Rond and Miller, 2005, p. 322). This is something further confirmed and elaborated in McMullen and Shepard's (2006) study. It shows that a strong pressure to publish a certain number of articles within a short timeframe coupled with a risk of getting punished (increased teaching load etc.), significantly discourage not only junior but also more senior academics to engage in more consensus-challenging research.

Another policy change that may encourage the development of more innovative and influential research is to *counter narrow instrumentalism*. It can be done in several different ways, such as institutionalizing less rapid promotion, reducing extrinsic rewards by counteracting title inflation (perhaps reserving the position of full professor for those that have made significant contributions, rather than emphasizing the quantity of publications) or payment by journal publication, and using broader research criteria for employment, tenure and promotion, including demands for variation in research topics and methods plus variation in publication outlets. Business schools could also discourage overspecialization and a strong emphasis on productivity by comparing publications and disqualify cases where texts overlap with more than say 50% (This can easily be done

through computer programs and form a routine part of performance monitoring and promotion evaluations).

Schools and departments could also reduce the dominance of incremental gap-spotting research by nurturing a more reflective scholarly orientation and consensus-challenging research through *training and workshops*. For example, instead of mainly cultivate academics as paper authors for journals, more training and workshops on questioning assumptions; creative writing, writing for a broader audience, and encouragement of research book publications are needed. Needless to say, we are not arguing against journal publications – it is a key quality improvement and assurance resource tool and major outlet for research – but to vary intellectual work and give space for contributions less easy to shoehorn into the (contemporary) standard journal format.

Rethinking Professional Norms

Except for the institutional arrangements instigated by governments, universities, and departments, there is a need to rethink professional norms, in particular in relation to journal publication. As outlined above, a highly peculiar norm that has spread rapidly is that authors should comply with almost all of the reviewers' requests. This is the case even if, as happens many times, the comments from one reviewer are highly inconsistent with the comments from another reviewer. We think the norm for such a strong adoption to reviewers' comments needs to be de-emphasized. Still, most submissions need to be rejected – and we often think that too much is published, even in A-listed journals. But one could imagine journals *upgrading innovative and original ideas* (a consequence would be rejecting many more papers than now on that criterion) and then let authors treat reviewers' comments as collegial advice for how the paper can be improved rather than strict instructions for what to do.

In many or most areas, there is of course a shortage of papers with really good ideas. Therefore, given the space to fill, the use of list-like sets of criteria for what is acceptable (clearly written, sufficient gap-spotting, extensive literature review, conservative anchoring in established method, and a lengthy method section indicating rationality and rigour, a lot of data summarized, modest contribution, and call for more research) may be the only option that seems possible for journal editors. And this is sufficient for getting acceptable papers with incremental contributions. But if one is interested in more imaginative and novel studies, perhaps using checklists for faultfinding should be de-emphasized.

Another criterion for evaluating submitted papers that needs to be reconsidered is the conventional notion of *rigour*, requesting researchers to systematically vacuum clean existing literature to demonstrate how their own study makes a contribution to that literature. This kind of rigour is often used as the prime guillotine for rejecting a paper in the review process, often justifiably so, but it may work against really innovate and interesting ideas. Rigour and imagination can of course be combined (e.g., Cornelissen and Floyd, 2009; Donaldson et al., 2012; Weick, 1989). However, while conventional rigour in the sense of logical consistency and thoroughness is always important and can assist creativity, it typically encourages a refinement of existing theories rather than a development of more frame-breaking theories, as is evidenced in Donaldson et al.'s

paper. Yet as an addition to conventional rigours thinking one could emphasize the need to identify and challenge assumptions. In other words, as part of the standard journal policy, it can be requested that authors need to carefully consider the assumptions underlying existing literature, and how those assumptions shape the understanding and conceptualization of the subject matter in question, thus demonstrating reflexivity as a key quality of rigorous thinking (Alvesson et al., 2008). But it may be better to relax the emphasis on 'rigour' – there are other and more balanced ways of pointing at ideals for good research, including interestingness.

Cultivating a More Scholarly Identity: From Gap-Spotter to Path-(Up)Setter

Although changes in government, university, and journal policies like those discussed above are important for reducing the shortage of high-impact research, they are only partly helpful because, at the end of the day, it is we academics who decide what we do and how we do it. The impression from the studies and comments discussed previously is that gap-spotting researchers – at least those who make it into highly ranked journals and therefore 'count' – are not just intelligent, rigorous, diligent, and methodologically and theoretically well trained, but also cautious, instrumental, disciplined, career-minded, and strongly specialized. This gap-spotting identity is, to a degree, difficult to avoid and not entirely negative.^[3] But against this, one could put forward more genuine scholarly values and qualities like being intellectually broad-minded, independent, imaginative, willing to take risks, enthusiastic about intellectual adventures, and frequently provocative. This would imply giving priority to discretion and integrity and doing meaningful research that matters rather than prioritizing tenure at a top university, rapid promotion, and publishing in the most prestigious journals. This is something also advocated by Rynes (2007, p. 1382) in her concluding note of an *AMJ* editorial forum 'on looking back and looking forward on management research', where she argued that management researchers should have a 'higher purpose beyond simply getting another "hit" in a top-tier journal'. Instead, researchers should be 'committed to . . . ideas we care about rather than focusing on what our publications will do for our image, our compensation, or our careers'. That is, we need less instrumental gap-spotting and publication-prioritizing sub-specialists working for a long time only within one area, and more researchers with a broader outlook, curious, reflective, willing and able to question their own frameworks and consider alternative positions, and eager to produce new insights at the risk of some short-term instrumental sacrifices, that is, a more critical and path-(up)setting scholarship mode. Such 'scholarly research reflects our pressing and irreversible need as human beings to confront the unknown and seek understanding for its own sake. It is tied inextricably to the freedom to think freshly, to see propositions of every kind in an every changing light. And it celebrates the special exhilaration that comes from a new idea' (Boyer, 1990, p. 17).

In order to win back and cultivate a more critical and path-(up)setting scholarly attitude amongst management researchers, cultural and identity issues need to be directly targeted. Even if journals should adopt and try to implement an upgrading of interesting work at the expense of technical excellence, the success of this is almost

entirely dependent on a sufficient number of good researchers defining themselves and their work in a more scholarly fashion. This is a task for all of us in academia. It is partly a matter of cultivating a specific self-understanding – done through research choices, reflexive exercises, thoughtful (and not mainly gap-spotting instrumental) use of networks, collaborations, etc. and partly in our capacity as PhD advisors, colleagues etc. to influence others.

There is of course an endless number of ways of doing so. We will only give one example. In terms of seminar presentations, why only invite people to give a paper? Perhaps visitors could be asked to present and discuss a very interesting book or an article they have been inspired by lately, informing their own research or general line of thinking. As Gabriel (2010) notes, reading and discussing texts are increasingly marginalized – and a continued de-focusing on books can be seen as deeply problematic, calling for countermeasures.

A Need to Consider Alternative Methodologies for Theory Development

We started by emphasizing how the dominant gap-spotting logic of adding-to-the-existing-literature leads away from the development of interesting research questions and theories. But perhaps we can work with methodologies that more directly stimulate new and challenging ideas and contributions? Such methodologies can support the researcher identity we argue is needed to get management studies back on track again. There are no guarantees, of course, and most research is almost per definition not capable of accomplishing something that is seen as really interesting, as this is something that clearly stands above the average or normal. But more than what is currently accomplished is surely possible and we think one can point at research methodologies that are less focused on gap-spotting and incremental contributions.

On a meta-theoretical level it is possible to point at some key ingredients for how to think about research in a way that contradicts incremental gap-spotting ideals and instead emphasizes assumption-challenging in both the construction of research questions and working with empirical studies (Alvesson and Kärreman, 2007, 2011; Sandberg and Tsoukas, 2011). Although we are not alone in trying to deal with this (for other examples, see Abbott, 2004; Becker, 1998; Davis, 1971, 1986; Smith and Hitt, 2005; Starbuck, 2006), we bring our own methodologies forward here as they are grounded in organization studies and specifically designed to generate more innovative and influential theories. In particular, we bring forward two such methodologies: using problematization as a methodology for challenging assumptions, and using empirical material for challenging assumptions underlying existing literature.

Using problematization as a methodology for assumption-challenging studies. As we pointed out above, the overall majority of contemporary publications use (or at least communicate) a form of gap-spotting and gap-filling as the overall research logic. An alternative to this is to formulate research questions by problematizing some dominant assumptions in existing research (Davis, 1971). As we see it, formulating novel research questions through problematization involves not just using a particular preferred meta-theoretical standpoint in order to challenge the assumptions of others (as is often the case in the

paradigm debates) or as in various applications of critical perspectives (Alvesson and Sandberg, 2011). This ready-made or pseudo 'problematization' only reproduces the assumptions of the framework inspiring the researcher and is unlikely to lead to particularly novel and interesting ideas (Sandberg and Alvesson, 2011). 'Real' problematization also involves questioning the assumptions underlying one's own meta-theoretical position. The ambition is of course not to totally undo one's own position, but only to unpack it sufficiently so that some of one's ordinary held assumptions can be scrutinized and reconsidered in the process of constructing novel research questions.

As we argue elsewhere (i.e., Alvesson and Sandberg, 2011, p. 252), the aim of the problematization methodology 'is to come up with novel research questions through a *dialectical interrogation* of one's own familiar position, other stances, and the domain of literature targeted for assumption challenging'. This approach supports a more reflective-scholarly attitude in the sense that it encourages the researcher to start 'using different standard stances to question one another . . . [and combining them] into far more complex forms of questioning than any one of them can produce alone' (Abbott, 2004, p. 87).

To be able to problematize assumptions through such a dialectical interrogation, the following methodological principles are central: (1) to identify a domain of literature; (2) to identify and articulate assumptions underlying this domain; (3) to evaluate them; (4) to develop an alternative assumption ground; (5) to consider it in relation to its audience; and (6) to evaluate the alternative assumption ground. Successful problematization is of course also very much a matter of creativity, intuition, reading inspiring texts that offer critical insights (but without being accepted as a new fixed framework), talking to other people, having specific experiences, or making observations that may trigger new thinking etc. Although we have no strong belief in rational, logical, or mechanistic procedures for problematization, we do think some structure can be helpful. The problematization methodology also has the advantage that it facilitates focus, can work as a support for a research identity around being a problematizer (and not a gap-spotter), and can facilitate description of what one has done and accomplished. This methodology is extensively developed and exemplified in Alvesson and Sandberg (2013).

Creating and solving mysteries in empirical research. A second methodology for challenging dominating theoretical ideas is the use of empirical material. Unlike many others with a strong faith in the robustness of data (like quantitative or grounded theory methodologists, celebrating discipline and diligence rather than imagination), we claim that data, or – our preferred term – empirical material, are simply not capable of showing the right route to theory or screening out good ideas from bad. As we see it, the interplay between theory and empirical material is more about seeing the latter as a source of inspiration and as a partner for critical dialogue, than as a guide and ultimate arbitrator. Acknowledging the constructed nature of empirical material – which is broadly accepted in the philosophy of science (Alvesson and Skoldberg, 2009; Denzin and Lincoln, 2000; Gergen, 1978; Kuhn, 1970) – has major consequences for how we consider the theory–empirical material relationship and calls for giving up the old idea of data and theory being separate.

Crucial here is to challenge the value of an established theory or a framework, and to explore its weaknesses and problems in relation to the phenomenon it is supposed to explicate. It means to generally open up, and to point out the need and possible directions for rethinking and developing it. We consequently suggest a methodology for theory development through encounters between theoretical assumptions and empirical impressions that involve breakdowns. It is the unanticipated and the unexpected – the things that puzzle the researcher – that are of particular interest in the encounter. Accordingly, theory development is stimulated and facilitated through the selective interest of what does *not* work in an existing theory, in the sense of encouraging interpretations that allow a productive and non-commonsensical understanding of ambiguous social reality. The ideal research process then includes two key elements: (a) to create a mystery, and (b) to solve it (Alvesson and Kärreman, 2011; Asplund, 1970).

The empirical material, carefully constructed, thus forms a strong impetus to rethink conventional wisdom and to find input to a possible rethinking of something, becoming less self-evident and instead surprising and calling for new ideas. However, the ideal is *not*, as in neo-positivist work, to aim for an ‘intimate interaction with actual evidence’ that ‘produces theory which closely mirrors reality’ (Eisenhardt, 1989, p. 547).^[4] This is an effective hamper of imagination as reality-mirroring means low-abstract and trivial results. Chiefly, our goal is to explore how empirical material can be used to develop theory that is interesting, rather than obvious, irrelevant, or absurd (Davis, 1971). But this calls for a more active construction of empirical material in ways that are interesting, and not just waiting passively for data to show us the route to something interesting, as is typically the case in more conventional research. For example, careful work with data as in grounded theory is hardly sufficient to trigger imagination and lead to really novel and challenging ideas (Alvesson and Sköldberg, 2009). Of course, all this calls for some relaxation of the pressure for the required standard in incremental gap-spotting research that emphasizes rules, mechanics, and data management. For an extensive description and exemplification of this methodology, see Alvesson and Kärreman (2011).

The two methodologies proposed imply a somewhat different researcher identity than the common one. Both methodologies call for drawing upon a broader set of theories and vocabularies as resources for challenging dominant assumptions and constructions of empirical material, more emphasis on critical and hermeneutic interpretations, and some boldness in counteracting consensus. This typically means less detailed knowledge of all that has been done within a narrowly defined field, a reluctance to divide up theory and data as separate categories and to address these as distinct parts and sections in a report, and facing some antagonism from defenders of an established position. In short, it calls for a shift of emphasis in researcher identity: from cultivating an incremental gap-spotting research identity to a reflexive and path-(up)setting scholar, with some preferences for irony and promiscuity over a fixed, programmatic position. It also calls for some backup of professional norms, celebrating other ideals than find and fill the gap.

The proposed assumption-challenging methodologies therefore differ significantly from the rigour methodologies (e.g., statistics, mathematical modelling, and causal modelling) in theory development suggested by Donaldson et al. Their rigour methodologies are primarily designed for refining existing theory rather than challenging it. For

example, as they rightly point out, statistics can be applied to detect and reduce errors in an existing theory and causal modelling can be applied to examine ‘the internal coherence of the theory’. As Donaldson et al.’s rigour methodologies primarily focus on refining existing theory rather than challenging assumptions, they seem therefore to exacerbate the problem with the shortage of high-impact research rather than solving it. In contrast, the two methodologies suggested above are specifically designed for identifying and challenging assumptions underlying existing literature and, based on that, develop more interesting and influential theories.

To reiterate, we have in principle nothing against the use of rigour in theory development. Instead, our main point is that while rigour methodologies such as those proposed by Donaldson et al. are important, they are not enough for developing more interesting and influential theories: it also requires that we deliberately try to challenge the assumptions underlying existing literature in some significant ways. This is what the assumption-challenging methodologies described above help us to do.

CONCLUSION

There is a widely spread disappointment with the lack of interesting and influential work in management studies. Several leading journals editors and prominent scholars have made repeated calls and attempts to change the situation but without success. The primary aim of this paper was twofold: (a) to understand why there is such a serious shortage of interesting and influential work in the management field despite the dramatic increase in research during the last decades; and (b) to suggest ways forward for how more innovative and influential studies can be produced. In addressing those aims we have made two main contributions.

First, we contend that the prime reason behind the severe shortage of influential studies is the dominance of gap-spotting research (across theoretical camps) within the field. It is by identifying or constructing a gap in existing literature to be filled that management researchers try to make a contribution. We noted that the prevalence of gap-spotting research is highly surprising given that it is now well-known that what makes a theory interesting and influential is not that it adds incrementally to existing literature, but the extent to which it challenges its assumptions in some significant way. Most importantly, we identified three interplaying key drivers behind this puzzling behaviour, namely how specific institutional policies, professional norms, and researchers’ identity constructions interplay in a way that almost forces researchers into gap-spotting research.

Second, we proposed and discussed how specific changes in those key drivers can facilitate development of more innovative and influential management theories. While the ‘evil’ system of institutional policies for academic ranking, professional norms, and researchers’ identity constructions appears to be near impossible to break away from, we proposed that researchers may not be as much victims of the system as it looks like. This is because it is we, ourselves, who to a large extent are the developers and executors of the ‘evil’ system that forces us into incremental gap-spotting research, leading to the severe shortage of interesting and influential theories in management studies. To blame ‘the system’ for doing incremental, uninteresting research is hardly credible or constructive.

Table I. The main features of a gap-spotting versus a path-(up)setting scholarship mode

<i>Basic features</i>	<i>Gap-spotting mode</i>	<i>Path-(up)setting scholarship mode</i>
Main focus in theory development	Consensus-seeking: theory development through incremental additions to existing literature, and ignorant about own prejudices	Consensus-challenging: theory development by challenging the assumptions underlying existing literature, and strong awareness of own prejudices
Scope	Researchers often pigeonhole themselves (and subject matters) into a narrowly confined and well-mastered area	Researchers often span across areas and theoretical frameworks in their search for new insights
Research outcome	Additive and incremental theories – often dull and formulaic	Frame-bending theories – often seen as interesting and influential, sometimes controversial
Publication outlets	Journals in designated journal lists	Journals, books, book chapters, conference proceedings

As summarized in Table I, we suggested several ways for moving away from a one-sided cultivation of the gap-spotting mode to more actively cultivating a genuine scholarship mode where consensus-challenging rather than consensus-seeking studies are emphasized.

In enabling such a shift, governments need to broaden their criteria for evaluating academic research performance; not only using the number of articles published in A-listed journals but also citation counts, as well as taking into account other publication outlets such as books and book chapters. Universities and business schools need to revise their policies for hiring, tenure, and promotion in accordance with the proposed changes in governments' evaluation practices. Journal editors and reviewers need to reconsider a whole range of professional norms such as 'adding-to-the-literature', conventional views of rigour such as those advocated by Donaldson et al., and pigeonholing, that strongly drive researchers into incremental gap-spotting research. In particular, they need to develop a set of alternative norms that actively encourage less constrained work, where the value of innovative and novel ideas needs to be upgraded and the pressure to adapt to conventional journal format and standards should occasionally be relaxed.

We as individual researchers must also actively cultivate a more critical and path-setting scholarly orientation to research. One crucial step is to engage in critical debates and reflections of what the purposes of research are and how more innovative and influential theories can be produced. A researcher identity engineered to only produce similar-looking journal articles for a limited group of sub-specialists is counterproductive to the ideal of interesting and influential studies, in which assumption-challenging is a key characteristic. Furthermore, in order to cultivate a more path-setting scholarly attitude, we urge management researchers to use and develop alternative methodologies for developing theories with a focus on breaking away from the reproduction of established frameworks. We proposed two different methodologies that are specifically designed to identify and challenging assumptions underlying existing literature and, based on that, be able to develop more innovative and influential research.

NOTES

- [1] Similarly and closely related, but not discussed specifically in this paper, a huge number of scholars, editors, and practitioners have also raised grave concerns that most management research is becoming increasingly irrelevant to management practice (for an overview, see for instance Pfeffer, 2007; Sandberg and Tsoukas, 2011; Van de Ven and Johnson, 2006).
- [2] For a more extensive review and description of the dominance of gap-spotting research in management studies, see Alvesson and Sandberg (2011) and Sandberg and Alvesson (2011).
- [3] It is of course necessary to have a reasonable knowledge of the area one is working within and do some positioning in relation to at least significant existing studies.
- [4] Neo-positivism (or post-positivism) assumes the existence of a reality that can be accurately apprehended, the observer and the observed be separated, and data and theory be treated as separable, although the theory-ladenness of data is acknowledged. The aim is to produce generalizable results (Lincoln and Guba, 2000). Most contemporary quantitative social research and qualitative research like grounded theory (although there are different versions of the latter; Charmaz, 2000) appear to be based on neo-positivist assumptions.

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