**Challenges and Controversies in Management Research**

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**Introduction**

Britain’s public sector debt currently stands at a record levels and at a time when the higher education sector thought it would be continuing to expand it suddenly is faced with significant retraction bringing with it huge challenges. Management academics have long known that change rarely takes place in ordered ways; instead it is pain and impending crisis that forces organisations to act. The potential loss of funding to universities, research councils and other bodies could herald a crisis, but it might also offer an unlikely opportunity to change the way we think about management research.

For some time now a number of academic studies have considered management teaching and research as having lost its way, become disconnected from practice and conducted research for the benefit of the academy rather than for society as a whole. Structural changes have also impinged on our profession and reviews (cf. ESRC Demographic review of the UK Social Sciences, 2005 and AACSB Sustaining Scholarship in Business Schools, 2003) all point to management research as a field that faces a number of significant challenges. This chapter raises a number of contemporary issues facing the term management and business schools in general and relates these to the challenges they pose for the conduct of research. Although these challenges are those faced by the social sciences more generally they relate particularly to management and business. This chapter is structured around what we see as the four key challenges facing management researchers:

* Our contribution to meeting global *Grand Challenges*
* Renewed impetus in the *Relevance Debate*
* Challenging the *methodological orthodoxy*
* Plugging the *demographic gap*

A final concluding section offers perspectives on *rethinking management research*.

**The Contribution of Management Researchers in Meeting Global Challenges**

High profile statements of the challenges facing humanity itself such as the UN Millennium Goals and the Copenhagen Consensus have begun to influence the policies of Government agencies. Amongst these are the Research Councils and the resources they command. These are being increasingly ordered with respect to a number of broadly-conceived needs. In its 2009-14 Strategic Plan the Council of the Economic and Social Research Council (ESRC) placed a strong emphasis on innovation in research and training, and on the need to foster purposeful interdisciplinarity to address what the considered are increasingly complex, and multi-faceted research questions. The Council identified seven interdisciplinary challenges facing social science:

* global economic performance, policy and management
* health and wellbeing
* understanding individual behaviour
* new technology, innovation and skills
* environment, energy and resilience
* security, conflict and justice
* social diversity and population dynamics.

The pervasiveness of management in our personal and social lives – argued by some to be a transformation force in society similar to engineering in an earlier period - means that *managemen*t is necessarily a component of interdisciplinary programmes that crystallise around global challenges. Evidence that new thinking is needed is provided by Gary Hamel who recently convened (2009) a group of leading US management scholars and practitioners to articulate ‘Management’s Moonshot’; step-changes in management to enable organisations to respond creatively and meaningfully to a changing world. The list of 25 challenges that this group proposed (Hamel, 2009) emphasised the importance of revealing the human side of organisations with everything that means for unlocking individual creativity. Top of his list was [ensuring] ‘that the work of management serves some higher purpose’; a re-orientation towards socially-significant goals (Hamel, 2009: 94).

In such a rhetorical context the question of how such management should be understood and judged remains far less certain. It is, we argue, to pursue this understanding and judgement that the field of management research is devoted, a field that, from tradition, has been dominated by a number of disciplines from the social sciences. Amongst those who study managers, the activity of management and the wider organizational structures and effects of managerial endeavour, gaining agreement across the disciplines has proved difficult to reach. Psychologists, sociologists, mathematicians and anthropologists each have their own ways of deciding the problematics to be studied and how knowledge can be gained. Thorpe and Holt (2008) argue that these traditions, paradigms, worldviews and tools cast the character and influence of management and managerial problems in particular forms. So, for example, where some researchers emphasize an overtly technical understanding of management as though it were akin to social engineering or eugenics, others emphasize its inherently political nature. Whether managers should be seen as being equivalent to overseers, or designers (Pandza and Thorpe, 2009), or even artists, is open to constant debate. Some researchers may see the role as having an inherently positive role in society whereas others remain suspicious of or antagonistic to its influence. Similarly, where some researchers might argue that what they are studying are the cognitive patterns associated with managerial judgement and decision taking, others regard the appropriate unit of analysis to be wider forces such as the structural influence of foreign direct investment, or the cultural influence of prevailing norms. This variety of perspectives and approaches in the field can make any attempt to locate the edges of management research activity a messy one. In order to make sense of this ambiguity Thorpe and Holt (2008) invoke Whitehead’s (1929: 14; 23) approach to informing human inquiry as particularly useful. He see the influence coming from any form of broadly scientific activity as not inherently despotic in its nature, but rather a self-control emerging from the capacity to see things anew, to envisage how the world is and so how it can be both different from the way it is, and better. This ability to see things anew is what Whitehead sees as being at the root of good science, irrespective of the field or discipline. So for a social science such as management this will involve researchers recognising the intimate relationship between their perspectives of the on-going problems experienced by people engaged in managing. Social science requires researchers to engage in ongoing relationships with the human experiences that form the raw material for the data from which they will make sense of the social world they study. The focus of their work can be the problems of managers or their colleagues, or those under the influence of managers, including the researchers themselves. As life moves on, so the problems will change. As a consequence, the problems are not fixed, universal nor entirely tractable and as social scientists the researcher’s role requires them to reflect and attempt to make sense of this. It is only through the process of recognising this relationship they have with the phenomena they research that management researchers will understand the kind of control that Whitehead talks about because it is only from this recognition that management can be understood in terms of its potential rather than simply its bare existence.

We might consider then how has management research preformed hitherto in relation to such practical considerations? To what extent have the findings of management research been judged as relevant to the needs of practicing managers, and it is to these questions that we turn in the next section.

### Renewed Impetus in the Relevance Debate

In 1992 the Harvard Business Review debated the "complex case of management education" (Linder et al., 1992).  One thread of this debate was the contribution played by management research. The conclusion was that both practitioners and professors alike were in agreement that it was usually minimal.  Harold Leavitt wrote ".. a lot of business school research is overly esoteric and done with more of an eye to academic peers than to utility.  I fully agree that much business school research (like all research) turns out to have been a waste of effort". The persistence of this debate, and the relative absence of the same debate and problem in other sciences (Fuller, 1999) suggests that it is perhaps time for management to put its house in order and time is not on the side of management research.

One year later, Donald C Hambrick used his presidential address to the Academy of Management to issue the following call to arms (Hambrick, 1994):

*"Colleagues, if we believe highly in what we do, if we believe in the significance of advanced thinking and research and management, then it is time we showed it.  We must recognise that our responsibility is not to ourselves, but rather to the institutions around the world that are in dire need of improved management, as well as those individuals who seek to be the most effective managers they possibly can be.  It is time for us to break out of our closed loop.  It is time for us to matter"*.

 (Hambrick, 1994)

Strong words that still seem current and appropriate today.

In the UK, this debate took a decisive step forward with a series of debates under the auspices of the British Academy of Management on the nature of management research. Tranfield and Starkey (1998) were centrally involved and their work concluded with a series of propositions for the management research community to debate.  The sixth and final proposition continues to strikes a resonant chord:

*"A distinguishing characteristic of management research is that it engages with both the world of theory and the world of practice.  Management researchers can locate themselves at different points in the cycle but they cannot stay fixed in either the world of practice or the world of theory.  The problems addressed by management research should grow out of the interaction between the world of practice and the world of theory, rather than out of either one alone"*.

(Tranfield and Starkey, 1998).

Following the debates the British Academy of Management took up the question of relevance and has made it a cornerstone of its mission.  A later report commissioned from Starkey sought his elaboration of the policy implications of his paper (an abridged version of this report was published in 2001 (Starkey et al., 2001)).  In addition, a special edition of the British Journal of Management was convened (Hodgkinson, 2001) to debate the ideas raised by Starkey and colleagues.  This discussion has again been recently revived in the Journal of Management Studies (Fincham et al., 2009). This rather briefer encounter was stimulated by Kieser and Heiner’s claim that the rigour-relevance gap is actually unbridgeable (Kieser *et al.*, 2009). Applying systems theory, they suggest that worlds of management practice and management research are entirely distinct, with the implication that communication systems cannot be integrated or converted. On the other hand, other academics (e.g., MacLean et al., 2002) recognise that differences between the two spheres exist but the main issue is that academics and practitioners value different types of theory; nevertheless, there are still areas where differences are not as large as thought and where development of high quality research and outputs has been offered (Hodgkinson and Rousseau 2009). Starkey's own response (Starkey *et al.*, 2009) to Kieser allows him to extend some of his earlier arguments, and most crucially to our mind "re-imagine relevance as a necessary condition for rigour"; an argument to which we’ll return at the end of this chapter.

From a practitioner perspective there is an irony in a response to the challenge of relevance that involves an endless debate amongst academicians. Hodgkinson and Rousseau (2009) are undoubtedly correct in their assertion that relevant management research “is already happening”, but such endeavour has most certainly not entered the mainstream even though changes are now in train through the Research Excellence Framework that may force a re-orientation of management research towards a discipline more relevant to practitioners.

The increasing importance of university research in the economic development of national economies is a significant theme in the management literature (Etzkowitz, 1997). A similar thread has been evident in UK Government’s innovation policy during the first decade of this century (e.g. BIS 2009). Such policy development has culminated in changes to the burden placed upon universities by funding bodies to articulate the societal and economic impact of their research. Since May 2009 Research Councils in the UK require researchers to produce a two page “Impact Statement” in support of new grant proposals. This requirement places the onus on researchers to outline who will benefit from their researches, how these benefits will be realised, and what actions will be undertaken in order to connect potential beneficiaries with research outputs. More far-reaching still in its consequences, is the consultation document issued by the Higher Education Funding Council for England (HEFCE, 2009) with respect to its proposals for the *Research Excellence Framework* (REF). This suggested that the 2012 assessment of university performance should take into account considerations of research impact, and a total of 25% of the available assessment score would be based on impact. These proposals have been met with a chorus of opposition from individual academics including in excess of 16,000 signatories to a petition organised by the University and College Union to remove research impact from the REF assessment. A summary of the main arguments presented against the REF is that:

* Measures of research impact are not easily related to measures of research quality
* There may be a time delay greater than the REF assessment period before the real impact of a research study is realised
* It will inhibit the pursuit of research that is recognised by peers to have intrinsic worth and interest
* 25% of the total REF score being based on research impact is too high because the measures of impact are untested, and it is unnecessary if the challenge to is encourage a behaviour of communicating basic research to a wider audience
* there is no unambiguous way of assessing the impact of a piece of research that stopped something happening in the wider world

HEFCE are now evaluating the formal responses to their proposals. However, even in the absence of the final terms of the Research Excellence Framework, the direction of Government policy is clear in that is requiring UK academics to think more about the impact of their research.

The debate amongst management scholars on the relevance of their research seems in microcosm to be the same debate that has erupted in response to the REF proposals. Taking one side or the other depends to a large extent on the researcher’s personal values and their understanding of the discipline. However, it is inevitable that societal factors and national agendas will influence researchers in the future and so we anticipate that increasingly researchers will understand that management research is not about whether the communities of academics or practitioners are served but how a single knowledge producing process can contribute without fear or favour to both of the groups equally. The response by management researchers to the REF proposals is likely to mean more active engagement with the business community, in the way it might ensure that industry and academia collaborate to shape research agendas of relevance for the business community. It may also mean that business schools ensure that their ideas are appropriately disseminated to the business community, rather than simply reporting and theorising on business practices as is often the case at the moment.

**Challenging the Methodological Orthodoxy**

Management research has too often been viewed, especially in the USA as being similar to research in the physical sciences, where universal laws govern the topics investigated. However, increasingly scholars suggest that management research is distinctive from other disciplines in its pluralism, transdisciplinarity and non-reductionism. For instance, Tranfield and Starkey (1998) argue that management research operates no single agreed ontological and epistemological paradigm and it is heterogeneous and fragmented (Whitley 1984, Tsoukas 1994) since it draws from associated disciplines in the social sciences. As a consequence its intellectual territory is not clearly defined. This means that dictating what counts for useful and valid knowledge but also how the knowledge should be collected, interpreted and valued becomes difficult or even impossible. One practical implications of this is that, at times where disciplines often compete for resources, management research is often seen as weaker than those disciplines that are able to project more convergent characteristics (Tranfield and Starkey 1998). It has long been thought that this is the reason that disciplines such as psychology perform so well in terms of funding from the research councils. This is not to say that management research has no ontological status; it’s just that awareness and further development of this aspect is required in order to distinguish the ontological status of management research and in parallel, as Tranfield and Starkey (1998) suggest, acceptance of the fact that its ontological status is likely to shift in time in accordance to local context.

Acceptance of such variations implies a greater openness to alternative perspectives for a given field of enquiry. The adoption of different worldviews, methodologies and methods takes advantage of the range of disciplinary influences on management research. With each worldview and methodology come different background emphases, different techniques to be learnt and different data to be collected. Yet in our experience this ‘critical mess’ of views, methods and data (Gartner and Birley, 2002) is the stuff of doing good research. Judging appropriate moves in the field requires a familiarity with different views, methods and data because from such familiarity come the skills of discernment and hence the ability to go on and do research in new situations.

Many of the methods that achieve this comprise largely non-statistical approaches to data collection and analysis, and are conventionally grouped under the term qualitative research (Thorpe and Holt, 2008). The definitional split between qualitative and quantitative research is familiar throughout the social science. Whilst often presented as being associated with irreconcilable schools of thought, there is a comparable logic to what each is attempting to achieve as well as inevitable operational overlaps. Researchers from both schools seek to identify significant patterns within data, assign categories and propose theories that they believe will create a particular truth, and resonate with all interested in the same questions. However, they do differ in the manner in which experiences are highlighted and how they are sifted. If we return to Whitehead’s (1929) ‘three-fold urge’, then quantitative work tends to limit its range to finding out what exists from a perspective of distance (isolating variables) and of averaging phenomena through numerical proxies, whereas qualitative work looks to find what exists by involvement and hence accepts the ensuing messiness and difference of using rich descriptions. Both approaches are prescriptive insofar as comparisons are made with other situations, yet where quantitative justifies their legitimacy using causal weightings of significance, qualitative work uses exemplary stories or cases. Qualitative work typically requires the researcher to involve themselves with those they are studying in some way; a dialogue is created, whether cursorily and at some physical remove (as in short telephone interviews or e-post cards, for example) or through sustained engagement (as in participatory research). The urge to explain what exists and so make recommendations for a better life through generalizations is tempered by the wish for the research to have some orientation to questions of how and why managerial life can be lived well, and lived better, differently. These are questions of difference for which the ‘smooth ground’ of quantitative research offers little traction. Of course the downside in accepting there are different causal stories to be told depending on where one is researching and with and ‘upon’ whom, is how to maintain sufficient distinction as a scientific researcher in order to see problems anew and so offer persistent critique rather than simply concur with settled patterns of experience. If quantitative research has a problem with its being too distant from immediate problem-solving activity; so qualitative research might have the problem of being too complicit with it.

**Plugging the Demographic Gap**

If the practice of management is undergoing to a period of significant change, and if, as we argue, the manner in which the research field is approached needs to shift in order to better align with practice, then it is reasonable to ask how the management research community is preparing for this challenge. Pettigrew (2001) has argued that the key challenge facing lies in issues of capacity and being open to new forms of delivery.  A similar conclusion was reached by Hodgkinson et al. (2001) who drew on experiences in the field of organisational psychology.   They argued that achieving research that scores highly both in terms of rigour and relevance will require more than simply adopting a "Mode 2" approach (Gibbons et al., 1994).  Realising such high quality research generally will mean that another gap needs bridging before the relevance gap - "a competency gap".

Through the 2009-14 Strategic Plan the Council of the Economic and Social Research Council (ESRC) has sought to address relevance through its revised postgraduate training framework. The health of certain disciplines is also to be invigorated through strategic investment for those particular disciplines (including management research) with a pressing need to strengthen or expand existing research capabilities. In 2005 the Council conducted (ESRC 2005) a major demographic review of the social sciences that indicated that the number of doctorates produced in the business and management area will not be sufficient to meet the demand for trained researchers. Compounded by the upcoming, anticipated number of retirements in the field and increase in demand for management-related studies over recent years, management research now faces an inevitable erosion in its capacity to sustain the number of quality academics it needs to deliver its programmes. As a consequence, those engaged in management research might need not only to maintain their research but also devote a greater amount of their time focussed on the development of future researchers, whether these be from an academic or from professional backgrounds.

The new revised Postgraduate Training Framework will be founded on the creation of a national network of accredited Doctoral Training Centres (DTCs) and smaller Doctoral Training Units (DTUs). The network will replace the current arrangements for delivering postgraduate training through ‘recognised’ training outlets (Departments, Schools and Faculties) and individually recognised training courses. The framework has also been designed to support the Council's key strategic ambition to strengthen the impact of its research and training beyond the social science community. The transfer of skilled people between sectors is one of the most potent mechanisms for maximising impact. The framework therefore places considerable emphasis on the deeper embedding of transferable (employability) skills development into training and also offers a greater and more flexible range of opportunities for forging partnerships with public, private and third sector organisations. Movement in the other direction from practice to the academy has led over the recent years to the significant growth of professional doctorates (DBAs). While such growth in professional doctorates and doctoral titles was generally welcomed, the nature of the programmes is still rather ambiguous (Thorpe et al., 2010). This is because some institutions have been offering DBA programmes have a more applied orientation whilst others offer programmes that are for all intents and purposes identical to a PhD, apart from the designation of the degree (AACSB 2003). This lack of a clear understanding of what exactly a professional doctorate is and in what ways it is similar or different from a traditional PhD has implications for management research itself. If professional doctorates are considered as the only arena where practice is questioned and where the relationship of theory to practice is demonstrated, then what does ‘academic’ management research represent? These issues relate closely to and further fuel the debate about rigour versus relevance in management research.

Professional doctorates, we believe, do represent a good opportunity to demonstrate the relationship of theory to practice as well as for practice to be questioned. A key issue for any postgraduate doctorate programme will be its pedagogical philosophy and how the approach taken considers the link between theory and practice and vice-versa. This in turn has implications for how participants are equipped to approach their research, particularly during the induction process and this will include consideration of such things as; the nature of the thesis; the type of research design that is appropriate for the study, for example, action research, insider research and collaborative methods of enquiry and the use made of longitudinal designs and the case method. Issues relating to how students are encouraged to produce outputs for both academic and practitioner audiences (both conference presentations as well as journal articles) were also thought to be important. Bridging the gap between theory and practice also means that different arrangements for the supervision of students may need to be considered. Given that the majority of students currently studying for professional doctorates are studying part-time, there is a need for those delivering them to indicate how the supervisory support systems were managed in order to meet the particular requirements of this group of students, and how the very different constituency of part-time researchers are catered for within institutions. It may be necessary, for example, to consider other practical issues such as; the choice of the supervisory team; the frequency of supervisory meetings; arrangements for meetings with students that are for the most part studying at a distance; the support infrastructure during the research phase of their programme; how quality is monitored, and student feedback considered and what provisions are made for appeals and whether there were different requirements for examination and assessment. We also consider that it may well be necessary to involve in the assessment process the voice of a professional or a professional body within the viva. In addition, the student’s learning and impact on the candidate’s practice as a professional will also need to be considered. For example, in some institutions students are required to provide a critically reflective statement in addition to the thesis. The majority of programmes also contain a sufficient taught component which for many does not feature in the final assessment, and there is a debate as to whether or not these elements of coursework should form part of the assessment within the viva.

**Rethinking Management Research**

In this chapter we have argued that, as all academic researchers from whatever disciplinary field need to be more responsive to societal and economic needs, then management researchers now need to do their part. Calls for greater responsiveness exist at the level of rhetoric expressed as *grand challenges*, but also at a more pragmatic level that is set to influence the very funding of academic research. Such mandates provide an added impetus to the long-standing debate on the relevance of management research for practising managers. And as those managers are themselves seeking new ways to respond to the same grand challenges there is an opportunity to think afresh about the nature of our research projects.

Ken Starkey who has contributed so much to the relevance debate continues to lead the way; positioning management research as a design science he advocates “re-imaging relevance as a necessary condition of rigour” (Starkey, 2009). Viewed in relation to our contribution to *grand challenges* such relevance is not to be narrowly conceived as solving today’s business problem like some kind of management consultant; only with more methodological rigour. Rather, in line with Whitehead’s three-fold urge of human inquiry (1929), our concern, we argue, should be to move beyond what practitioners know well-enough now and discover new logics for management action.

Re-orienting our purposes toward greater relevance will we believe, open the door for fresh methodological perspectives. Adopting a new logic of discovery (Starkey et al., 2009) rather than the more rigour-orientated logic of verification will be facilitated by methods that promote active engagement between practitioners and researchers. In advocating qualitative research we share a desire for scientific rigour in making accurate representations explicit in quantitative research, but accept implicitly that research only remains meaningful to the extent that it is used, and the likelihood of its being used in managerial life is increased if it concentrates on the problems being experienced by managers and their organizations. Whilst greater involvement in such problems may be consistent with a shift toward qualitative research, the change needed may be as much to do with changes in the body of researchers as it is in their methods. Active steps are being taken to address the demographic gap that has been identified within the management research community that include holistic changes to how new researchers are recruited and trained.

Finally, we should perhaps not loose sight of the fact that the actual research area of interest (management) is an eclectic practice itself. Managers often work across technical, cultural, and functional boundaries, something that ultimately leads them not only to draw on knowledge developed by other disciplines but also understand what their findings mean for their practice (Thorpe and Beasley 2008). Also, the nature of management itself is an ambiguous activity where there are no ultimate answers or design solutions (Finchman and Clark 2009). If academics are to continue exploring management in its real context, research will promote further eclecticism. As a result, scholars should pay attention to the shifting of the management practice in order to produce useful and valid knowledge.**Bibiography**

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