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Management research and management practice: is the relevance gap closing?

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Abstract: Over the past decade, there has been much academic debate in management research circles bemoaning the fact that the management practitioners are not using the management research being produced in universities. These debates in the UK have focused on the research rigour and relevant question and on the need for new forms of social organisation for the production of management knowledge encapsulating concerns about the structure of business schools, the nature of management education and the conduct of research. Yet, despite this debate and the clamour for increased relevance, the amount of funding that the UK business schools are attracting directly from business is continuing to decline. The purpose of this article is to stimulate further debate on these issues.

Keywords: business schools; management practice; management research; relevance; rigour.

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Biographical note: Les Worrall is currently a Professor of Strategic Analysis at the Coventry University in the UK. He holds a BA (Hons) in Geography from the University of Hull (1974), a Master of Civic Design from the University of Liverpool (1977) and a PhD in information systems development and public policy from the University of Liverpool (1987). He has published extensively on organisational problems, organisational stress, the work-life balance and the nature of management knowledge. He has also been a Consultant to a large number of organisations including the Confederation of British Industry, the Chartered Management Institute and the various UK local government organisations. At the time of writing, he is Chair of the British Academy of Management's Directors of Research Network.

As an Academic Researcher, I have always attempted to keep my feet (well, at least one foot) in the world of management practice and have had careers in both the world of professional practice and the world of academia. I was educated in the world's first graduate school of planning (the Department of Civic Design at the University of Liverpool): the department was founded in 1909 with substantial funding from the Lever family to educate practitioners in the then newly emerging profession of planning. My

education to master's level incorporated both the theoretical underpinnings of my field of study and aspects of planning practice: I feel that I was educated to think and to do. My education to doctoral level also spanned the academic/practitioner divide as my doctoral thesis focused on the design of strategic information systems to support public policy development and the management of urban change.

1 Introduction

A personal note: as an Academic Researcher, I have always attempted to keep my feet (well, at least one foot) in the world of management practice and have had careers in both the world of professional practice and the world of academia. I was educated in the world's first graduate school of planning (the Department of Civic Design at the University of Liverpool): the department was founded in 1909 with substantial funding from the Lever family to educate practitioners in the then newly emerging profession of planning. My education to master's level incorporated both the theoretical underpinnings of my field of study and aspects of planning practice: I feel that I was educated to think and to do. My education to doctoral level also spanned the academic/practitioner divide as my doctoral thesis focused on the design of strategic information systems to support public policy development and the management of urban change.

In the recent past, there has been a growing appreciation in the business strategy literature, typified by Nonaka and Takeuchi (1995), that knowledge is an essential, non-imitable element of a firm's competitive strategy. However, other writers from the field of knowledge management have argued that there has been insufficient analysis of the nature of professional or practical knowledge which is often experience-based, tacit, difficult to articulate explicitly and even, 'beyond the reach of language' (Johannessen, 2006, p.229). Government policy in the UK has increasingly focused on 'knowledge transfer' from universities to businesses as a policy goal with, for example, the former Teaching Company Schemes being relabelled Knowledge Transfer Partnerships and universities in the UK being increasingly exhorted to develop more effective partnership relationships with businesses to capitalise on, exploit and transfer the knowledge being created in universities in order to enhance the competitiveness and productivity of the UK business. While these initiatives are focused much wider than the transfer of management knowledge, and often focus on transferring and then exploiting technology to return revenue back into an under-funded higher education system, one might expect that the adoption of the outputs of the management research being carried out in business schools should lead to the improvement of the competitive position of the UK and improve the productivity of UK businesses, assuming, of course, that university-produced research outputs are relevant, applicable and capable of being implemented.

Despite this hope, experience would lead us to feel that this transfer of management knowledge is not taking place. Indeed, there is ample evidence to suggest that the management research has had, and continues to have, little effect or impact on management practice (see Tranfield and Starkey, 1998; Starkey and Madan, 2001; Kelemen and Bansal, 2002; Ford et al., 2005; Brannick and Coghlan, 2006; AACSB, 2007). These concerns are far from new and there are references going back to the early 1980s, and beyond, expressing concerns that management research had had 'little effect on the life of organisations' and that more effort needed to be applied to achieving 'greater utilisation of organisational research' (Beyer, 1982, p.588 cited in Vermeulen,

2005, p.978). Where management research had been used and had been influential, Ghoshal (2005) went as far as to suggest that bad management theories were actually destroying good management practices raising a concern here not about relevance but about the applicability and fitness for purpose of much academically produced management theory. Management research, as currently enacted, seems to have a considerable 'utilisation problem' and a low level of 'face validity' among practitioners. Pettigrew (2001, p.s61), one of the leading authors on the rigour-relevance issue, has commented that the management research community has a long way to go to realise its potential in making a difference in the world of management practice. As Rynes, Bartunek and Daft (2001, p.340) argue, executives rarely turn to academics or their research findings to solve problems and academics rarely turn to practitioners when they are trying to frame research questions or 'for insight when interpreting their results'. Despite these concerns, Rynes, Bartunek and Daft (2001, p.342) also identified a paucity of empirical research on why the gap has been created and is being sustained.

The purpose of this article is to assemble some thoughts about management research and, in particular, to explore the 'relevance gap' by examining the impact of academic management research on management practice; to discuss the nature of management research; to discuss how changes in the UK universities may be affecting management research; and, to review the recent debate about the modes by which management knowledge is produced. The article concludes with a short discussion about some of the structural issues that seem to be perpetuating the academe–practice gap.

2 The impact of management research on practice

It is incontrovertible that the management research has had much less impact on management and organisations than the array of fads, half truths, self-laudatory accounts of business leaders and plausible sophistry that can be found in the bookshops of airports all around the world. While fads have the ability to pervade the world of management practitioners, more traditional management research does not. Some might argue that some of the fads that have been adopted have an academic parentage (for example, the business process reengineering movement, followed by reengineering the corporation and, finally, reengineering management (Champy, 1995) have some academic provenance) but this commodified research has, the author suggests, been transformed by the process of commodification itself into a form of plausible sophistry that is too easily ingested by a cadre of managers who lack the critical skills effectively to evaluate either the validity of the claims the research makes or the potential organisational damage that populist material of this type can bring about. Unfortunately, on the reengineering theme, the popular and business press has published several examples of firms spending millions on consultancy fees only to reengineer themselves completely out of existence. Too many managers seem to think that 'if it worked there, then it will work here' not being able to see the context-dependence of much of this putative research. The point remains: fads have a much greater mimetic capacity than more robust forms of academic research indicating that the managers may not have had the education and training in the management research that would enable them to distinguish between sophistry and substantive theory. This view is reinforced by Van Aken (2004, p.220) who concluded that 'management fads, scorned by academics' seem to have had much more impact on management actions than the output of mainstream academic researchers. Others, using

Pettigrew's (2001) rigour-relevance concerns have argued that academics have been more concerned with clearing the rigour hurdle than they have the relevance hurdle (van Aken, 2004, p.223; 2005, p.19) mainly because academic career paths are structured to place more value on rigour than on relevance (Caswill and Wensley, 2007, p.312). Some might even argue that some management researchers have no intention of even trying to clear the relevance hurdle as they regard the world of practice with some disdain.

The author's own concerns about these issues have become more pronounced recently following his involvement with the UK's Association of Business School's (ABS) Research Committee and the British Academy of Management (BAM). Information presented to the ABS research committee in the early 2007 (Wensley, 2007), using Higher Education Statistics Agency (HESA) data, has shown that the amount of funding coming into the UK management research from the UK private sector organisations has declined since 1995/1996 and has still continued to decline in recent years despite the relevance gap having received a significant attention by the academic management researchers since the late 1990s. If one examines the decline in '1995/1996 pounds', then the decline is even more severe (the data are shown in Appendix A). Arguably, if management researchers were closing the 'relevance gap' with managers in the private sector then surely the private sector would see academic research as something from which they could generate competitive advantage and the economics of self-interest would take over. Clearly, there is either a very limited market for management research as it is currently visualised by the private sector or the market is failing for some, as yet, unclear and unspecified reason. The two other sources of funding (the public sector and the Office of Science and Technology – the OST) shown in Appendix A have shown radically different trends from that of the private sector particularly since 2002–2003. For the OST, the break of trend in 2002–2003 is related to the significant funding from the Economic and Social Research Council (ESRC) for management research in the form of the Advanced Institute of Management (AIM). The increase from the UK public sector is more difficult to explain: though some suggestions are made later on in this article. The fact that there is a multi-million pound annual market for consultancy in the UK somewhat destroys the myth that there is no market for management knowledge (or what passes for management knowledge). In 2004/2005, the amount of funding from the business sector to the UK university-based management researchers amounted to < 0.1% of consultancy industry turnover (source: Wensley, 2007).

Why should this be so? At the ABS research conference in March 2007, Wensley (2007), Director of the AIM, raised several concerns about 'management research'. He cited evidence drawn from within the ESRC that showed that management research had the lowest success rate of any discipline in bidding for 'response mode' research funding and a higher proportion of research bids that were rated as 'problematic'. As an occasional reviewer for the ESRC, the author can concur with this assertion having found management research proposals often to be under-theorised with indifferently developed methodologies. Wensley also cited evidence from the 2001 Research Assessment Exercise (RAE) to reveal that while 3% of economics submissions were in the two lowest research rated categories, this increased to 20% in management and business. He cited evidence going back to the mid-1960s indicating that the funding councils of the time saw management research as having 'particular difficulties' though it was not entirely clear what these particular difficulties were.

Perhaps, the reason why the UK business sector does not fund more management research is because management research is perceived by them not to be very good? Is it because academic management researchers and management practitioners have different and perhaps even incommensurable frames of reference (see Rynes, Bartunek and Daft, 2001, p.341) with respect to what constitutes management knowledge in general and operationalisable management knowledge in particular? It may be that academics value the academic reputation system and value their desire to make progress within it as more important than producing research that resonates more strongly with the practitioner reputation and value system. It may be that the market is not failing: perhaps academic management researchers are just not responding to an unambiguous market signal from one of its stakeholders (especially management practitioners in the private sector) perhaps because they put other stakeholders' interests or their own self-interest ahead of management practitioners. It may be that the private sector management practitioners in particular do not see the value of more academic, theory-driven research because of the way that they have been socialised by the management education processes they have been through. Perhaps, MBAs that emphasise the 'how to' over the 'why so', 'war stories over theory' and 'doing over thinking' are to blame? Perhaps, MBAs and deliverers on MBA programmes that are detached from research are perpetuating a vicious circle?

Interestingly, while recent evidence suggests (Worrall and Cooper, 1999) that the proportion of managers with an MBA was about the same in both the public and private sectors (11%), managers in the public sector were twice as likely to have masters' degrees or doctorates from other disciplines (24%) than were managers in the private sector (12%). Does this mean that the managers in the public sector (who generally have higher levels of certificated education than managers in the private sector) might be more receptive to 'academic research' than managers in the private sector? On the other hand, Weick (2001) argues that the relevance gap is 'as much a product of practitioners wedded to gurus and fads as it is of academics wedded to abstractions and fundamentals' (p.71). But why are they wedded to populist gurus? Do gurus just fill the role of comfort blanket to many managers? Other critical management scholars, in discussing the relationship between the producers and the consumers of management knowledge, have raised questions about the portability of academically-produced management knowledge into practice implying that the practitioners are often only receptive to knowledge that reaffirms or reinforces their existing beliefs (Carter and Jackson, 2004) and are chary about having their pre-existing cognitive moulds cracked. Whatever the cause, or causes, of the relevance gap or the portability/receptivity gap there seems to be clear evidence that the gap between academic management researchers and management practitioners is widening and not narrowing (Hodgkinson, Herriot and Anderson, 2001) and that this may be particularly problematic in the UK when the quality of management is often characterised as not particularly inspired given the productivity differentials that exist between the UK businesses and their mainland European counterparts.

There is ample evidence to suggest that these gaps are wide and are widening further but little good research to explain why these gaps exist and what can be done to close them: it is clear that we need more relevant, rigorous and operationalisable research. Essentially, what is needed is a body of theory that will enable us to explain why these gaps exist and have persisted and a body of theory that will help us to identify how to close these gaps. As the author shall argue later, pragmatic management research needs to integrate both the 'why' (theories to develop understanding – episteme or 'disinterested

understanding’) and the ‘how’ (theories of action to guide effective implementation – *techne* or craftsmanship). In addition, management research needs to be concerned with *phronesis* which can be defined as

“practical knowledge of how to act prudently and correctly in a given immediate and ambiguous social or political situation.” (Van der Ven, 2007, p.3)

Arguably, in many cases academics have focused on the *why* much more than they have focused on the *how*. While academics tend to value *why* over *how*, practitioners tend to value *how* over *why*: in many cases, practitioners do not care *why* but they do need to know *how*! These issues are encapsulated in a quote from Mintzberg (2004, p.3) who argued that

“everything that every effective manager does is sandwiched between action of the ground and reflection in the abstract. Action without reflection is thoughtless; reflection without action is passive. All managers have to find ways to combine those two mindsets to function at the point at which reflective thinking meets practical doing.”

3 Is there something peculiar about management research?

During his involvement with organisations like the BAM and the ABS since the early 1990s the author has heard many presentations about the relevance gap, about developing more effective relationships with management practitioners and about academia contributing more towards enhancing the competitiveness of the UK economy. In this vein, it is interesting to read Donald Hambrick’s 1993 presidential address to the US Academy: he likened management research to George Bailey, the central character in Frank Capra’s film ‘It’s a Wonderful Life’. In the film, George is about to commit suicide when an angel (Clarence) takes him back to see the void that there would have been if George had never existed. As Hambrick disparagingly concluded, after applying the same test to management research, ‘we will find out that things might have worked out very well without us’ (Hambrick, 1994, p.11). So why has management research failed to make an impact on the world of practice despite the burgeoning volume of research outcomes it produces as evidenced by the sheer size of the Business and Management Unit Of Assessment (UOA) in the 2008 RAE; the huge number of students who now take business and management at undergraduate level and the legions of MBAs who have passed through our collective portals? Has the MBA done more harm than good to the practice of management?

The author’s questions are: is there any such thing as ‘management research’ over and above research in the disciplinary domains that underpin management and is there a coherent body of management research literature? A review of the 2001 RAE (Geary, Marriott and Rowlinson, 2004) revealed that around 1,580 different journals were cited within the business and management UOA ranging from the mainstream to the downright obscure. Bessant et al. (2003, p.62) concluded that in the 2001 RAE, the business and management UOA covered a wider range of disciplines than any other UOA. It is very clear that the management research is an umbrella term that has within it disciplines such as economics, sociology, psychology, and, ‘management sciences’; which embraces subject domains such as marketing, strategy, accounting and finance, human resource management, and, information management; and has embraced a wide range of

methodological stances ranging from econometrics, through action research to critical ethnography. It is clear that the management is a highly differentiated field which Tranfield and Starkey (1998) label as 'heterogeneous and fragmented' and a field that 'operates no single agreed ontological or epistemological paradigm' (p.345). Pettigrew (2001, p.s63) does not see management as a discipline but as 'a confluence of different fields of enquiry'. Perhaps, this is one of our more fundamental problems: as Hatchuel (2001, p.s34) has argued, within a renewed model of management research, there is a need for a clearer 'scientific identity' for management research 'differentiating it from social and economic studies'. This also resonates with van Aken (2004, 2005) view of management as a 'design science'.

These problems are also confounded by other distinctions: first, it is important to distinguish between business and management research and research that are conducted within business schools; and, secondly, it is important to distinguish between research about management and research for management. There are many questions about the social organisation and location of research in universities: over the recent past, it is undeniable that the universities have become more 'managerial' and the issues of structure and about the changing location of power have become more predominant (Bryson, 2004) in an increasingly hard financial climate. Increasingly, economics departments, industrial sociology departments, organisational psychology groups within larger psychology departments and many other disciplinary-based and relatively small-scale academic units have (often forcibly and with resistance) been relocated within business schools – 'the cash cow' of the UK university system (Starkey, Hatchuel and Tempest, 2004, p.1521).

While the creation of an environment where multi- and trans-disciplinary research can take place is not necessarily a bad thing, these coalitions and alliances seem to develop more effectively 'bottom up' and organically rather than from the units grudgingly being co-located spatially but not spiritually as an outcome of a top-down management review usually designed to reduce costs or to make the organisational chart look tidy. In addition, there has been a clear shift in the locus of power in many the UK universities (especially the former polytechnics) away from more collegial forms of governance structure where power is more diffuse and distributed to more managerial forms which are more hierarchical and where power is more concentrated within a cadre of elite academic managers most of whom have jettisoned their publishing/research careers in their quest to acquire positional status within academic institutions. These shifts have done much to reshape the milieu in which management (and other) research takes place particularly when they have been coupled with initiatives designed to increase what is known in the UK as 3rd Stream Funding. Third Stream Funding is that which is derived directly from business especially from the exploitation of knowledge generated from within the University and in partnership with external businesses: it is often seen as a way of topping up university finances given the continuing erosion of teaching and research funding.

Universities have been put under increased pressure to generate this form of funding as teaching and research funding has come under considerable pressure in the UK as government funding for teaching in particular has failed to keep pace with inflation, student numbers have increased and the number of teaching staff has failed to keep pace with the increased number of students. These trends have led to an intensification of academic work with, in many cases, increased teaching loads reducing the ability of academics to undertake research either at all or at the highest level of quality. Bessant

et al. (2003) in their review of the 2001 RAE noted that there are huge tensions in universities between teaching, research and practical experience and concluded that 'nowhere in the higher education sector is this tension more evident than in business and management' (p.64). It is somewhat paradoxical that as universities have been put under pressure to generate more funding directly from business, the amount of funding that business schools are attracting from businesses, as we suggest in Appendix A, has actually declined both absolutely and relative to other sources of funding. Clearly, institutional factors such as these have a major effect on some academic's capacity to do research.

4 Research about management or research for management?

A second area of debate is the difference between research about management and research for management. While some academics might value the contribution their research could or does make to management practice, there is also a clear core of management researchers, particularly those of a more critical persuasion, who are more concerned to deconstruct and critique management than to support it. A strong theme in much critical management research is to focus on the victims of management and the desirability of their emancipation. It is hard to see how many practitioners would engage with the research of the overwhelming majority of critical management researchers some of which is linguistically and conceptually impenetrable and some of which, to quote Parker (2002), is 'against management'. Whether some managers would actually benefit from reflexively deconstructing what they are doing, critiquing what their value systems actually are and are leading them towards is, however, another point entirely. Perhaps, many managers just take too much for granted, do not reflect upon their belief systems, do not think through the ideological bases of management and do not reflect on the consequences of their actions outside their effects on 'the bottom line'.

A recent article by van Aken (2004, p.219) begins with the premise that there are 'serious doubts about the actual relevance of present-day management research as developed by the academic community'. He argues that management research needs to adopt the paradigm of the 'design sciences' which are built on field-tested, technological rules. However, he is concerned that much theoretical work is 'too reductionistic and hence too broad or too trivial to be of much practical relevance' (p.221) – hence, the utilisation problem. Van Aken then contrasts management with other applied fields such as medicine and engineering, in which the integration of theoretical research within practice is, he asserts, much better developed. He argues that in medicine there is a distinction between the theoretically-grounded knowledge production process designed to improve our understanding and the development of theories focused on problem-solving which engender the transfer, adoption and implementation of that knowledge into medical practice. While there is a distinction between laboratory-based science and clinical practice this should not be taken to imply that these are distinct and non-interacting. There are distinct advantages to be gained between the joint working of both groups to engender the co-production of knowledge among these groups as clinicians feed back evidence about the effect of interventions back to the laboratory based groups which, in turn, might enrich existing theory, identify new paths for investigation, identify new problems or 'reframe' old problems. This pattern of organisation does not exist in

the management research domain in the UK though there are examples of this form of social organisation of management research in Sweden and France (Hatcheul, 2001; Starkey and Madan, 2001).

Increasingly, in the field of medicine, cross-, multi- or trans-disciplinary groups are being created – perhaps focused on specialisms such as oncology – which erode professional and disciplinary boundaries and create a more cooperative mode of knowledge production that fuses together those who ‘do the laboratory science’ with both clinical practitioners and social policy professionals facilitating the flow of feedback, learning and reflexivity. This is the embodiment of Mode 2 knowledge production as outlined in Gibbons et al. (1994) and discussed in Tranfield and Starkey, (1998) and by Eden and Huxham (1996) in their work on action research where it is seen as imperative that research and situated learning are embedded with action or change processes. Mode 1 research is defined as the ‘traditional mode’ of knowledge production in which knowledge is produced from a single disciplinary perspective and with little emphasis on its adoption and deployment. Huff (2000, p.288) characterised Mode 1 as ‘discipline based, university centred, and dominated by highly trained individuals’; it is

“primarily cognitive, carefully validated by peer review, and applied later, by others, if it is applied at all.”

Some of the distinctions between Mode 1 and 2 research are shown in Table 1 which has been adapted from Kelemen and Bonsal (2002) and augmented with material from Gibbons et al. (1994), Huff (2000) and MacLean, MacIntosh and Grant (2002).

Van Aken (2004, p.235) argues that

“the relevance problem of academic management research can be mitigated if description-driven research, resulting in what may be called Organization Theory, is complemented by prescription-driven research, resulting in what may be called Management Theory.”

This reflects Tranfield and Starkey (1998, p.346) assertion that management research is not only about ‘knowing what’, but also about ‘knowing how’. Interestingly, Beer (2001, p.59) argues that a key factor that prevents the development of usable knowledge is that a ‘split’ exists between those who develop ‘substantive management theory and those who practice and study how knowledge can be implemented and organisations changed’. Van Aken’s distinction between substantive theories and theories of action is clearly important and a challenge that few management researchers seem to have effectively addressed. Exceptions to this criticism are provided by Hatcheul (2001) who defines a model of ‘intervention-research’ based on a negotiated partnership arrangement between researchers and the host organisation. Here, the problems are historically and contextually embedded in an organisational setting but the prime aim of the researcher is to locate these problems within a more academically grounded context. In the French and Swedish organisations he describes, a research capability is developed in these organisations as a direct result of their structured engagement with academic researchers. He argues (p.38) that significant cultural change occurs in host organisations which means that ‘managerial choices, endeavours and evaluations can be progressively designed with increased scientific awareness’ and that ‘there is no better method to reduce misleading mimetic behaviour, blind compliance to gurus or fashion in management practice’. Modes of working of this type do not seem to exist in the UK: perhaps they should.

Table 1 Mode 1 and 2 research compared

<i>Aspect</i>	<i>Mode 1</i>	<i>Mode 2</i>
Research boundaries	Single discipline, impermeable, paradigmatic conformity	Trans-disciplinary, permeable
Stakeholders and audience	Academics often within a prescribed discipline and often a much smaller sub-field of 'experts'	Academics and practitioners
Aim	Production of new knowledge, theory building, adding to the base of disciplinary knowledge, replicability, validity	To gain insights that are useful and usable to practitioners and society at large
	Production of enduring knowledge	Production of solutions Production of transient knowledge
Outcome	Basic and applied research	Applied and applicable research
Organisation	Often individualist	Team based
	Research agenda set autonomously	Externally defined research agenda
	Hierarchic Substantial commitment to extant bodies of knowledge	Heterarchic, networked
Dissemination	Peer reviewed journals controlled by other academics, well defined and institutionalised channels, single and limited public	Transfer into practice, practitioner-oriented journals, dissemination often through professional bodies, multiple publics
Usage	Production precedes consumption	Simultaneous production and consumption: knowledge production and diffusion are interlinked and may be multi-modal
	May never be used to support practice	
	Potential use does not influence research design	
Orientation	Elitist, exclusive	Pluralist, participatory
Evaluation criteria	Excellence as determined by peers, disciplinary norms and quality audits (e.g. the RAE)	Applicability, perceived usefulness by research users and contribution to practice by practitioners
Validation	Validation through peer review and publication	Validation in use
Methodology	Defined by the academic discipline	Plural and defined by the research context, often emergent

A major corollary of the move to Mode 2 knowledge production is that its social organisation is completely different from the form of social organisation that underpins Mode 1 knowledge production. Many of the aspects of the social organisation of Mode 2 knowledge production run counter to the current social organisation of academic management research. Despite the 2001 RAE giving a clear indication that trans-disciplinary research would be valued highly, almost all the research submitted was constrained to the traditional disciplinary silos found in most business schools. It seems unlikely that the trans-disciplinary challenge will be met by business schools in the 2008 RAE given that the assessment panel is organised very much on the traditional divisions that still reflect the functionalist structure and mode of operations of most business schools (e.g. marketing, strategy, and human resource management). The key point to

emerge is that the move to more cooperative processes of management knowledge production is being held back by institutional inertia in both the UK universities and the UK business organisations and by cognitive inertia among the UK academics and practitioners. As Ghoshal (2005, p.88) argues,

“the only alternative to any form of ideological absolutism lies in intellectual pluralism, which is likely to lead to both better research and to broadened usefulness.”

5 The social organisation of management research and management education

Not only does the move to Mode 2 knowledge production have implications for the social organisation of research, but also has implication for the design of business schools and the design of management education programmes. If the medical analogy is taken further, this would imply that business schools should adopt the organisational model of medical schools and become what some have called ‘Professional Schools’. There has been some debate of late about the future of business schools: Pfeffer and Fong (2002) have forecast the end of the business school and Starkey, Hatchuel and Tempest (2004) have sought to rethink the business school. Murray (2006, p.10) has warned that business schools ‘educate new generations of managers who ignore the academy because it cannot offer coherent evidence based enlightenment’ and has argued that managers want ‘research to be enmeshed in action’ but that this is resisted by academics. The essence of Pfeffer and Fong (2004) argument is that business schools should be assessed in two fronts: their contribution to the production of management research and their contribution to management education. The authors argue that business schools fail on the research front because they neither deliver rigour nor relevance and, according to Starkey, Hatchuel and Tempest (2004, p.1522), they fail on the teaching front because they have conspicuously failed to ‘generate critical thought and enquiry about business and management’. More recently, other writers have been even more acerbic about business school: Emeritus Professor Ackoff (2007) has stated that he thinks that ‘Business schools are way back, they are behind even corporate practice. They are a major obstruction’. Ackoff (2007) has, rather cynically, identified three contributions of a business school education: he felt

“it gives students a vocabulary that enables them to speak with authority on things they do not understand; it gives them a set of operating principles that enable them to withstand any amount of disconfirming evidence; and, it gives them a ticket to a job where they can learn something about management.”

In the face of this barrage, it is not surprising that Starkey, Hatchuel and Tempest (2004, p.1523) have argued that there is a need to reinvent business schools

“geared to developing skills in reflective, collaborative and analytical thinking as well as action mindsets that enable managers to negotiate the complex tensions that exist between the conceptual and the concrete.”

This will require new forms of business school organisation that focus on the co-production of knowledge as business schools form partnerships with businesses to focus on the issues that businesses themselves consider relevant not on the issues that academics think practitioners should focus on. Other writers have suggested that business schools should seek to create a breed of ‘professional’ managers who practice forms of

‘evidence-based management’ that are common in the medical profession. Unlike Murray (2006), Rousseau and McCarthy (2007, p.84) argue that

“contemporary managers and management educators make limited use of the vast behavioural science evidence base relevant to effective organizational practice.”

While one might not question their ‘relevant to’ thesis (Murray, 2006 does, however, question the ‘relevant to’ thesis), one might be legitimately concerned about whether behavioural science research is actually accessible to, and then implementable by, practitioners because of the lack of what van Aken terms complementary ‘management theory’.

The new form of business school advocated by Starkey and Madan (2001) and Starkey, Hatchuel and Tempest (2004) does not sit well with the form of many schools both in the teaching and research styles of academics nor in the learning expectations of students who, from personal experience, seem to have acquired an increasingly instrumental and often minimalist approach to their own education as the educational system has itself been commodified. Certainly, the configuration of most business schools does not look remotely like the ‘new agora’ that Nowotny, Scott and Gibbons (2001) and Starkey, Hatchuel and Tempest (2004) see as a form of organisation where multiple stakeholders and participants in knowledge production ‘co-mingle’. While this is easily said, it is not easily delivered due both to the ‘cognitive inertia’ of many participants and because it would require the radical transformation of the academic recognition system. In this new system of values, high quality practitioner-orientated research would be valued as highly as that published in top rated academic journals and academics would be able to progress their careers based on their contribution to practice coupled with their contribution to the development of their specialisms though, under Mode 2, they may have to take a more eclectic and inclusive approach from those working in single discipline arenas (Starkey and Madan, 2001).

What is surprising is that the management literature on these issues – especially the creation of the new agora – never mentions the impact of new and emerging technologies on the increasing ability of currently disparate stakeholders to cooperate and communicate as a virtual community of practice to develop new knowledge and share different perspectives. Perhaps, this can be explained by inertia and also because academics might feel that they are losing control over the educational experience and the ‘educational dynamic’ that they have controlled in the past. Perhaps, some might see the end of ‘academic magistracy’ as a problem as new players become more influential in the management knowledge production system. Under the existing Mode 1 regime, it is academics themselves, through the peer review regime, who decide what constitutes knowledge, what is worthy of being published and what can enter the academically-approved public domain (even if that public may be very small and self-selecting).

6 The rigour-relevance issue pursued

The discussion so far has focused on issues such as rigour and relevance and the adoption of different research styles (or modes of knowledge production) which have different forms of social organisation that collide with some highly institutionalised practices and values in both academia and practice. Some of the emerging issues are addressed in

Table 2 where following Hodgkinson, Herriot and Anderson (2001), a two-by-two table (ubiquitous in management publications) is developed in which research is categorised based on the level of theoretical and methodological rigour (high/low) and the level of practitioner relevance (high/low). Again, following Hodgkinson, Herriot and Anderson (2001), research is categorised into four types labelled ‘puerile’ (low relevance/low rigour), ‘populist’ (high relevance/low rigour), ‘pedantic’ (low relevance/high rigour) and ‘pragmatic’ (high relevance/high rigour). While populist research addresses the ‘issues of the moment’ as defined by practitioners there is a clear concern that some of the ‘research’ conducted in this field may have been stimulated primarily by the availability funding and not by scientific enquiry. As a consequence, it may fail to pass muster when compared against the methodological and theoretical standards demanded in the ‘pedantic’ quadrant. The pedantic quadrant is characterised by many of the criteria associated with Mode 1 (see Table 1 earlier) which, some might argue, are being perpetuated by a highly traditional and well institutionalised academic recognition system and research quality assessment exercises such as the UK RAE which, like many other performance/quality management systems of its type may have distorted the very thing it set out to measure.

Table 2: A typology of research

<i>Practitioner relevance</i>	<i>Theoretical and methodological rigour</i>	
	<i>Low</i>	<i>High</i>
High	‘Popularist’ Consultancy? Epistemic drift Publishable in practitioner journals Fads, sophistry? Inappropriate solutions to pressing problems Doxa	‘Pragmatic’ Problem-solving focus Theoretically robust Academic and practitioner collaboration Mode 2 characteristics Unlikely to be published in ‘top’ journals? Episteme, techne, phronesis (practical wisdom)
Low	‘Puerile’ Addressing irrelevant problems with inadequate theories and methods	‘Pedantic’ Accessible to the few Targeted on the few Mode 1 characteristics Main aim to be published in the ‘top’ journals RAE oriented Episteme

Source: Hodgkinson, Herriot and Anderson (2001).

Arguably, in the pursuit of relevance, more management research (but not all) should be trying to reposition itself within the pragmatic quadrant where the focus is clearly on developing theoretically and methodologically robust solutions to urgent managerial problems ideally by academic researchers working closely with practitioners to achieve what Gibbons et al. (1994) would call knowledge production ‘in the context application’. However, Starkey and Madan (2001) suggest that significant institutional, structural and cultural changes are needed to bring this about: they argue for ‘business education

reform' (p.s16); the move towards interdisciplinarity (p.s18); the restructuring of academic institutions to improve knowledge exchange and dissemination (p.s20); the creation of new cross-disciplinary, impact-focused journals; the development of new measures of 'academic impact'; and, the creation of academic/practitioner forums to facilitate cooperation, improved mutual understanding, shared languages and, ultimately, the co-production of knowledge.

While many have seen the advantages of these new formations, others have counselled against too radical a shift having concerns that quick response research to end-user funded, short term issues of the moment might lead to 'epistemic drift' (Tranfield and Starkey, 1998, p.350) and a damaging move towards 'populist science' (Hodgkinson, Herriot and Anderson, 2001, p.s43) as these funding opportunities distort the behaviour of management researchers and also the type (rigour?) of research that is conducted: this is a real risk but one that can be countered by developing a portfolio approach to management research which values and rewards both basic and applied management research and contains the structures and knowledge production processes that can deliver pragmatic research. Ghoshal (2005) and Van Der Ven (2007, p.7) both cite Boyer (1990) who defined four kinds of scholarship: the scholarship of discovery (research); the scholarship of integration (synthesis); the scholarship of practice (application); and, the scholarship of teaching (pedagogy). Perhaps, business schools have lost sight of the need to take a more balanced and integrated view of these four scholarships and our key challenge as academics is to learn how to (re)unite research, synthesis, application and pedagogy into a more coherent whole. By (re)uniting these four scholarships, Ghoshal (2005, p.87) argues that we would

"build management theories that are broader and richer than the reductionist and partial theories we have been developing over the last 30 years."

which Ghoshal sees as 'bad management theories' that have destroyed 'good management practices'.

7 Concluding comments

In common with Gibbons et al. (1994), the author's view is that Mode 2 should co-exist with Mode 1 and should not be seen as a replacement. Not only should theory inform practice, but also practice should inform theory: achieving this is, however, far from trivial as it will require radical shifts in the social organisation of management research, of the value systems that structure management research and of the funding regimes that drive it. It will also require a radical reshaping of management education if future managers are to be socialised into engaging in the co-production of management knowledge. As a researcher who has always sought to engage with management practice, the author can attest to the fact that the potential for learning and personal development by an academic conducting applied research is considerable but disengagement from theory and theory building would mean that the 'applied research' work the author do could no longer be called academic.

There are real risks for academics if they fail to engage their core stakeholders in more collaborative forms of research. Hodgkinson, Herriot and Anderson (2001, p.s42), for example, comment on a 'worrying trend' which confirms not only a wide, but also a widening gap between academics and other stakeholders which, if current trends persist point towards 'the demise of university academics as key stakeholders in the knowledge

production process' as, for example, consultancies and perhaps even Information Technology (IT)-enabled virtual knowledge production environments colonise the gap that academics have failed, or been unwilling, to occupy. Hodgkinson, Herriot and Anderson (2001), like Pettigrew (2001) see the wholesale adoption of Mode 2 research as problematic and unlikely to close the relevance gap and urge us to move towards undertaking pragmatic research which is high in theoretical and methodological rigour and high in practitioner relevance using a blended research model that combines the strengths of Modes 1 and 2 – or, as Huff (2000) labelled it in her presidential address to the Academy of Management – Mode 1.5. Perhaps, a subsequent article to this one could be labelled 'In search of Mode 1.5' – the characteristics of which are very unclear from Huff (2000) article.

While the heated debates that surrounded management research around five or six years ago seem to have abated (given, no doubt, the focus on the 2008 RAE that has preoccupied senior faculty in the UK universities for the last two or three years), issues about how management research should develop persist and remain unanswered: 'management research' is still seen as problematic in many quarters. The relevance-rigour debate seems not to have been answered, if indeed, the rigour-relevance question was the right question to address in the first place. Academic careers, as increasingly reinforced by the RAE, are still being made following the tenets of a Mode 1 world: it is still publishing in 'top journals' that counts. Attracting research funding (from funding councils such as the ESRC or charitable bodies such as the Leverhulme Trust) is seen by many academics to be more prestigious than attracting research income directly from the end users of management research. In the world of management education, while there are sporadic debates around the design of MBAs often focusing on the MBA dissertation as the prime evidential element of the research component of the MBA, little seems to have changed. In my experience, it is rare to see an MBA dissertation that passes muster as a robust piece of management research with many of them falling unambiguously into the 'populist' quadrant of Hodgkinson, Herriot and Anderson (2001) model: some, unfortunately too many, often seem to display the characteristics of the 'puerile' quadrant.

In the author's opinion, far too large an element of MBAs address the 'how to' rather than the 'why' with the MBA experience of many students being more akin to training in the use of technique rather than to management education geared to developing an awareness of the need for, and the skills to undertake, reflexivity and conceptualisation. A 'professional school' of the type outlined by Hatchuel (2001) and van Aken (2004) needs to have a far better blend of thinking and doing than many UK-based MBA programmes particularly those at the lower end of the MBA esteem continuum. Far too many MBA candidates seem to see the MBA as a salary and status enhancement tool than an educational and developmental experience. Despite attempts to digitise the educational experience, we are many years away from a 'new, IT-enabled agora' that allows multiple stakeholders in management research to co-mingle virtually and, by so doing, share existing knowledge and develop new knowledge and insights. Could emerging technologies help us to reunite the four scholarships of discovery (research), integration (synthesis); practice (application); and teaching (pedagogy)?

What is very disconcerting is that much of the UK research cited here about the relevance of management research to practice was produced, largely through the activities of BAM, in the early 2000s: since that flurry of activity calling for more practitioner-relevant research, the volume of research funding going into the UK

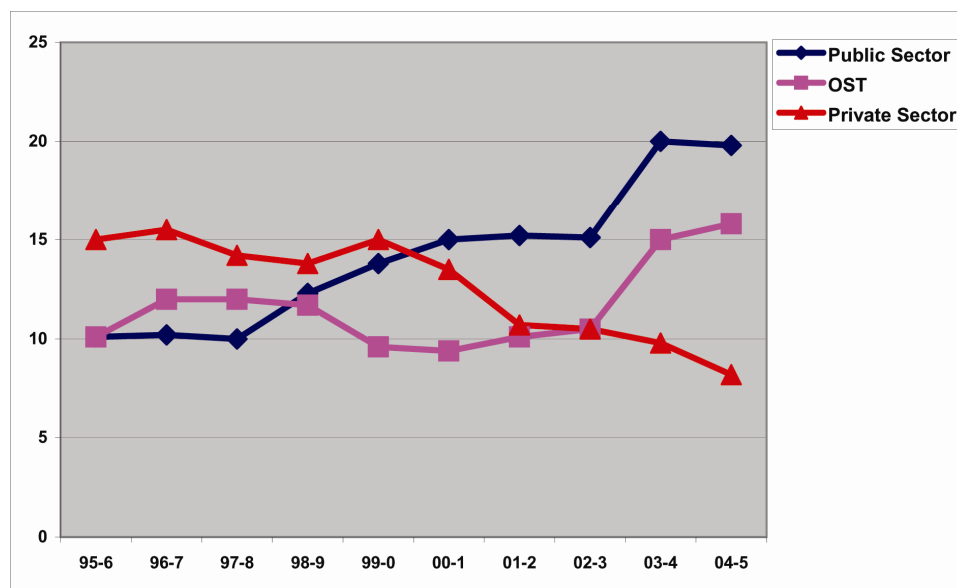
business schools from the private sector has continued to decline. Caswill and Wensley (2007, p.313) remarked that in 2007 ‘opening doors and crossing boundaries’ between management researchers and management practitioners is ‘still work in progress’. If we are exhorting our students to pursue more evidence-based research, as Rousseau and McCarthy (2007) exhort us to do, the author wonder what that piece of evidence is telling us?

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Appendix A Sources of research funding to the UK business schools: 1995/1996–2004/2005
(see online version for colours)



Notes: Source of data, Robin Wensley, ABS Research Conference, 27th March 2007 based on HESA statistics.
Funding is expressed in 1995 GB£ million.
OST stands for Office of Science and Technology.