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Beyond Rigour and Relevance: The Underlying Nature of both Business Schools and Management Research

Robin Wensley

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Robin Wensley,
AIM and University of Warwick
Warwick Business School
Coventry
CV4 7AL
robin.wensley@warwick.ac.uk

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ABSTRACT

This paper aims to develop further the common distinction that is made between rigour and relevance in management research. The debates around this issue have also been closely linked with the critique of Business Schools and in particular the nature of both their students and their teaching. A more critical look at not only the history of these issues but also the institutional nature of the Schools suggests that the underlying tensions are both more complex and also more intractable.

The range of challenges to management research not only in terms of relevance, but also in terms of interdisciplinarity and the nature of disciplinary rigour combine with additional issues about the nature of managerial knowledge and expertise and a wide range of stakeholders to produce a complex environment in which neither market forces nor institutional incentives will necessarily produce the desired outcomes.

However, a wider appreciation of the importance of overall engagement with the range of stakeholder groups and the continued importance of codification and teaching if combined with less of an overall focus on any single set of incentives and objectives is much more likely to aid the further development of management research.

A BIT OF HISTORY

The debate about the nature of both management teaching and management research has a long history. Some might indeed wish to start with the disputes and early experiences around even the formation of that most prestigious Business School – Harvard – in the early 1900s which were hardly propitious (Cruickshank 1987) and in particular its form of research and teaching based on the case study method which remained controversial (Contardo and Wensley 2004). Indeed as Spender (2006) reminds us even this might be seen as too recent, given the importance of the impact of the German Cameralist Schools on the early US developments. The Cameralists had earlier developed and proselytised European notions of mercantilism and the role of industry in the nation state.[1]

However, this paper will start from the mid-sixties when Business School education and research became much more high profile in the UK, with the

[1] Indeed some would go so far as to argue that the Cameralist tradition also provides a better framework within which to consider such current policy concerns as the underlying nature of international competitiveness than attempts to refine a neo-classical economic analysis (Reinert 1994)

establishment of the two graduate Schools in both London and Manchester. At the same time the Social Science Research Council (SSRC) was being formed. With some prescience the first informal meeting of SSRC (11th November 1965) under the chairmanship of Michael Young, noted that :

" The Council would also have responsibility for research in Education and Management: *the latter created particular difficulties* which would be discussed at a later meeting". (highlight added)

By the early seventies there was a renewed focus on the issues of both the value of management teaching and the nature of management research. Of particular note were a number of articles around the future of nature and scale of management teaching.

Even in 1970, issues of expansion were already contentious, as was the issue of the relationship between teaching and more academic orientated research. Harold Rose, then a Professor at the London Business School, produced a report for NEDO that recommended a substantial expansion of UK business schools.

However, the Economist, in commenting on the background to a study by John Bruce Lockhart, previously in charge of management development at Courtaulds, said:

"Academic values have come to predominate over a more vocational kind of training. The balance could be re-established by itself. If it is not, business school academics face the prospect of seeing the whole project going into decline as firms turn increasingly to consultants to supply their educational needs" (Economist January 9 1971:75)

In the same month, Professor J.S. Livingston produced a serious critique of MBAs in the Harvard Business Review noting that:

“Preoccupation with problem solving and decision making in formal management education programs tends to distort managerial growth because it overdevelops an individual's analytical ability, but leaves his ability to take action and to get things done underdeveloped." (1971: 82)

Despite these forebodings, business education flourished in both the US and the UK over the next twenty years, at all undergraduate, postgraduate and executive levels. In the UK, numbers for the so-called “flagship” MBA rose at least tenfold over this period to around 4,000 per annum. One thing that did not change, however, was the continuing the debate about both the usefulness of management education and the value of management research. Indeed in 1992, the Harvard Business Review yet again re-visited the question of the usefulness of the MBA (Linder and Smith Sep/Oct 1992 and subsequent commentary in Nov/Dec 1992). The conclusion seemed to be that MBAs were young and inexperienced, too technically biased, too mobile and learnt the wrong things.

In the UK on the research front the problems were seen as severe enough in 1993 for Howard Newby of the ESRC to agree with the British Academy of Management that a Commission on Management Research under the chairmanship of Professor George Bain, then Principal of the London Business School, should be established to consider the funding and impact of social science research in both business and management[2]. The Commission's report focused on two central issues:

(1) The need to recognise that research in the management field faced the so-called twin hurdles of relevance and rigour.

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[2] This is not to deny that there had also been other important initiatives over the previous period. In particular in terms of engaging non traditional users in management research, was the SSRC/ESRC Open Door Scheme (see Caswill and Wensley 2006)

(2) The need for a more defined and strategic forum in which the priorities for management research could be discussed and resolved.

Within this context the other specific recommendations related primarily to increasing research fellowships and research training, a greater concern for both the ESRC and HEFCE in evaluating research relevance, and improved funding both directly and via various sources of co-funding.

The advent of the new millennium brought little respite from the on-going debate. Indeed a flurry of academic and popular articles generally repeated many of the former concerns, the only difference perhaps being that whilst the nineties had continued to observe substantial growth in MBA numbers the new millennium seemed finally to indicate that demand, at least for the more traditional full-time individual and career investing MBA was maturing or even perhaps in decline in some areas. Pffefer (1995) had earlier started a debate about the extent to which management research was bedevilled by the lack of a coherent and consistent framework or paradigm. The British Journal of Management devoted a special issue to the problems of management research, focusing attention on a number of concerns including the way such research is conducted- the Mode 1/mode 2 debate -, the nature of so-called rigour and the question of relevance. Later Ferraro et al (2005a and 2005b) engaged in a debate with Brazerman (2005) as to what and how management researchers might learn from economists in terms of creating greater impact and also academic status in the field of social sciences. Mintzberg, who was far from being a novice in the previous debate repeated and developed his previous critique in a book entitled “Managers not MBAs” (2004) which was both lauded and challenged (see, for instance, Shepherd 2005). Finally, though perhaps not finally at all, Bennis and O’Toole (2005) rehearsed many of the criticisms voiced more than a quarter of a century earlier by Livingston in indeed the same Harvard Business Review under the rather polemical title “How Business Schools lost their Way”.

In amongst this entire dispute at least three surprises have been evident. The first perhaps is the most obvious: despite the rather dire predictions of the Economist in 1971, Business Schools have prospered substantially in the last thirty to forty years. The other two are the extent to which the overall debate seems to have been conducted on the basis of remarkably little substantial empirical evidence (Pfeffer and Fong (2002) and Baldridge et al(2004), being notable exceptions) and the fact that to a considerable extent the debate has failed to be cumulative over time but exhibited many signs of bald repetition.

To develop further we need to both have a rather better model for understanding both the institutional nature of Business Schools themselves and a better way of deconstructing what lies behind the notions of both rigour and relevance. Again there have been genuine attempts address both of these questions but they have yet to be often properly integrated into the overall debate.

THE INSTITUTIONAL CONTEXT OF BUSINESS SCHOOLS

As we have indicated above, the story of Business Schools in the last thirty years has been one of general expansion, both in the UK and the US. In the UK, the Association of Business Schools was able to report that over the ten years up to 2002, first degree UK enrolment had increased by 30%, postgraduate UK enrolment by 60% and figures for non-UK students had more than doubled in both categories. The general indications, whatever the particular issues with the MBA, were of a growing and secure market. The biggest challenge appears to be maintaining the required level of academic staff given the twin problems of imminent retirement for many and a relative shortage of suitably qualified new staff with doctorates.

Obviously this evidence of market demand should not be seen as gainsaying all the criticisms that have been made of Business Schools and their teaching but it should perhaps cause us to think a bit more. If they are doing so badly as their critics suggest why are they still so thriving[3]? It does seem that maybe the period of collective excess economic rents that the Business Schools could expropriate for the MBA degree may indeed be coming to an end albeit much more gradually and later than the critics suggested but this is only a small part of the picture. Even here the MBA is perhaps moving back to more of an equilibrium situation where only the small elite group of providers can still claim such excess rents on the base of access to networks and personal contacts.

Starkey et al (2004) argue that whilst Business Schools in their current form stand criticised in terms of their research and teaching, let alone their ethical standards, there are ways in which Business Schools can be reconfigured.

In particular they espouse "a growing and influential role in developing knowledge". Alongside this they see it as essential that the Business

[3] Some might argue that Business Schools are thriving only because they pay sub-market rates for their inputs particularly faculty salaries so somewhere between surviving and thriving might be a better bet!

Schools recognise its complicity in the new forms of practice that are seen to have dysfunctional consequences in the wider society.

One might argue that this is to overplay the wider social significance of Business Schools. Indeed it is far from clear how far Business Schools with their essential focus on the management of organisations have also readily and exclusively embraced the language and practice of economic incentives, market mechanisms and private property rights which are the core of many of the wider social prescriptions. Too often there is a tendency to regard the stereotype of the MBA as the sole embodiment of the whole Business School. For instance, Stiles (2004) empirical data points to a much more diverse range of values amongst business school academics.

In general, however, complicity rather than espousal may well be a fair charge, and Starkey et al's argument that:

"The business school stands at the fault line where the future of the university and the future of society intersect. This is an important place, a site where, for better or worse, thinking about the future of business and management will be shaped"

is reasonably compelling and does suggest a different but important role for Business Schools to engage with the wider knowledge agenda. Indeed, as Starkey and Tempest had previously noted:

"The business schools seems to us a potential site par excellence for this kind of (knowledge) interconnectivity, sitting at the interface of theory and practice, of social science and business, and with the capability to manage a knowledge-brokering role, for instance, between science faculties and business"[4] (77)

But such a development would also have substantial implications. For instance, as they recognise, there would need to be changes in the practices of not only business schools but also the end-users of the knowledge. But the questions go further than this to require that we consider whether such an intermediary institutional role is indeed sustainable and why it has not so far developed in this form[5].

[4] We also need to recognise that the "fault-line" however indicates some of the problems of the required process of what in an earlier guise Wroe Alderson described as the sorting and matching process in any retail channel!

[5] Indeed it is worth noting that in the UK alone the management consulting business is estimated to have a total turnover per annum around £10 billion compared with, say, around £1billion for UK Business Schools on a fully costed basis.

It would, moreover, be appropriate to also recognise that at least to the extent to which the previous at least partial complicity evolved it was itself facilitated or encouraged by strong forces such as financial pressures within Universities and the increasing perceived need to define students as customers. Maintaining critical distance from key sources of demand in a market economy is a challenge to which we will return.

Should we interpret the success of Business and Management Schools as merely the result of a collective response by Universities to a short term profitable opportunity? Of course financial viability is an important consideration but we should perhaps have greater confidence in the likely longevity of what is no longer just a young organisational upstart. There would seem to be strong evidence that Business Schools are now embedded not only in a range of individual institutions but also within the Higher Education sector as whole.

Business and Management Schools have undoubtedly grown at least partly because students, parents, sponsors believe that what they do is teach people to be managers and leaders. Maybe indeed to some extent they do but it also true that much that goes within the Schools relates to a wider agenda. We should be hardly surprised that this is happening. It is not unusual in Universities, as Abbott notes:

“Today, most university professors of English have as little to do with teaching writing as they can manage, such teaching having been shifted to graduate students and hirelings steadily over the years. The more prestigious the university, the more extensive the shift. The trend holds despite the belief – of the parents who send their children to college, of the state legislators who most often pay the bill, and of the students themselves – that the major reason universities have English professors is to teach students to write. English is of course hardly alone” (2001: 145)

THE CHALLENGES FOR MANAGEMENT RESEARCH

If, on average, Business Schools themselves have seemed to be in a reasonable shape, the same cannot yet be said for management research. Indeed it is also true that a number of those critics of what is taught in Business Schools and to whom conflate their concerns with the nature of the research undertaken. Bennis and O’Toole (2005) are fairly typical in this regard in arguing that it is the focus on what they term “scientific” research and the faculty they hire to do this which means what is taught neglects the issues of complex and unquantifiable choices. The issue of a “problem” seems widely accepted the question is how to frame both the problem and the specific solution. Indeed the Bennis and O’Toole approach seems to primarily fall into the “management research is different category” as opposed to the “management research is not scientific enough” category. This issue of the extent to which we see management research as different or as in deficit within the social sciences has lain beneath many of the debates in this area (Caswill and Wensley 2006). In this long running debate - sometimes explicit but more often implicit - it does not come much more explicit than Tranfield and Starkey (1998) who

"argue(s) for the distinctiveness of management research... the key defining characteristic ..is its applied nature and its central concern should be "the general (engineering) problem of design".

More recently, however, Tranfield in discussing further his conception of a so called design science perspective of management research (see also van Aken 2005), to which we will return later,, rather surprisingly argues that for evaluation purposes:

"(the) idea of dual hurdles for management research, summarised under the general categories of academic rigour and practitioner/policy relevance, provide a good start" (2006:13)

To look at this assertion more critically we really need to deconstruct somewhat the twin notions of rigour and relevance that have been often used to characterise the nature of the challenge for management research.

THE NATURE OF RIGOUR

Whether rigour is seen as part of the problem or part of the solution depends somewhat on the commentator. Bennis and O'Toole for instance argue rather cryptically that the

“ (the) problem is not that business schools have embraced scientific rigor but that they have forsaken other forms of knowledge “ (2005:104)

We assume they mean by this knowledge derived by the scientific method, and clearly they are right that there are other relevant sources of knowledge. However it is not clear whether they do or do not wish to also indicate that such potential sources of knowledge should themselves be subjected to rigorous inquiry. The extent to which they quote approvingly from the philosopher A.N.Whitehead suggests they would support such a requirement but it remains unclear.

In this sense a rigorous approach might be defined in rather broad terms as one that involved a critical evaluation of the underlying theory, evidence base and inferential links. However, as others have pointed out, a more systematic but equally more restrictive framework within which to assess the level of rigour in any piece of research is to be found in the core disciplines such as sociology, psychology and economics.

There are certainly those who are believe that a lack of disciplinary rigour is part of the problem for management research. Kilduff and Kelemen (2001), for instance argue that it is important that management research benefits from the rigour that is bought about within the codifications and conventions of particular disciplines whilst Pettigrew (2001) argues that better training in management research will result if individuals are more expose to the rigours expected in one or more of the core disciplines. Anderson et al (2001), at least in the context of industrial, work and organizational psychology go further and argue that without an attachment to disciplinary rigour we risk engaging in either popular or, even worse, puerile research.

However, we should also recognise that this privileging of disciplinary rigour itself does not go uncontested. McCloskey has consistently challenged the economics discipline, seen to many in social sciences as the most evident source of disciplinary rigour, about the actual nature of its' academic discourse (1986, 1994) and argues that the legitimation of a particular form of generally quantitatively based discourse in academic research, is to be seen as no more and no less than an effective rhetorical device. Rigour, by implication therefore, is not to be seen, in a disciplinary sense, as some form of objective criterion but the outcome of a wider political debate within a discipline about the preferred forms of argumentation.

THE ISSUE OF INTERDISCIPLINARITY

Much emphasis has often been given to the argument that notions of individual disciplinary rigour are particularly inappropriate in management research because of the essentially problem driven and therefore interdisciplinary nature of the work. This argument is often also presented as also one related to historical development if not also progress: from disciplinary to interdisciplinary research. Whilst there is little doubt that the nature of many management research issues can be seen as encouraging an interdisciplinary approach, Abbott has argued that, in fact, the interest in interdisciplinarity in the social sciences more broadly is not a recent phenomenon:

"In short, over the last forty years, a serious interest in Interdisciplinarity appears to be an almost stable concomitant of the disciplinary system… Indeed the long history and stability of interdisciplinarity - unsuspected by its current publicists raises interesting questions of why interdisciplinarity has not transformed the intellectual system, even though now that it has been a permanent feature of the American intellectual landscape for so long" (p134)

His own explanation for this failure focuses directly on the particular problem driven nature of interdisciplinary work:

"There is ample evidence that problem-orientated empirical work does not create enduring, self-reproducing communities like disciplines except in areas with stable and strongly institutionalized external clientele such as criminology" (p 134)

Even in these circumstances, as Abbott notes, there tends to be a one way flow of academic appointments: criminology departments hire sociologists but not vice versa.

Indeed it is worth noting that in this sense management as a discipline has made substantial progress: it has a very substantial teaching activity from undergraduate programmes through to recognised doctoral programmes. It has its own academic labour market and it even to a limited extent exhibits some degree of two way flow of appointments with other social science departments.

Alongside this demand effect Abbott also considers the impact of both the structure of the academic labour market and the dominant design of undergraduate curricula.

His second explanation relates to the assertion that

"there are far more research problems than there are disciplines so many, in fact, that a university organized around problems of investigation would be hopelessly balkanized"

He argues that it would be impossible to recreate the enduring structures of academic disciplines with their key rationale in terms of both careers and activities on the basis of an ever changing portfolio of diverse problem domains.

However, Abbott recognises very clearly that this level of relative stability of the disciplinary structure has to be sustained alongside a continuing flux in terms of ideas as well as problems.

This leads to a set of complex and time dependent linkages between the disciplines themselves. Unlike the natural sciences where at least to a considerable extent what Abbott calls the axes of cohesion within the discipline can be articulated within some form of levels of analysis, the equivalent axes of cohesion for the social science disciplines do not relate to each other in any particular hierarchical form. So:

"Overlap means that scholars who may seem to be in the hinterland or backwaters of their home discipline are often cheek by jowl with the leaders of other disciplines and hence in a position to borrow from them highly developed techniques that can reroute their own mainstreams" (p143)

In most cases therefore it is important to recognise that the process of disciplinary development is co-evolutionary and path dependent, accompanied by periods of growth, fragmentation and consolidation. Indeed we should encourage multi-disciplinary work but not expect stable interdisciplinary combinations and alliances. Maybe, however, there are also actually strong signs that management itself is achieving the institutional characteristics of a discipline and as such developing its own specific criteria for rigour and research standards.

We could go further and argue that the more important concern to evaluate such work, at least in the context of management research is to establish an appropriate framework for such argumentation more from first principles than merely imitate a particular existing discipline. Such a framework might, for instance, by analogy with issues of common law, establish the criteria for the admission of evidence, define the nature of the process – whether it is adversarial or investigative, and resolve the basis for the burden of proof – the balance of probabilities or "beyond reasonable doubt"[6].

THE ISSUE OF RELEVANCE

As we have suggested above the issue of relevance has be-devilled management research for a considerable period. In the more recent debates there has been considerable repetition of anecdote and allegation rather than firm evidence. To be fair, such use of relatively unsubstantiated evidence has not been solely amongst the critics of management research,

[6] We could of course move even further away from forms of argumentation based on statistical approaches to consider the legitimate roles of rhetorical tropes for instance (see White 1987)

in many ways the positive arguments presented by Christensen and Raynor (2003) suffer similar flaws. However there has also been some attempt to introduce some evidence and analysis into the debate: most notably by Pfeffer and Fong (2002), who conclude that on balance there is a problem and by Baldridge et al (2004) who conclude that there is not one. Equally other commentators have challenged the extent to which relevance itself should be seen as an important evaluative criterion anyway. In particular Grey (2001) argues that independence is more important than relevance and indeed is in grave danger of being compromised if too much weight is place on relevance

Weick (2001) with his usual insight rather turns the issue of relevance, at least to practice, on its head, when he argues that:

"The much lamented 'relevance gap' is as much a product of practitioners wedded to gurus and fads as it is of academics wedded to abstractions and fundamentals. The gap persists because practitioners forget that 'the' real world is actually 'a' world that is idiosyncratic, egocentric and unique to each person complaining about relevance. Greater attention to the conceptual underpinnings of fads and egocentric perception suggest the existence of more fundamental barriers to effectiveness such as ceilings on improvement, weak situations, ambiguous signals, non-obvious adaptive forms and preoccupation with vision, Joint practitioner-academic effort devoted to questions of how events come to be seen as 'real' could re-bridge a gap whose nature has been misidentified."

On top of this concern, others have commented rather ironically that at least in the UK, the behaviour of Research Councils can reinforce a focus on fads rather than evidence (Newell et al 2001). In particular what can inform the development of research themes can be the very sources of information that are also often seen as one of the sources of the fads themselves.

In a number of these comments there are echoes of the distinction that Eldridge[7] (1986) developed between what he called convivial and critical relevance when he reviewed the development of some significant contributions to management research. In particular he drew attention to Joan Woodward's programme of research on the impact of technology on organisations in the fifties. Whilst recognising the practical usefulness of this research, he also recognises the extent to which it failed to treat various aspects of managerial control as problematic:

[7] Eldridge was himself involved in disputes around the research of the Glasgow University Media Group in the late seventies and early eighties

"It is perhaps the questions that are not asked that in part enable one to distinguish between a sociology which is congenially relevant to an interested party and one which is critically relevant. If we neglect to ask what a particular form of technology is designed to do, in whose interests and with what consequences, it does save a good deal of unpleasantness. If we do not inquire into the structure of domination within which typologies of technology and control systems are embedded , then the critical relevance of the theory is diminished." (p175).

The notion of relevance therefore in management research can be seen as flawed, if (to use Eldridge's terms) it is used or defined solely in the sense of “congenially” relevant to a partial group, community or their representatives, rather than “critically” relevant.

INDICES OF RIGOUR AND RELEVANCE

Arguably, as Bladridge et al (2004) assert, the best indicator of disciplinary rigour is publication of the relevant work in a high quality academic journal and an indicator of relevance is the extent to which the topic of the research is regarded as of interest to a representative group of users.

Even here however we encounter problems, which in general terms conflate research, or more strictly research design with outcomes and interpretation. In the academic sphere, Armstrong (1996) has long argued that in practice such confusion arises even in the acceptance process for academic journal where manuscripts are rejected on the basis of the results rather than the research design. Indeed his analysis bears some relationship to Eldridge's but in this case we are consider convivial relevance more from the perspective of the academic elite. If this is true for the manuscript acceptance process then it must be even more true for the citation process which is the basis of the most commonly used measure of academic quality: the citation count.

It is also worth noting the orders of magnitude in the case of citation counts. On the most commonly used Social Science Citation Index (SSCI), a paper in a top journal faces an average citation count (in all other journals in the database) of often around three per year, whereas the figure for a middle ranking journal maybe one or less. The equivalent range for one article may vary from at least 25 at the top end to a vanishingly small fraction at the bottom. Even at the higher end this is a small figure compared with the number of Journal copies distributed, which can vary between around 2,000 where there is no link to a large Professional grouping to over 10,000 in the case of journals linked to the Academy of Management[8].

On top of this, clearly achieving a high citation rating is not just an effect of rigour: in terms we have used before it might be seen as more linked to issues of academic fads or fashions. Ironically therefore the citation measure may itself be a measure of relevance or impact rather than rigour, even if more to an academic rather than a policy or practice community.

Good measures of relevance to practice and/or policy are perhaps even more difficult to come by or interpret. Most common approaches have tended to look either at the authors of best selling management books, or at surveys (often not very carefully controlled in terms of the respondents) on gurus or experts. Even so we tend to find a contradiction in that only a minority of authors of best selling management books are academics but a majority of nominated management gurus are academic or academic

[8] Strictly the US Academy of Management calls itself the Academy of Management although there are shades of the notion of a World Series in which only one, admittedly large, country really participates!

affiliated. Compared with our numbers for citations the sales of books (or maybe the actual fees for individual gurus although these are a little more difficult to come by!) are impressive. In terms of books figures in excess of ten thousand copies per annum are common[9].

Pfeffer and Fong (2002) focus attention on various measures of what might be termed practice impact. For instance they noted that the proportion of best selling management authors that could be described as academic researchers was small: of the order of 10%. Based on these and similar statistics they concluded that the impact of management research on practice was distinctly limited. Baldridge et al (2004) on the other hand approached the empirical analysis from the other direction and looked at the potential practitioner interest in academic articles published in top quality journals. To assess this they had to engage in a translation process and summaries the content of each paper on two pages that was then assessed by what might be regarded as a somewhat positively biased sub-set of the

[9] It may well be that the advent of new data formats such as those provided by Google Scholar may got a long way in providing more balance measures of citation effects between both journal articles and books.

overall population. However, they did conclude that there was a rather weak but positive correlation between academic quality measured by citation counts and practitioner interest revealed by their particular method[10].

FADS AND EVIDENCE

Weick's earlier comment above reminds us that the most obvious juxtaposition to represent the so-called relevance gap is to be seen in the fads vs. facts distinction or indeed the management guru vs. careful researcher. These distinctions may be useful for a rhetorical debate but they also need to be put in some context. In common parlance and indeed some key academic commentary (Abrahamson 1991) the notion of fads is twined with that of fashion. Yet is we look behind this issue, fashion can often be seen as the result of situations in which there is social interaction and imitative behaviour. One might ask what is the fundamental difference between "fads" which might be more often seen as merely rational behaviour (such as the common adoption of a particular standard: VHS, MP3)

[10] Of course it is important to also note that the practitioner respondents as members of Journal Advisory Panels might be presumed themselves to be somewhat biased towards a positive assessment. Overall therefore we should perhaps interpret their relatively weak association as no more than, to use the Scottish terminology, a "not proven" verdict!

compared with those which are seen as fashion (particular colour or styles as signifiers of membership of particular groupings – or “tribes” as Cova (1999) chooses to describe it).

Equally, we can get beyond general concepts of both academic rigour and also practice relevance in management research if we confront more directly the nature of the task of what we might term engagement. This means addressing the direct question of what informs the nature of managerial action. As our discussion above indicates we have to start from the reasonable assumption that in most cases the direct impact of general, what might be termed scientific, knowledge as reflected in our academic journals is very limited.

Much more significant is what Pearce (2004) refers to as "folk wisdom" or what anthropologists would term, if they applied the concept beyond mainly exotic tribes, "vernacular knowledge". In her 2003 Presidential Address to the Academy of Marketing, Pearce considered the two worlds of academic research and folk wisdom. This is an important shift away from just a discussion of relevance. Primarily it recognises that in the world of practice, which as Pearce notes can include a number of us academics too, there is a well-established alternative in terms of knowledge and wisdom. It may operate to different rules of evidence and validation let alone dissemination but it is a very real option.

As Pearce also notes there are areas where such knowledge domains overlap (or at least are mutually reinforcing) such as in the understanding of various attribution biases but there are others where either the overlap is much less clear or indeed in the extreme where the available research evidence suggest a failure in the folk wisdom. A topical example might be performance related pay that currently seems to have much folk wisdom support but at best relatively little empirical evidence as to its positive impact on organisational performance.

As Pearce recognises therefore the critical debates and behaviours in this area should be "beyond relevance":

"The final problem of living in these nearly separate worlds (of scholarship and folk wisdom) is that I fear that it impoverishes the scholarly world. Keeping them separate makes it easier to leave managers' important challenges out of our scholarship. I am not saying our research is not relevant - that old chestnut. No. I think most of it is quite relevant; certainly there is a lot of good relevant research being done. Rather much of it is relevant for those less critical, more technical problems, rather than for the most important problems that those coping with large, complex organizations face" (176)

Whilst the more detailed analysis of vernacular knowledge had often been restricted to the activities of anthropologists looking at primitive or exotic tribes, it is also to be found in some education research. One particularly interesting area is that of so-called “Street Mathematics” (Nunes et al 1996)

The basic issue is the extent to which “Street Mathematics” can be seen as a form of vernacular knowledge which is not just a very focused set of procedures for dealing with particular practical problems but also capable of some degree of extension and implicit generalisation. The claim is not that there is an equivalent degree of formalisation and conceptual complexity with the more codified “School Mathematics” but that in these terms the boundary between the two is less rigid than is often assumed. In this sense it could be seen as to some degree alternative form of codification of mathematical knowledge compared with the more "common" form of mathematics as it is taught in Schools.

Interestingly there is also an extent to which these two forms of knowledge can interact in unexpected ways. For instance, Nunes et al note that an apparently simple arithmetical question such as "there are 10 birds on a line, two are shot, how many remain" requires a form of suspension of practical knowledge to arrive at the answer eight. In most normal circumstances the remaining birds would have flown away!

The process whereby knowledge derived from practical problem solving becomes codified and generalised might be seen as analogous to the development of what has been termed folk wisdom in management practice. Two particular aspects of this analogy are of interest: the nature of formalisation and the context dependency of experience. Not surprisingly the formalisation of management problems represented by most econometric and statistical modelling reflects a very similar basic approach to that used in "school mathematics", and therefore we might expect that just as with street mathematics the formalisation implicit in folk understanding of management would be quite different.

To some extent certain ethnographic and qualitative studies of managers and management provide some hints as to the nature of this alternative formalisation. We might note for instance Tony Watson's comments that:

“managerial competence or effectiveness is indeed a subtle, multi-faceted and context-bound thing. It does not just involve skills and attitudes, but encompasses knowledge even if that knowledge is stored in the form of intuitions and is manifested in what Donald Schon calls reflection-in-action” (p223)

This also links to the other main aspect: the context dependency question. Whilst in a quantitative framework we attempt to characterise context with a number of specific variables, this again rarely equates with the way in which individual managers would define the specifics of the situation they face. In such situations it is therefore hardly surprising that we find the nature of what might be called knowledge transfer is individual and idiosyncratic. As Huxham et al (2005) illustrate, the transfer of experiential knowledge depends more on a particular form of storytelling. The speaker provides a series of loosely linked anecdotes and experiences and individual listeners weave this into a coherent narrative from their own perspective.

A more fundamental analysis of the nature of folk theories is to be found in the work of Oaksford and Chater (1998). Their analysis of folk theories is primarily part of an argument they develop against the emphasis on the principles of logical inference in understanding the nature of human thought. Their central proposition is that we need to shift our perspective on human reasoning towards a probabilistic perspective to recognise the inherent uncertainties of thought and action. They start from the basic proposition that the key characteristic of folk theories is the degree to which they generate inferences that are defeasible, in other words capable of being readily defeated by further information. This is contrast to the nature of scientific inference that requires a much higher level of robustness to further information. Hence they argue that judged against normal criteria, folk theories will always be bad science in domains that are well understood scientifically. In particular there are severe ontological issues around the nature of the entities that are used as the building blocks of such folk theories:

" The upshot of these considerations is that, if common sense consists of folk theories, then the nature of these theories is unknown and only indirectly related to explicit verbal behaviour. The problem of discovering the theories underlying commonsense thought seems, therefore, to be analogous to, the problem of discovering the underlying knowledge of language that governs linguistic behaviour... the resulting linguistic theories are highly elaborate and sophisticated and are, of course, entirely inarticulable from the point-of-view of everyday speakers" (p 155-156)

However, they also note:

"Folk theories must allow us to make the best possible sense of our everyday world and guide our actions as successfully as possible: to do this they must face up to the full complexity of the everyday world, which, we suggest science rightly prefers to avoid...(folk theories) must deal with domains in which good science is more or less impossible and rough and ready generalisations must suffice. Thus the fact that folk explanations do not stand up to scientific scrutiny should not be viewed as a criticism of folk theories: it is an inevitable consequence of the fact that folk theories must venture where science cannot" (p166-167)

Their analysis links, as they recognise, to a number of issues originally developed by Herb Simon under the general framework of bounded rationality, which also provides some interesting issues with respect to the design science perspective to which we referred earlier (van Aken 2005). Essentially the design science approach is closely analogous to the Simon proposal that procedural rationality was a more appropriate objective in complex situations than substantive rationality. It is indeed true that in systems terms the application of procedural rules is likely to be underdetermining of outcomes compared with substantive rules. However, as Oaksfield and Chater remind us, if the underlying epistemological and ontological status of the basic elements of either types of rule remains contentious it may just be that we achieve apparent validity only because the evidential conditions for refutation of the rule are less well defined.

Hence, it is more important, rather than denigrating one set of knowledge structures or indeed behaviours or to privileging another, to understand more about their individual and collective rationale. In the context of sociology, for instance, Burroway argues that various forms of sociological inquiry and commentary need to co-exist, based on whether the knowledge generation process is instrumental or reflexive and the audience is intra- or extra academic (2004). He typifies four forms that he labels professional, policy, public and critical sociology but also notes that such an analysis can be readily applied to other social science disciplines.

His analysis is particularly useful in that it provides an important rationale for the co-existence of the different modes of enquiry and engagement and his focus on the way in which the key characteristics differ between the modes. The approach also helps to focus attention on the contentious issue of incommensurability. Ever since the Burrell and Morgan (1979) the issue of incommensurability has been a challenging topic in management and organisational studies. Whilst it remains a topic of lively debate (see for instance Hassard (1988) and Weaver and Gioia 1994) we would argue that in fields such as management research we are required to ensure forms of debate and dialogue across different perspectives and assumptions about the nature and purpose of empirical enquiry.

BEYOND RELEVANCE: RECOGNISING THE ROLE OF DISCIPLINES, MULTIPLE STAKEHOLDER INTERESTS AND ENGAGEMENT

We should also note how Burawoy deals with what he terms “the form of legitimacy”: he juxtaposes “scientific norms” in the professional category with “relevance” in the public category. Whilst this helps to emphasise the point we made earlier that we must be careful not to conflate a general notion of rigour with a very specific set of disciplinary scientific norms – as perhaps Anderson et al (2001) are in danger of doing in their analysis, it can be rather misleading in the context of the notion of relevance. We should perhaps redefine the basic notion behind relevance to recognise more clearly that there are not only multiple modes of enquiry but also multiple, and heterogeneous stakeholders to be considered. Indeed the issues raised by stakeholder heterogeneity are quite critical in this context. It is very easy for critics to dismiss the specific topic of a piece of research as not relevant where it would be more appropriate for them to ask for evidence that there is significant interest in the research amongst at least a sub-group of one of the stakeholder communities.

Any large community inevitably represents a wide diversity of interest but it is useful to consider three core and large stakeholder communities in the context of management research: Practitioners, Policy Makers and Academics. The practitioner community in general looks for research that provides understanding and prescription often in a very specific context and with a well defined and relatively small unit of analysis. The policy community although interested in understanding is on the other hand often more interested in aggregate effects and the likely behaviour of groups of units or agents rather than single entities, particular to changes in incentives or context. In terms of both of these stakeholder groups, as Bazerman (2005) has argued there is a general concern for prescription but others would suggest that this should not be the sole guiding principle of the research activity (Ferraro et al 2005). Again as a broad generalisation, academic stakeholders themselves are more interested in understanding and the ways in which the research both reinforces or indeed challenges particular theoretical approaches and adds to both the knowledge stock and the implications for further work.

There is also an issue about the appropriate forms of engagement in the research process itself. In more recent discussions about the nature of management research, there has been much discussion about the so-called Mode 1 and Mode 2 models of the research process (see Huff and Huff 2001 and Starkey 2001) and how these might be applied. As the originators of the basic models, Helga Nowotny, Peter Scott and Michael Gibbons comment wryly:

“Of course, like all theses that gain a certain popularity (and notoriety), this thesis was radically simplified, and collapsed into a single phrase – ‘Mode 2’. The old paradigm of scientific discovery (‘Mode 1’) – characterized by the hegemony of theoretical or, at any rate, experimental science; by an internally-driven taxonomy of disciplines; and by the autonomy of scientists and their host institutions, the universities – was being superseded by a new paradigm of knowledge production (‘Mode 2’), which was socially distributed, application-oriented, trans-disciplinary, and subject to multiple accountabilities.

Those with most to gain from such a thesis espoused it most warmly – politicians and civil servants struggling to create better mechanisms to link science with innovation; researchers in professional disciplines such as management, struggling to wriggle out from under the condescension of more established, and more ‘academic’, disciplines; and researchers in newer universities, other non-university higher education institutions, or outside the academic, and scientific, systems strictly defined. “ (2003: 179)

However, rather frustratingly they also note that some of the central issues to which this paper is addressed still need to be developed:

“The fifth and sixth contexts to which we attempted to apply the idea of ‘Mode 2’ were the least well developed. They were, first, the potential re-configuration of institutions that flowed from the wider distribution and greater reflexivity of knowledge production; and, second, the management of 'Mode 2' knowledge. These are key issues. The modern world is populated by expert institutions, which are not only essential for the advancement of social and technical progress and professional careers, but also shape personal and group identities and influence both the constitution and the uses of knowledge. Similarly the production of knowledge, however widely distributed, however trans-disciplinary, however heterogeneous, however reflexive, has to be 'managed'. More choices have to be made more urgently about scientific priorities. Although the explosion of choice may make it more difficult to aggregate them into, or shape them within, the framework of planned programmes, this does not mean that the problem of management has disappeared. Clearly 'Mode 2' knowledge must be managed in new ways. These are themes to which we intend to return in a third book " (2003: 189)

We would like to focus on two particular aspects of the debate around Mode 2. They relate to the notion of the wider public debate around the nature of scientific research, the *agora*, noted approvingly by Starkey (2001), and relationship between research and teaching. Again it is worth quoting Novotny et al on their own reflections on the notion of agora:

"This archaism was deliberately chosen to embrace the political arena and the market place – and to go beyond both. The agora is the problem-generating and problem-solving environment in which the contextualization of knowledge production takes place. It is populated not only by arrays of competing ‘experts’, and the organizations and institutions through which knowledge is generated and traded, but also by variously jostling ‘publics’. It is not simply a political or commercial arena in which research priorities are identified and funded, nor an arena in which research findings are disseminated, traded, and used.” (2003: 192)

Hence, whilst the whole notion of wider debate could be seen by some as a single or integrated form of organisation, in this approach it must be a set of loosely coupled debates and discourses. We therefore need to consider and indeed experiment with various organisational forms and processes for engagement with various groups of stakeholders around all stages of the research process including priority setting, empirical work and dissemination. We also need to recognise that this issue is further complicated by multiple levels of analysis and interests. In general those with a more general interest in agenda setting may have a different interest from those engaged with a specific and focused research project and both these groups may again differ somewhat from those who have an interest either directly or as intermediaries in the so-called dissemination process. Similarly all these processes can have significant elements of interaction and co-production but these elements will, of course, be enacted in different ways in different contexts.

Finally, to return to where we started there is the question of not only what is researched but also what is taught. Again to start with Novotny et al, they note:

"The great increase in the number of students over the past half century, and the equally spectacular expansion of research, have often seemed uneasy bedfellows. Between 1945 and the mid 1970s, the former initially enhanced the resource base for the latter, but in recent years, these two elements have become increasingly competitive. More seriously, mass access and high-quality research have come to be driven by, and to address, different value systems. But this may partly be explained by the persistence of traditional – 'Mode 1' – accounts of research. Within the context of 'Mode 2', these tensions are reduced, and new synergies are apparent between the democratization of higher education and the wider social distribution of knowledge production. " (2003: 188)

In the context of management as we have discussed earlier the expansion of teaching activities has been even more dramatic and at all levels. To arrive therefore at a more balanced critique of what is researched and what it taught in Management and Business Schools we must pay as much attention to the nature of what is taught, particular both our texts and also our participants, as to what is researched. Indeed much academic research may be more influential on practice through incorporation in our core texts than through direct readership of either academic articles. We have already also noted that at least in general it is not the fruits of academic research but more the perspectives of successful practitioners that feed into the more popular management texts[11].

Henry Mintzberg as well as others' critiques start from the premise that "pretending to create managers out of people who have never managed is a sham". The question might be more who is doing the pretending to whom. Overall the way in which the nature of what is taught and how it is then used is already defined both by participants and those organisations that are sponsoring or employing them, as well as by the providers.

[11] Indeed this process extends to those more satirical representations of management wisdom which are found in the highly popular Dilbert cartoons, fed apparently by many stories sent to Scott Adams from those at the workplace!

CONCLUSIONS: HAVE BUSINESS SCHOOLS LOST THEIR WAY?

It would seem that the repeated claims that in one way or another Business Schools are failing in what they teach and what they research are misplaced. The evidence that we currently have at best suggests that, as we might expect, both individually and collectively Business Schools can improve but that rather than having "lost their way" Business Schools are continuing to address the demands of a large and heterogeneous population of participants for their teaching and development activities. At the same time, in the context of the management research, there do remain important issues that still need to be addressed but in general the evidence hardly suggests that these issues are being ignored.

However, as we strive to ensure that management research activities are organised so that there is more engagement with relevant stakeholders, more attention to appropriate research design and methods, and careful and systematic attention to previous evidence and theory, we also need to appreciate the effect of differing and sometimes conflicting demands both within Business Schools and wider in the University system. This almost certainly mean that sustainable improvements will require a significant change in not only economic incentives but also need to develop new mechanisms and institutions to reinforce the desired changes.

Amongst these new mechanisms could well be ways to ensure that the research agenda is to some extent more clearly guided by the views of the various stakeholder groups and also ways in which, in certain areas, we can ensure that there is both more designed experimentation in specific approaches related to engagement, and also more systematic approaches to the use of cumulative empirical evidence as well as more recognition of the multi-faceted nature of management research.

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Advanced Institute of Management Research
6 -16 Huntsworth Mews
London NW1 6DD

Telephone: +44 (0) 870 734 3000
Fax: +44 (0) 870 734 3001
Email: aim@london.edu

www.aimresearch.org

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