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Management Research After Modernism

Andrew M. Pettigrew
Warwick Business School, University of Warwick, Coventry CV4 7AL, UK
email: ccscap@wbs.warwick.ac.uk

If the duty of the intellectual in society is to make a difference, the management research community has a long way to go to realize its potential. The Starkey and Madan (2001) report is a useful entry point into the debate about what kind of management research, but it defines the issues too narrowly and seeks solutions too particularly. The big strategic issues about management research are about capacity, capability and delivery. In an era of knowledge production after modernism there is a more receptive context to meet the double hurdles of management research. Research without scholarly quality will satisfy no one and will certainly disable our capacity to meet the double hurdle of scholarly quality and relevance. A more contextualist and dynamic view of knowing needs to be supported by a re-engagement of management researchers with social scientists and users, a re-engagement between European management researchers and their colleagues in the USA and a period of experimentation and learning with all the potential partners out there waiting to engage with us.

Ken Starkey and Paula Madan's report 'Bridging the Relevance Gap' offers some topical insight into a long-running issue where there are no easy answers. For most of the twentieth century, social scientists have debated 'knowledge for what purpose' (Lynd, 1939). As these debates have moved in and out of fashion so they have been mirrored by changing social and economic contexts, altering expectations of science and a greater variety of conduct of science in different natural and social science disciplines (Nowotny, Scott and Gibbons, 2001). One of the lessons from the natural history of development of the social sciences is that there can be no one best way of framing, producing, disseminating and using knowledge. The changing contexts and content of knowledge in the social sciences and management will pierce any bubble of conventionality which claimed universal appeal and applicability. In this sense, Gibbons *et al.*'s (1994) Mode 2 form of knowledge production is no more the answer to contemporary doubts about the relevance of management and social science research than any of its predecessors.

In the sequel to *The New Production of Knowledge* (Gibbons *et al.*, 1994), Nowotny, Scott and Gibbons seek to reframe and widen their message in order to condition and contextualize it. In locating the development of Mode 2 knowledge production in a co-evolutionary process with the development of a Mode 2 society, Nowotny *et al.* encourage a much more theoretical treatment of science in an age of uncertainty. In so doing they also seek to counter two criticisms of their earlier influential book. They have clearly been stung by the view that *The New Production of Knowledge* has become an apologia for applied science. They also wanted to shift the ground of the debate in this new book to counter the view that *The New Production of Knowledge* could be assessed purely in empirical terms.

The danger and the promise in the Mode 1–Mode 2 knowledge production debate lies in the dichotomous form of the argument. There is a long tradition in the social sciences and management of using bi-polar modes of thinking. These bi-polar concepts are variously portrayed and used as dichotomies, paradoxes, contradictions and dualities. Dichotomies are remembered. They are powerful simplifiers and attention directors as the influence of Burns and Stalker's (1961) mechanistic and organic systems and Lawrence and Lorsch's

(1967) differentiation and integration of structures testify. Dichotomies also tease. They may provide much analytically but deliver little in empirical analysis or practical application. Dichotomies may then conceal as much as they reveal and close down debates as readily as they open them up.

In the management research community we should thereby use the Mode 1–Mode 2 dichotomy to open up debates about the character, quality and relevance of the knowledge we produce, and not slip into the easy but shallow option of saying the answer lies in Mode 2 (whatever that is in practice). If Gibbons *et al.* (1994) have been chastened by the way their book has been stereotyped as an apologia for applied science, I feel similarly flattered but also subdued by the narrow interpretation put on 'The Double Hurdles for Management Research' (Pettigrew, 1997a). In the original speech (Pettigrew, 1995) and subsequent chapter (Pettigrew, 1997a), I was very careful to use the plural 'double hurdles', and yet the theme of my message has been picked up in the singular as the double hurdle of scholarly quality and relevance. This is not to deny the significance for us all of attempting research which meets the challenges of scholarly quality and relevance, but merely to re-affirm my original argument that:

> 'the challenges for management research are best captured in a series of concurrent double hurdles which together raise a wide spectrum of cognitive, social and political demands on our skills and knowledge as researchers'. (Pettigrew, 1997a, p. 291)

My view now, as then, is that the complexity and uncertainty of the knowledge production process, demand of us the exploration of many different types of knowledge production, user engagement and mechanisms of impact. Morgan (1983), referring to a narrower landscape of knowledge, expressed this need well in terms of 'conscious pluralism'. In the absence of unambiguous foundational truth in the social sciences, the only sensible way forward can be conscious pluralism. If we are open to exploration and exploitation in our research efforts this pluralism will naturally arise as we attempt to meet the challenge of the concurrent double hurdles. Of these hurdles the most important are:

- scholarly quality and relevance;
- engagement with the social science and management literatures; at appropriate moments and with appropriate partners, combining knowledge production and use;
- ensuring the internal development of the fields of management whilst engaging with co-beneficiaries and co-producers;
- challenging and transcending the current beliefs and knowledge of those stakeholders we work with whilst also engaging with those beliefs;
- preserving researcher autonomy as a precondition to build scholarly identity and creativity, whilst encouraging different forms of engagement with stakeholder communities.

The Starkey and Madan (2001) report is a useful entry point into the debate about what kind of management research for the early part of the twenty-first century, but in my view it defines the issues too narrowly and seeks solutions too particularly. The three strongly-made points in the Starkey and Madan report are the recognition of the relevance gap and the search for causes amongst the management research community and potential users; the stated need to increase the stakeholding of users in various stages of the knowledge development process; and the recommendations for new forms of research partnership and research training to address the relevance gap. Important as this agenda is, it is also too narrow. The relevance gap is not the only strategic issue facing management researchers at this time. There are big strategic issues of capacity, capability and delivery. Research without scholarly quality will satisfy no one. More than most the management research community is losing out in the talent wars as fewer and fewer able UK postgraduates are doing PhDs as a stepping stone to a university-based career. With time there is also evidence of a disengagement between the various management fields and the disciplinary areas which have traditionally nourished them. Alongside stronger and more creative engagements between management researchers and users, there is also a complementary need to re-engage management research with the social sciences, whilst also maintaining the intellectual developments of the various sub-fields such as strategy, marketing, organization studies and so on.

In an era of knowledge production recently labelled by Clarke (2000), Whittington, Pettigrew and Thomas (2001) and Pettigrew, Woodman and Cameron (2001) 'after modernism' there is also a need to make some big commitments to novel research themes energized by greater openness of research epistemology and practice. Some of these new possibilities will be signalled later in this short essay.

After the introduction, this essay has three parts. First, I pick up the crucial theme of the engagement between the social sciences and management research. An open and reciprocal relationship between the social sciences and the various management fields is an essential life-force for management research and certainly not 'a retreat to academic fundamentalism' (Burgoyne, 1993; Tranfield and Starkey, 1998). In this section, I raise the prospect of a new kind of doctorate in the management fields which finds its core purpose in engagements between the social sciences and management, and begins to prepare younger scholars for the double hurdle of scholarly quality and relevance. In part three of the paper I move on to consider the other crucial engagement for management researchers – that between themselves, users and other critical stakeholders in the research process. Here I draw upon the wider social science literature on interactive social science as well as contemporary examples of engagements between management researchers and users on both sides of the Atlantic. The conclusion summarizes the main themes in the paper and articulates the importance of epistemological, spatial and temporal openness if we are to have a chance of meeting some of the challenges of the double hurdles of management research.

Management research and the social sciences

The primary double hurdle for management research of scholarly quality and relevance has built into it a tripartite challenge. How to build, maintain and recreate scholarly quality; how to construct and exploit stakeholder links which can open the way to a relevant management research; and how to build the intellectual, social and political platform to conduct research which is simultaneously of high scholarly quality and relevant. The first of these challenges, building and recreating scholarly quality can be interpreted in many ways. Here I see it as a dual task and opportunity: how to build various fields of management studies which develop their own identities and specialist bodies of knowledge, while drawing on and contributing to social science disciplines; and how to re-engage European management research with the bigger academic market place in the USA and beyond.

Management is not a discipline, but represents a confluence of different fields of inquiry. The field is certainly multidisciplinary, with many of its early practitioners receiving their training in social anthropology, sociology, psychology, economics, mathematics and engineering. This early disciplinary diversity has now been overlaid by the development of a string of sub-fields. The original disciplinary scholars have assumed other titles and identities as interdisciplinary teaching and research themes (for example, organizational behaviour and strategy) and functional themes (such as finance and marketing) gained prominence. Further fragmentation has occurred as sub-fields such as international business, operations management and public-sector management have appeared as mobilizing research and teaching themes. There has also been pressure for management to become a practically-oriented social science. An already fragmented field has been asked to meet the dual demands of theory and practice (Pettigrew, 1997a).

This growing intellectual diversity has accelerated as the contributors to management knowledge have spread way beyond the university sector to include consulting firms, training agencies and contract research institutions. Further varieties of knowledge and knowledge production processes have arisen from the changing economic and political context of business post-1970, and also from the different intellectual and social organization of the social sciences and management in different nation states and continents. The depth and extent of this knowledge differentiation has led Whitley (2000) to argue that there is little likelihood of a collapse down to any singular mode of knowledge production – whether Mode 2 or any other. Fragmented adhocracies (argues Whitley, (2000)) are the norm and not the exception for most scientific fields in 2000.

It is hard to disagree with Whitley's interpretation of where we are in 2001. However, the key issue for the management research community is how we develop from where we are located now. The developmental pathway of management research sketched by Pettigrew (1997a) and Whitley (2000) has brought many positive attributes. In an activity charged with creative aims, intellectual diversity of fields and pluralism of theoretical development and method must be a plus. Compared with the 1960s and 1970s, when many of the current senior professors in UK business schools were trained, the different sub-fields of management now have more diverse but clearer and deeper intellectual traditions and identities. Since the 1980s there are also clear signs of a more capable and more confident intellectual tradition in management research in Europe than had existed before.

However, doubts remain about the quality and quantity of management research produced in the UK (Economic and Social Research Council (ESRC), 1994). But why? There are many candidate explanations. Here I will address two. First, a downside of the rise of the various sub-fields of management and their associated doctoral programmes has been a tendency for those sub-fields to (more or less) disengage from related theoretical and empirical developments in social science disciplines such as economics, sociology, psychology, anthropology and political science. Where engagement has occurred between management and the social sciences there is a widespread view that management has taken more from the social sciences and has given very little back. The one strong exception to this trend of disengagement has been in finance. Here the case can be made of a strong and continuing reciprocal relationship between finance, economics and applied mathematics, with clear evidence of intellectual benefits to all three parties. It is indisputable that scholarly finance could not have made the theoretical and policy impact it has without the intellectual links into economics and mathematics. I am much less sure that the two fields closest to my current interests, strategy and organization studies, have had as strong or as reciprocal a relationship with their candidate social science disciplines. It is certainly quite rare in the UK now for strategy and organization scholars to publish in top unidisciplinary journals in sociology, psychology, political science or economics, or to be seen to be contributing directly to the theoretical and empirical literatures in those disciplines.

So what can we consider doing to re-energize links between management researchers and the social sciences? Since early experiences are fateful and founding we should consider experimenting with a new form of doctoral programmes in the UK which is more explicitly grounded in management and the social sciences. This is clearly one important target to develop capacity for newcomers in the field of management and management research. There are clearly many design options in developing such a new doctoral programme. Alongside the customary courses on research design and methods and the various specialist fields of strategy, marketing and organization studies, might be located thematic courses chosen for their connectedness with management and the social sciences. Candidate themes that have strong literatures in management and various social sciences disciplines include:

- choice and change;
- uncertainty and risk;
- innovation;
- globalization;
- corporate governance;
- business and society.

These themes could expose multiple theoretical, methodological and empirical literatures from the management and social sciences, thus widening any student's intellectual awareness, critical faculties and ultimately choice of perspective and approach. The above themes would also complement a theory-driven approach to research design with a thematic form of scholarship that would encourage the development of skills to meet the double hurdle of quality and relevance. In such a doctoral programme it is not beyond the bounds of possibility to envisage the thematic teaching and the supervision of the eventual doctoral theses being joint tasks involving business school academics and faculty from other social science departments such as economics, sociology, psychology and political science. This might also be a valuable developmental opportunity for more experienced researchers from all the communities concerned, thus enabling a more open and reciprocal relationship between management and the social sciences.

This new form of doctoral programme in management and the social sciences would also help to solve some of the supply-side problems we face, as well as the capacity development problems we have. The social science and management ethos of the programme would be an attractive proposition for young scholars coming out of disciplinary social-science departments. The thematic teaching elements would be equally attractive to the group of potential doctoral students in management who wish to return to academia in their thirties after a period in industry, commerce, consulting and the public sector. In the USA that group of post-experience thirty-year-olds are now some of the most creative and committed doctoral students.

Thus far I have been arguing that some of the quality issues now facing management research in the UK and beyond might be tackled through a stronger and more reciprocal relationship between management and the social sciences. The second form of disengagement I think we now need to think about and act upon is an unfortunate side-effect of the attention we have given from the 1970s to the 1990s to building a strong European management research capability. After a necessary period of inner-directness to build the more creative and confident European management research community we now have, it is perhaps time to raise our heads (and our game) by re-engaging more deeply with our colleagues in the USA.

Like it or not, accept it or not, but the great bulk of management research production and dissemination is occurring in the USA. As we all know this issue is not just one of scale, but also reputation. Most of the top management and social sciences journals are published in the USA and have editors and editorial boards populated mostly by North America-based scholars. I see many dysfunctional reactions in Europe to the immense power and influence of the management and social sciences in the USA. One is fear and trepidation, 'we cannot compete in that world and with those people – so let's withdraw'. A second is a crude form of negative stereotyping, 'this is trivial number-crunching that few are interested in and even fewer read. We can do better than this and we should nourish our journals and forget theirs'. Sometimes, but thankfully not often, one finds this kind of negative stereotyping overlaid by ideologically consumed zealotry. Carefully-scripted arguments in terms of difference are underpinned by a misplaced and often unverifiable sense of superiority. I worry little about these kinds of dysfunctionalities amongst the few senior professors who reveal them. But it is a different matter if the younger generation of scholars are listening. The big issue and the big risk here is that disengagement from the US management and social sciences means potentially a loss of influence and impact of European research in the biggest market place for management knowledge. Frankly, opting out of publishing in the top US journals means relegating oneself to competing in division two. Surely a sounder strategy for a younger European-based scholar would be to build a portfolio of publications. No one, not even the very best US scholars, can publish in the 'A'-rated US journals all the time. There are many forms of quality and of relevance amongst the management research communities. In terms of breadth of influence and impact, a portfolio approach involving 'A' and 'B' journals across both sides of the Atlantic (and where possible beyond), plus research monographs and chapters and practitioner articles, seems a pragmatic and attainable publishing strategy for many.

The problem issue is the lack of close awareness and network connectedness of younger management scholars in the UK with what is happening in key academic centres, conferences and networks in the USA. This is in marked contrast to the opportunities afforded to the European scholars who had no choice but to do their doctorate in management in the USA in the 1960s, or who achieved the same appreciation and network connectedness by teaching over there for one or two years after a European doctorate. If the resources for those opportunities to learn the art of research, teaching and publishing and network-building in the USA have dried up, perhaps we need to find other mechanisms to achieve the same benefits. Research in the natural sciences by Gambardella (1992), in medicine by Harvey, Pettigrew and Ferlie (forthcoming) and in the social sciences by Robson and Shove (1999) all point to the crucial enabling, energizing and knowledge-building possibilities of scientific networks. Network building is particularly valuable at the founding stages of an academic career, when the scholar does not have the public reputation to draw an audience to their work and themselves. One of the great successes of the European Institute for Advanced Studies in Management (EIASM) in Brussels has been the building and nourishment of pan-European networks. Given the natural self-absorption of the vast US academic communities, perhaps we now need an organized attempt to build network mechanisms into the USA for UK and other European scholars. The signs from the policy commitments and actions of the (US) Academy of Management are of a rekindled openness to European attempts to re-engage.

In this part of the essay I have drawn attention to two key issues which can raise the quality of existing management research and develop the capacity for future knowledge production. Action to widen and deepen engagement with the social sciences, together with strategies to complement gains in building a European management research tradition with a re-engagement with US social sciences and management, can raise the quality and profile of UK management research. Before I move on to consider the parallel need for further experimentation with 'interactive social science' and management research–user engagements, I wish briefly to mention the new opportunities for management research arising from knowledge production 'after modernism'. The discussions of the changing context and character of knowledge and knowledge production in Gibbons *et al.* (1994) have stimulated considerable debate in many fields of research. The fact that these authors' statement of Mode 2 knowledge is being used vigorously, but perhaps clumsily, to legitimate localized drives for more applied science has clearly not always pleased the authors, as their later book testifies. (Nowotny *et al*., 2001). Both of these books can be interpreted as an extended critique of modernist science. In this sense they have many fellow travellers inside and outside the field of management studies (Clark, 2000; Toulmin, 1990; Whittington and Mayer, 2000). Nowotny *et al.* (2001) pile up arguments and evidence to undermine scientific claims for rationality, universalism and autonomy. Toulmin (1990), of course, had argued that the 1960s were the last years of modernism. Since the 1960s the modernist faith in progress based upon reason and the construction of knowledge expressed in universal laws has been challenged by the combined assaults of relativists, constructivists, postmodernists and mediativists. However, modernist forms of science variously labelled as 'normal science', 'positivism', or 'rigorous research' are still alive and well in most of the subfields of management research. However, the ever-more explicit and public attempts to move beyond modernism in management and the social sciences are creating a new setting and opportunities for experimentation and engagement between management researchers, social scientists and user communities.

After modernism, and reflecting the more complex, dynamic and internationally conscious world we live in, there is a recognition that any search for general patterns should give much greater significance to temporal and spatial context. Generalizations are hard to sustain over time, they are even tougher to uphold across organizational, international, institutional and cultural borders. The modernist tradition has been uncomfortable with dynamism. Social science has developed comfortably as an exercise in comparative statics. States are privileged over the complex processes that lead to them. After modernism the new excitement to explore time and dynamics will perhaps help to overturn many management researchers' preoccupations with 'what is' knowledge and foster a climate where 'how to' knowledge is given a higher priority (Pettigrew *et al.*, 2001). Management research which delivers both what-is and how-to knowledge is much more likely to meet the challenge of scholarly quality and relevance. These kinds of knowledge will also provide a firmer and more forceful bridge to the potential users and funders of future generations of management research.

If one scholarly route to user engagement lies in new possibilities to catch reality in flight, a second lies in elevating embeddedness to a principal of method (Pettigrew, 1997b) The more explicit attempt to contextualize knowledge is now regarded by Nowotny *et al.* (2001) and others as a generic ambition of scientific endeavour. This is a particularly beneficial trend and condition for management researchers interested in demonstrating how variations in organizational, institutional and national context can shape patterns of managerial behaviour and firm outcomes. Modernist tendencies to prize the universal over the particular may have been an attractive prospect in reinvigorating notions of scientific progress, but this universalistic value has not delivered in the social sciences and management. The new opportunities for a contextualist and dynamic social science will offer management researchers a further attractive bridge to user communities who are often interested in how and why general patterns are variably expressed in different organizational and national settings.

One of the fondest dichotomies in modernist conceptions of science has been that of theory and practice. Recently, in examining the future of strategy research in management, Whittington, Pettigrew and Thomas (2001) have asked us to regard theory and practice as a more tightly-linked duality. They argue that this 'greater sensitivity towards practical complexity will prompt a more comprehensive notion of rigour' (Whittington *et al.*, 2001, p. 486). The aspiration here is clear. There is to be no softening of academic standards. The practical working out of the new duality of theory and practice will considerably raise the stakes in terms of the social production of knowledge.

The action steps to resolve the old dichotomy of theory and practice were often portrayed with the minimalist request for management researchers to engage with practitioners through more accessible dissemination. But dissemination is too late if the wrong questions have been asked. A wider and deeper form and range of engagement between management researchers and practitioners would entail experimentation with the co-funding, co-production and co-dissemination of knowledge. Here also, after modernism, there is no simple recipe, or easy set of choices, just some hard challenges to our scholarly routines.

Management research and stakeholder engagements

The thesis of a change in the character of knowledge and a new process for producing knowledge rests on a broad-ranging theoretical and empirical argument. Nowotny *et al.* (2001) characterize this as a co-evolutionary process between science and society. The elements of the change are many, but the most often debated features include:

- a more porous boundary between science and society;
- a resultant loss of researcher autonomy;
- a breakdown of assumptions about unitary views of science and linear notions of the scientific process;
- a greater range of participants in the knowledge-development process and greater pluralism of research practice;
- a greater recognition of the localized (in time and space) character of research practice and outcomes;
- a wider recognition of the emergent rather than planned views of the research process;
- a recognition of the complex interactions between multiple stakeholders in the research process and a more contested landscape for evaluating the quality and relevance of research processes, outputs and outcomes.

There are many difficulties in making sense of such a complex multi-dimensional process. One undoubtedly is trying to find an appropriate analytical vocabulary to make sense of a process which is still incomplete and emerging. This is a further problem with the Mode 1 and Mode 2 ideal types. Useful as they are as attention directors, ideal types are peculiarly blunt instruments to make sense of moving targets. We have seen this problem in many areas of management studies. A recent example is the easy and imprecise use of ideal types such as the 'N' form, cellular form and the virtual organization to study the emergence of new forms of organizations. (For the debate in this area see Fenton and Pettigrew, 2000.) Interactive Social Science (ISS) is another label which is a helpful rallying cry to collect together debate and experience (Caswill and Shove, 2000). Scott and colleagues have defined ISS as: 'a style of activity when researchers, funding agencies and "user groups" interact throughout the entire research process, including the definition of the research agenda, project selection, project execution and the application of research insights' (Scott *et al.*, 1999). Problems of labelling are in turn compounded by varying assumptions about precisely who are the range of stakeholders in the research process and what kind of roles each are capable of performing. The varieties of stakeholder mentioned in the literature include users, consumers, funders, co-producers, subjects, gatekeepers and translators.

Users as a category remain obstinately difficult to classify. Thus in the set of papers on ISS recently edited by Caswill and Shove (2000), Shove and Rip (2000) distinguish between generic, influential and intermediate users. In a related but earlier publication, Robson and Shove (1999) reflect on the issues in trying to engage with organized and diverse users, and in a not entirely tongue-in-cheek discussion, Wensley and Caswill (2000) reflect on the invisible user. In a very interesting paper on the process of use, Shove and Rip (2000) note how difficult it can be to identify users, and how users may retreat from that label and claim they are really only translators or mediators. They also point to the dangers of collapsing ISS just down to social science–user engagements and thereby ignoring wider streams of social science interaction, influence and impact. Networks are clearly an essential part of the interactive field, which build academic reputations, form identities, provide avenues to attract resources and offer the kinds of relationships where relevance and impact may be negotiated. Woolgar (2000) picks up the related issue of identifying user needs in research. He notes that user needs are rarely so self-evident they can be observed and acted upon; 'instead we should accept that user needs rarely pre-exist the efforts and activities of producers to engage with them' (Woolgar, 2000, p. 169).

These observations make clear that all the familiar categories of user, user needs and user engagements are both hard to define generically and contestable. The same applies to that equally debatable and variably interpretable phrase 'relevance'. Drawing on actual accounts of experienced researchers, Robson and Shove (1999) note that user interest in research is fickle and highly time and context-dependent; often rests on quality informal interaction established between researchers and policy makers over long periods of time; and is dependent on the institutional position of user and researcher. User reactions to the relevance of any particular project may depend not just on their interpretations of the research output of that project and the rise and fall of issues in their own policy context, but in the credibility of the researcher over a long period of time. But for many users, issues have a short attention span, while the researcher may pursue themes for several years. The availability of dense networks between researcher and potential users is a critical factor in allowing relevant interpretations of research ideas and findings to emerge. The maintenance of these networks through regular contact, the offering of advice from the stockpile of researcher experience and the use of a variety of mechanisms to maintain and deepen dialogue, all increase the potential for researcher influence and impact. For the experienced or inexperienced researcher the message here is absolutely clear. The creation and exploitation of networks is a strategic challenge that demands conscious effort and the deliberate investment of time and energy (Robson and Shove, 1999, p. 29).

These observations about social science and management researchers in action make clear the dangers of us basing our future aspirations upon broad and abstract categories such as Mode 1 and Mode 2 research, needs for scholarship and relevance, interactive social science and the virtues of user engagement. Such distinctions are only meaningful in practice when they have to be mobilized with a variety of different stakeholders, often with different users with varying needs and interests, drawing upon varying interpretations of relevance and operating in different institutional settings. Perhaps it is now time to move on from the very useful categories of Mode 1 and Mode 2 knowledge production and to examine any relevance gap in management research where it really matters, in the highly local situations where management researchers and stakeholders actually meet.

Whether guided by a problem-focused agenda, or by a more positive aspiration to conduct research which meets the double hurdle of scholarly quality and relevance, we certainly need to be encouraging an innovative era of exploration and exploitation in management research. In this process of knowledge production after modernism, intellectual diversity and not intellectual closure is the more attractive and possibly more effective goal. We need also to reflect on these new research practices at a local level. Alongside this era of adaptation and learning needs to come a reconsideration of the skills, knowledge, relationships and standards now necessary to conduct a more broadly influential management research. This also means greater reflexivity in exploring accounts of our own research practice (MacLaren, Macintosh and Tranfield, 2001; Pettigrew, 2001; Pettigrew and Whittington, 2000; Twomey, Yorks and Harman, 2001), but above all in making big commitments to deliver our aspirations about new forms of management research and not just articulating views about those aspirations

Conclusion

The duty of the intellectual in society is to make a difference. After modernism there is a more receptive context to achieve such an aspirational goal. But can a wider set of us in the management research community rise to the challenge? Not without a concerted attempt to raise the capacity and openness of our community and to deliver on the resources which may yet come our way.

We can no longer rely on the absence of a ready-made peer group within business and the public sector which can readily relate to the social sciences and management. No single action by the management research community or its supporters will make a difference. Our future lies in our own hands and that future requires action in a series of linked complementary steps. The need is for holistic thinking and action. Some of these actions have been sketched in this essay while many others have been articulated by Starkey and Madan (2001) and other commentators in this special issue of the *British Journal of Management*.

Enhancing scholarly quality remains essential but any further retreat to defining scholarship just in terms of publication in 'A'-rated scholary journals will trap us even further in the social echo chamber of our voice. After modernism, there are new possibilities to widen and deepen our concept and practice of scholarship. A more contextualist and dynamic view of knowing will open doors to all the other potential stakeholders of management research. To do this we will need to re-engage and deepen our links with the social sciences and users. We should also recognize that the duality of scholarly quality and relevance is attainable, but only if we make big emotional and practical commitments to pursue big themes in the social present that are enlightened by ideas and theories perhaps in tension with that social present. Above all we must be prepared for a period of experimentation and learning with all the potential partners out there waiting to engage with us.

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