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# Not Simply Returning to the Same Answer Over and Over Again: Reframing Relevance

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**From its earliest days, the field of business and management studies has wrestled with fundamental questions concerning its nature and purpose: for whom and to what ends is scholarly research ultimately directed? However, amid unprecedented changes to the world of work, over the past two and a half decades these questions have become of central importance to academicians, practitioners and policy-makers. The British Academy of Management (BAM), through the work of its Research Policy Committee and the *British Journal of Management*, has played a central role in these developments. This paper traces the lineage of BAM's contribution and offers a critical assessment of the current state of play with regard to the so-called relevance problem, arguing that design science and critical realism have the potential to take the field forward by transcending the 'either/or' game into which the rigour versus relevance debate has a tendency to develop.**

> The fact is, Phaedrus, that writing involves a similar disadvantage to painting. The productions of paintings look like living beings, but if you ask them a question they maintain a solemn silence. The same holds true for written words; you might suppose that they understand what they are saying, but if you ask them what they mean by anything they simply return the same answer over and over again. (Plato, *Phaedrus*)

In this paper we present a critical overview of the major developments that have occurred with regard to the question of relevance in business and management studies (BMS) and related fields of scholarly research over the past two decades. In particular, we trace the evolution of debates stimulated initially through the deliberations of the Research Policy Committee of the British Academy of Management (BAM), which culminated in publication of the paper by Tranfield and Starkey (1998) entitled 'The nature, social organization and promotion of management research: towards policy'. In reviewing the considerable volume of literature that has amassed on both sides of the Atlantic in part at least as a result of the various conversations stimulated by the publication of this work, which appeared in the *British Journal of Management* (BJM), our goal is to enhance the prospects for the emergence of a trans-disciplinary field of inquiry that can authentically meet the twin imperatives of scholarly rigour and social usefulness, at a time when many management, economic and social 'truths' are ripe for rethinking. We believe that business and management research is well placed to make an important contribution to ongoing debates pertaining to the fundamental nature and purpose of the social sciences in academia and wider practitioner and policy-making circles.

The business and management field is not so much a discipline as a confluence of disciplines, uniquely situated at the nexus of practice and contributing (social science) disciplines (Pettigrew, 2001). A number of commentators, dating back to Herbert Simon's treatise, *The Sciences of* *the Artificial* (Simon, 1969), have suggested that the field is akin to engineering (in the physical sciences) and medicine (in the biological sciences). Following this line of thinking we suggest that one way of capturing the essence of the management research problem of relevance and the dilemmas it poses for the scholarly BMS research community is by arguing that its central concern should be the general (engineering) problem of design – how to create organizations and systems of management and economy that are a better fit for purpose than those we have currently. We also suggest that critical realism has the potential to move the debate about rigour and relevance forward. We maintain that management research should aspire to be trans-disciplinary and problem-led. We also agree that closer involvement with non-academics in identifying research problems, methods and solutions can often both enrich the quality of academic research and yield outputs of greater usefulness to policy and practice (cf. Trist and Bamforth, 1951).

## The 1990s

As noted above, our point of departure is Tranfield and Starkey's (1998) BJM paper, which argued that the management research field was entering a new era in which the identity and offerings of the academic management research community faced unprecedented threats, for example from substitute products and new entrants, each competing for scarce resources and challenging academic legitimacy in defining the field. Tranfield and Starkey (1998, pp. 351–352) used the vocabulary of Mode 1 and Mode 2 research, drawn from science policy debates (e.g. Gibbons *et al.*, 1994), to frame their argument, which in essence maintained that management research should adopt a Mode 2 approach, as a better fit to the cognitive and social organization dimensions of the field than the purist Mode 1 approach, arguing that

> [t]he mode 2 knowledge-production system offers a different and potentially more appropriate (useful/relevant) model of the link between theory and practice. Here, research problems are framed in the context of application and research activity is driven by trans-disciplinary concerns at the levels of both theory and practice. In effect mode 2 collapses some of the binary divisions that bedevil the mode 1 orientation. Knowledge production and diffusion are interlinked rather than sequentially disaggregated. Crucially, it becomes more difficult to divide theory and practice. Mode 2 enables contribution to both simultaneously.

Mode 2 as the preferred approach to knowledge production in BMS was framed as a dual approach, combining both the theory-sensitive and the practice-led and thereby countering the respective dangers of epistemic drift, dominated by concerns with policy funding, and academic fundamentalism, defined only in terms of academics' own agendas.

The science policy origins of Mode 2 are worth re-emphasizing. Commentators in this domain had traced a major shift in policy debates in certain of the sciences, for example in environmental science, with these fields becoming more complex and the legitimacy of scientists to define agendas increasingly challenged by a growing range of stakeholders. Knowledge itself was also evolving at a fast pace and in ways that undermined the strength of disciplinary boundaries. For example, Galison's (1997) study of microphysics demonstrated how that field had evolved as a changing disciplinary space in which, for example, specialists in theorizing, experimentation and instrument building worked out new ways of translating ideas into theory testing and practice by capitalizing upon the new knowledge created in the different contributory disciplines. In a seminal work, Gibbons *et al.* (1994) demonstrated the importance of trans-disciplinarity and co-production of knowledge in the evolution of scientific fields more generally.

Of course, debates on relevance in management and organization studies (and indeed the wider social sciences) pre-date the Tranfield and Starkey (1998) publication. In the USA, in particular, leading management researchers including several eminent Presidents of the American Academy of Management had addressed similar issues. Their usual starting point was not the nature of knowledge in management research *per se* but the lack of impact. Hambrick's 1993 Academy of Management Presidential Address (Hambrick, 1994) was particularly influential and, indeed, continues to pose key questions for management scholars. He, somewhat plaintively, posed the question 'What if the Academy actually mattered?' and 'mattering' has been a constant theme ever since, usually identified in its breach and its absence rather than its observance. Porter and McKibbin's (1988) *Management Education and Development: Drift or Thrust into the 21st Century?* had raised similar questions and identified a number of problems for the status quo, not least the growing credibility problem among corporate recruiters of business school students. Wren, Buckley and Michaelsen (1994) critically questioned the 'theory/applications balance' in management pedagogy.

These three contributions, along with others, shared the views that the management field had overcompensated in its attempts to infuse the field with the scientific model and/or that the practice/practical needs of students were inadequately met in the classroom. These views received strong endorsement in Khurana's (2007) recent study of American business schools and what he perceives as their loss of direction due to a growing emphasis upon science spearheaded by an infatuation with economics as a model discipline. Indeed, Pfeffer (1993) had seemed to suggest, albeit somewhat ambivalently, that the only way management researchers would have more impact would be if they started to organize more like economists rather than as a loose grouping of warring tribes, united only in their sense of embattlement in relation to those who were hostile to the management discipline as a matter of principle. One needs only to recall the opposition to the founding of a business school at Oxford University (*The Observer*, 25 July 1999), mirroring similar debates in the USA a century earlier, to remind us that BMS has struggled continuously to establish its legitimacy as a distinct field of study.

A number of commentators in the UK addressed similar issues to their US counterparts. For example, in a series of papers that appeared in the *Journal of Management Studies*, Whitley (1984a, 1984b) and Tsoukas (1994) endorsed the view that management studies is a heterogeneous and fragmented field. The Economic and Social Research Council (ESRC) Commission on Management Research (chaired by Professor Sir George Bain in 1993) highlighted the relevance issue (ESRC, 1994). Subsequently, Pettigrew (1995, 1997) made the seminal argument that good management research needed to fulfil the 'double hurdles' of rigour and relevance. These debates exercised BAM for several years, through the deliberations of the Research Policy Committee of its Council under the chairmanship of Gerry Johnson followed by Ken Starkey, with significant contributions in particular from Elizabeth Chell, Paul Jeffcut, David Sims and the late Richard Whipp.

It is probably fair to say that one outcome of these deliberations was that BAM endorsed the double hurdles philosophy of management research as expounded by Pettigrew. It is less clear that this became an actual policy impacting on the day-to-day activity of UK management research. How such a philosophy is to be operationalized remains a live issue, particularly in the transition from the UK Research Assessment Exercise (RAE) – historically one the UK government's principal funding mechanisms for allocating scarce resources to scholarly research in higher education institutions across all subjects – to the new Research Excellence Framework (REF), currently scheduled to take place in 2014. This new funding mechanism will place a much stronger emphasis on the assessment of the wider social and economic benefits of publicly funded research ('impact'), beyond the academic benefits *per se*. That this is still a vexed topic is perhaps hardly surprising, given that one of Tranfield and Starkey's (1998) central observations was that business and management research is a 'soft' (low consensus), applied, divergent and rural (as opposed hard, pure, convergent and urban) field.

## The debate continues: from 2000 to the present

The strong Mode 2 thesis argued that the conventional (Mode 1) approach to business and management research was becoming outmoded. The Mode 1 approach was single discipline, or, very occasionally, interdisciplinary (rather than trans-disciplinary). Academics defined both the research problems to be investigated and the methods of study. If implications for practice emerged this was rare and typically followed publication, often many years later. This trend, in part, reflects the 'rural' separation of much management research from the sites of management practice, performed on 'campus', a word derived from the Latin for 'field'!

The arguments about Mode 1 and Mode 2 have continued to resonate up to the present moment. Across the Atlantic, Huff highlighted this in her 1999 Academy of Management presidential address and subsequent *Academy of Management Review* paper (Huff, 2000). The Foundation for Management Education, a key body in the establishment of business schools in the UK, co-funded, with the British Academy of Management, a fellowship to research the usefulness of management research and the gap between research and practice. This culminated in a report by Starkey and Madan (2000), which elaborated the case for and the context of a need for change, developed the critique of the North American model of research, and presented an even stronger advocacy of Mode 2 and related approaches, together with case examples.

The Starkey and Madan report was taken up in the deliberations of the Council for Excellence in Management and Leadership, commissioned by the UK government's Department for Trade and Industry (DTI), particularly in its discussion concerning a possible separate UK research council for the funding of management research. This potential development was motivated, in part, by a perceived imbalance in the funding of management research in comparison with other social sciences at a time when business and management had become one of the most popular choices for both undergraduate and postgraduate education, its recruitment far outnumbering the other social science disciplines.

At a conference sponsored by the DTI (now known as Business, Innovation and Skills) in late 2000, Starkey spoke in favour of the idea of a separate research council for the funding of BMS research, with the then chair of the ESRC, not surprisingly, speaking against the motion. Although this event did not lead to a separate council, one outcome of the discussion was a major ESRC investment, with additional funding provided by the Engineering and Physical Sciences Research Council (EPSRC), in an initiative dedicated to management research: the Advanced Institute of Management (AIM) Research. Currently in its final phase, the AIM Research initiative was directed initially by Anne Huff and later by Robin Wensley. There was also significant investment in management research through the ESRC-funded Evolution of Business Knowledge programme, directed by Harry Scarbrough (2003–2006).

One of the major contributions to the ongoing debate was the special issue of the BJM edited by Hodgkinson (2001). This contained an abridged version of the Starkey and Madan report, together with seven rich peer commentaries from a variety of perspectives, by Grey (2001), Hatchuel (2001), Hodgkinson, Herriot and Anderson (2001), Huff and Huff (2001), Kilduff and Keleman (2001), Pettigrew (2001) and Weick (2001). There was also a plethora of point–counterpoint debates, both here in the UK and in wider continental Europe and the USA, for instance in the *Academy of Management Journal* (e.g. Bartunek, 2007; Cohen, 2007; Latham, 2007; Rynes, Giluk and Brown, 2007), in the *Journal of Occupational and Organizational Psychology* (e.g. Anderson, 2007; Gelade, 2006; Hodgkinson, 2006; Symon, 2006; Wall, 2006) and in the *Journal of Management Studies* (Fincham and Clark, 2009; Hodgkinson and Rousseau, 2009; Kieser and Leiner, 2009, 2011; Starkey, Hatchuel and Tempest, 2009). In addition, various attempts were made to enact and refine Mode-2-related approaches (e.g. Burgoyne and James, 2006; MacLean, MacIntosh and Grant, 2002).

At a conceptual level there were a number of alternative conceptions directed to the bridging of the academic–practice/policy gaps (e.g. Anderson, Herriot and Hodgkinson, 2001; Briner and Rousseau, 2011; Briner, Denyer and Rousseau, 2009; Huff, 2000; Huff and Huff, 2001; Rousseau, 2006; Van de Ven, 2007; Van de Ven and Johnson, 2006). In the remainder of this section we selectively highlight a number of themes emerging from this body of work and related publications that deserve further elaboration and which might provide an advance in framing future discussion.

A recent addition to this debate, which we consider particularly promising, is the growing discussion about management as a design science (see, for example, van Aken, 2004, 2005; Denyer, Tranfield and van Aken, 2008; Dunbar and Starbuck, 2006; Hatchuel, Starkey and Tempest, 2010; Hodgkinson and Healey, 2008; Romme, 2003). In general, a design science approach requires better translational skills on the part of researchers and a rich ecology of interactions between knowledge generators, knowledge intermediaries and knowledge end-users (Keleman and Bansal, 2002), in an attempt to generate design artifacts that communicate meaning and co-production across diverse stakeholder groups. A key driver of this discussion is the legacy of Simon (1969). Recently, however, Panza and Thorpe (2010) have argued that because BMS researchers have only engaged with Simon's intellectual legacy on a superficial basis, they have failed to consider alternative conceptions of design and what these might imply for addressing the enduring relevance debate in BMS. In particular, they maintain that BMS scholars have treated Simon's differentiation of explanatory-based and prescriptive-based social sciences in arbitrary and inconsistent ways, each of which implies a rather different pathway for the emergence of design artifacts and a rather different role that such artifacts might play. This seems to us an important argument that takes the design debate forward.

According to Panza and Thorpe, deterministic design conceptions, akin to professional engineering, which seek to create technical and social artifacts on the basis of purposeful design decisions, are only applicable to a narrow range of problems, ones in which pre-existing knowledge is of a form that can be codified readily into prescriptions. Hodgkinson and Healey's (2008) narrative review of the personality and social psychology literature to distil design principles to help guide the process of scenario planning exemplifies this approach to design science, as does Denyer, Tranfield and van Aken's (2008) use of systematic review to distil design propositions to assist in the design of high reliability organizations. As observed by Panza and Thorpe (2010), in order to implement such prescriptions effectively it is crucial that a central designer or group of designers is able to effectively control the design process and/or implementation of the design principle(s) in the particular context(s) of application. Furthermore, there must be basic agreement as to what constitutes the appropriate outcome(s).

The evidence-based approach possesses elements of the deterministic design philosophy. There have been a number of studies arguing for evidence-based management in BMS founded on systematic review (e.g. Briner and Rousseau, 2011; Briner *et al.*, 2009; Rousseau, Manning and Denyer, 2008), as there have been in other disciplines such as medicine. This argument is likely to become more central as public funding bodies are tasked to justify their investments and management research wrestles with the challenge of impact. Although we are sympathetic to the agenda of evidence-based management, echoing the observations of Panza and Thorpe (2010) in relation to design science we caution that researchers need to be mindful that not all management design problems and policy questions are of a form suitable for systematic review. There are many situations in which conventional literature reviews are more appropriate, enabling more creative insights and solutions to emerge, especially in cases where an insufficient body of evidence has accumulated that specifically addresses the problem at hand. Furthermore, even when a considerable volume of evidence has accumulated of a form that is directly amenable to research synthesis via systematic review, there can be no guarantees that impactful solutions will be readily implemented, as demonstrated time and again by the widespread disproportionate use of poor personnel selection and assessment practices *vis-à-vis* the more effective, evidence-based alternatives, a finding that generalizes across many organizational contexts (e.g. Robertson and Makin, 1986; Shackleton and Newell, 1994; Zibarras and Woods, 2010). In the final analysis:

> . . . the need to integrate more effectively the insights of the scientific literature with the complex realities of organizational decision processes and gain a more detailed and systematic understanding of why the demand for non-evidence-based practices and solutions so often outstrips the demand for evidence-based ones are problems that far outweigh the need to further refine techniques for research syntheses. (Hodgkinson, 2011, p. 52)

Achieving the best balance is clearly an ongoing design challenge for BMS researchers.

Panza and Thorpe (2010) suggest two alternative conceptions of design which offer promising alternative ways of developing the design debate in BMS. The first of these is path-dependent design, which is akin to evolutionary design in engineering, whereby design artifacts develop progressively as a function of trial and error learning. The prescriptive power of this perspective lies not so much in the distillation of design principles informing interventions as in identifying limitations for design decisions and processes for coping with uncertainty, exemplified by the routines and capabilities literature (e.g. Nelson and Winter, 2002), which affords a limited role to managerial agency, designers being constrained by their own blind spots, and thus a prime source of cognitive inertia (Tripsas and Gavetti, 2000). The other perspective identified by Panza and Thorpe, path-creation design, is akin to radical design in engineering, and seeks to understand the emergence of novelty and in so doing identify processes through which organizations galvanize evolutionary forces to amend path-dependent trajectories in the service of design. The notion of path-creation design thus accords a greater role to human agency in the purposeful transformation of enterprises than that implied by the evolutionary perspective. This third perspective is exemplified by the literature on the development of dynamic capabilities (Hodgkinson and Healey, 2011; Teece, Pisano and Shuen, 1997). Each of these design perspectives fits well with the notion of bridging emphasized by Aram and Salipante (2003) who argue that knowledge development at the leading edge of science cuts across different disciplines. The design approach thus seems a particularly attractive and potentially relevant approach to BMS, which as observed earlier is a synthetic rather than a pure discipline.

## Where are we now – Mode 1 or Mode 2?

Perhaps we were too infatuated with the idea of Mode 2 and allowed the argument to run too far in this direction. This was understandable, given the direction of travel of the field in the Mode 1 direction, a trajectory exacerbated by the growing stranglehold American journals were coming to have in the management field. This is still an issue. UK research is increasingly subjected to a plethora of league tables, each with their own modes of evaluation and assessment. In this process, leading US journals have come to be adopted as the touchstone of quality. Non-American management academics have come to see the US Academy of Management as the world's leading management conference. This is not just a UK or a European issue as one sees growing evidence of this trend worldwide. Yet, ironically, top American scholars continue to lament the disconnect between management research and management practice and US journals have been criticized for failing to make significant contributions to the development of both theory and practice (e.g. Hambrick, 1994; Huff, 2000; Rousseau, 2006; Van Maanen, 1995). There are system-wide issues here concerning factors such as editorial policy, accreditation pressures, tenure decisions and career incentives, which are beyond the scope of this paper.

The strong anti-Mode 1 argument was overstated. Although there have been interesting localized Mode 2 developments, Mode 1 has not been universally outmoded. The single discipline approach is still important in trying to frame research in what remains a relatively young research field, seeking to transcend its disciplinary origins in fields such as economics, psychology and sociology. Mode 1 academics do often identify socially useful research problems and methods, but need help from practitioners to identify better problems and gather and make sense of better data. Although not always the case, potential implications for practice do emerge directly from the research process but, again, strengthening links with practitioners at the problem formulation stage will help close the implementation gap.

Meeting the double hurdles challenge set out by Pettigrew (1995, 1997), and further elaborated in Pettigrew (2001), is of critical importance and there is a long and successful history of academically rigorous and socially useful research, including research that tackles issues of fundamental concern to managers and organizations. For instance, Hodgkinson and his colleagues (e.g. Hodgkinson and Healey, 2008, 2011) have demonstrated how research at the interfaces of personality and social psychology and the cognitive and management and organization sciences can both contribute to the advancement of new theory and research and generate actionable prescriptions and tools for strategic management practice. This work illustrates why we need bridge building not only between academics and practitioners/policy-makers, but also between academics working within the different subfields of BMS and the wider social sciences. However, it is fair to conclude that the desire to compete academically in world-class terms has driven and continues to drive many capable scholars towards Mode 1 and away from concerns with relevance, and as a result too much management research exists in a vacuum of its own devising.

## Where do we go from here?

*Debates about impact*

In the USA there is continuing concern about these issues and leading scholars, and deans, bemoan the tangled webs that they have created in shaping the field, unintentionally, in ways that favour Mode 1 research of a particular kind (e.g. Walsh, 2011). The Association to Advance Collegiate Schools of Business (AACSB) recently conducted its own review of the impact of management research (AACSB, 2009). In its report, it took the criticism of lack of relevance and of 'mattering' very seriously and made several recommendations about how this state of affairs might be rectified, detailing how new and stronger connections between basic scholarship and the practice of management might be created. The report also recommended that researchers should be more vocal in identifying and promoting the management research that has had impact, citing, among others, the work of Black and Scholes on options pricing, March and Simon on decision-making, Christensen on managing technology, Nonaka and Senge on knowledge and learning, Herzberg on motivation, Prahalad on poverty and business and Porter on strategy.

But here we must sound a cautionary note. Black and Scholes's work has definitely had a major impact on financial economics and on the practices of Wall Street. Their own use of their own models led to one of the biggest corporate failures in Wall Street history with the demise of Long-Term Capital Management, a hedge fund they co-founded. This paled into insignificance compared with the banking crisis that began in 2007–2008, from which we are still struggling to recover. Again, it was the models and the products, such as credit derivatives derived from research led by Black and Scholes in financial economics, which provided the theoretical and ideological basis for the trading practices that culminated in the financial crisis. The traders were themselves MBAs from top US business schools who had migrated, as their career preference of choice, to Wall Street (Delves-Broughton, 2008). Be careful what you wish for! Even a committed devotee of this financial regime, ex-chief of the Federal Reserve Alan Greenspan, having previously been one of the staunchest defenders of a free market in financial services, had to conclude, in a state of shock, that all he had believed about the benign nature of markets and the power of financial models had been proved wrong by the crisis.

It is also rather ironic that Herzberg's work should have been highlighted by the AACSB as an example of high impact research; its popularity among practitioners notwithstanding, the two-factor theory has long been criticized as an example of poor science leading to flawed insights and prescriptions (House and Wigdor, 1967). Management research has a bad habit of identifying what it sees as best practice, erecting a management theory around it, only to find, all too soon, that the firms and managers associated with the latest management fad are not what they seemed. For instance, Harvard Business School 'best practice' case studies of Enron proliferated prior to its downfall, as did cases of Sir Fred Goodwin, former chief executive of the Royal Bank of Scotland, as a model of leadership. Indeed, Gary Hamel (2000), acclaimed by many as the world's leading strategy guru, built a theory of business 'revolution', innovation and transformation on an analysis of Enron.

There are fundamental questions here about what constitutes knowledge in management research, what 'fashion' is and what is 'fundamental' (Abrahamson, 1991; Weick, 2001). One way of framing this issue is in terms of debates about levels of analysis, using a critical realism perspective (Bhaskar, 1978, 1979) that distinguishes between the domains of the empirical and of the real. The empirical domain comprises that which is deemed to exist because it is observable, 'the positivist's view of the world: a space of observed events or experiences' (Wilson and Dixon, 2006, pp. 261–262). The real consists of the generative mechanisms, themselves a complex outcome of structure and agency, which produce events in the world: '[t]he real consists not of events but their causes: the generative mechanisms and structures, the potencies, so to say, of which events are but the effects' (Wilson and Dixon, 2006, p. 262).

Science goes awry when it assumes that the empirical is a straightforward mirror of the real. Contrary to a simple correspondence theory of observable phenomena and the deduction of causality, this is not necessarily the case. What Hamel saw in Enron might have had simple empirical validity in terms of his case methodology and his rather simplistic 'great leadership' view of superior performance. It did not, however, do justice to the real explanation of the Enron phenomenon, its short-lived superior performance and its spectacular fall. Even in economics, there is a growing group of dissident voices that challenge the orthodoxy of economic modelling on the grounds that it assumes too much, not least that the empirical is an adequate mirror image of the real, an assumption that is based on what theory assumes away in a closed systems model of the world (Lawson, 1997). For example, 'discussion of methodology traditionally is confined to arguments about which preconceptions and procedures should be adopted to the permanent exclusion of all other sets' (Fullbrook, 1998, p. 435). The critical realist view 'is that the world is knowable, but characterised by ontological uncertainty since knowledge of wholes cannot be simply built up from knowledge of parts and the history of the organic processes involved matters' (Pinkstone, 1999, p. 47).

Critical realism focuses upon trying to model and explain 'why what happens actually happens' (Danermark *et al.*, 2001, p. 52), which it does by challenging the assumption of a naïve empiricist positivism that what we observe is what is important and focusing instead on the generative mechanisms, which, by definition, are unobservable. Events occur when actors mobilize the resources they have in particular contexts to shape change, which, in social contexts, unfolds in open systems where generative mechanisms (social, cultural and biological) operate independently or in concert in complex interactions. Bhaskar (1979) argues that the hidden and complex nature of the interaction of generative mechanisms means that the social sciences can only operate by 'retroduction', the theoretical reconstruction of a plausible explanation of the conditions and mechanisms necessary for a particular turn of events to occur. Social science is at its best when it explains. We see this approach as building a capacity for prescience rather than prediction. In this sense, social science is as much pre-science as a science in the narrow positivist sense. The latter only applies in those clearly delimited areas where empiricism can legitimately deal with obviously observable phenomena, though the evolution of science, as for example in physics, teaches us that we can cling to what seems obvious at our peril.

### *Debates about the future of the business school*

Questions of relevance and impact have been an important aspect of recent debates about the evolution and the role of the business school and debates about the future of management research need to be placed in this broader context. A number of commentators on the business school are quite negative about its achievements, despite what some might see as its great success, most notably the exponential increase in students studying BMS in the last few decades. Pfeffer and Fong (2002) are two of the most trenchant critics, arguing that business schools have delivered far less than they have promised, in both teaching and research. One of the implications of their argument is that we should look to the basic disciplines for major research breakthroughs, but this runs the risk of fracturing the management research field when it urgently needs to become more united.

On the basis of a carefully executed empirical study of business schools, Khurana (2007) argues that top US schools have lost their way, colluding in a process in which 'higher' educational aims and the historical mission of creating a profession of management have been corrupted by more worldly goals, motivated not by the disinterested search for knowledge but by instrumental gain and naked self-interest. Recent events on Wall Street would seem, at least in part, to support this assertion (Delves-Broughton, 2008). Starkey and Tiratsoo (2007) make similar points, arguing for a different balance of disciplines in business schools to challenge the hegemony of finance and economics, and to reinforce the emphasis on 'school' rather than on 'business'. The thrust of their argument, with which we agree, is that business schools need to become stronger schools of social science rather than pursuing a professional school ideal for which they lack both the knowledge and the sense of mission.

### *Design and a critical realist turn*

This challenge to business schools about the scientific nature of their claims to knowledge supports a possible critical realist turn. Critical realism suggests that the relationships discovered in social science, not least the flux of human organizing and managing, are likely to be 'relatively enduring' rather than 'completely invariant', as would be expected in the hard, physical sciences (Kilduff, Mehra and Dunn, 2011, p. 307). As observed earlier, critical realism examines (among other things) the relations of the real, the actual and the empirical, one concern being to demonstrate that structures and cultures, while being relatively enduring, can be changed. In this sense critical realism has an emancipatory rationale: '[i]n order to break the mold of current thinking, it would be necessary, from this perspective, to tackle the past inheritances put in place by prior thinking that tended to shackle new discovery' (Kilduff, Mehra and Dunn, 2011, p. 308). This observation sets a challenging agenda for management research at a time when management in many sectors, public and private, commands a very low level of respect.

We think a renewed emphasis on the design aspects of management in a spirit of critical realism is one of the most promising ways forward for business schools and for management research. This requires scientific understanding of generative mechanisms for knowledge creation, a pragmatic concern for effectiveness ('does it work?') rather than 'truth' ('is it true?') as a guiding research principle, and knowledge of how to facilitate research agendas in specific contexts of application (Simon, 1969). Too much management research is context-free, evidence of a lack of credible research relationships with end-users. Design artifacts, grounded in generative research processes, constitute boundary objects that can facilitate productive interaction and collaboration between practitioners, consultants and academics, conferring insights from basic theory and research (Boland and Collopy, 2004). Design propositions (one form of design artifact) enable the communication of meaning between social science research and others involved in the design process, thus helping to close the gap between basic research and practice (Denyer, Tranfield and van Aken, 2008; Hodgkinson and Healey, 2008).

The design science approach has, at least implicitly, much in common with critical realist ontology (Hodgkinson and Rousseau, 2009), which frames knowledge generation in terms of generative mechanisms that foster new concepts and ways of conceiving of the empirical and the real and what can be made actual. A design philosophy promotes integrative thinking, as opposed to 'either/or' thinking, based on observation and inquiry, 'imagining something that does not now exist' but, if it could be brought into existence, would be more effective than that which it replaces (Dunne and Martin, 2006, pp. 513–515). Design science also embraces open systems thinking, using 'abduction', a logic of exploration, starting with tentative hypotheses that are explored until they lead to new practicable ideas. Of course, in this process one needs to remain sensitive to one of the dangers of the design science approach and, we would add, the search for evidence-based management, i.e. the danger of distortion by policy-makers and practitioners in the formulation and implementation stages. This is not the same as contextual adaptation, in which generating and communicating new meaning across previous 'divides' is a central concern. Design science operates well in the context of 'intervention', exemplified by the pioneering field work of the management research group at the Ecole des Mines de Paris led by Hatchuel (see, for example, Le Masson, Weil and Hatchuel, 2010).

Design science has yet to emerge as a unified approach to BMS but it offers a strong basis for moving the field forward and resolving some of the challenges we face. The design philosophy also challenges us to reconsider the boundaries of our field and of our own practices. Recently, for example, there have been a number of calls to downplay the emphasis on a science of management and think more about the art of management. This would require bringing elements of aesthetic and literary concern from the arts and humanities into our scholarship and management education programmes (Adler, 2006; Harrison, Leitch and Chia, 2007; Starkey and Tempest, 2009; Wensley, 2009). We are sympathetic to this suggestion, as we are to any suggestions that enrich understanding of the complexities of management thinking, decision-making and practice grounded in a concern for more effective management practice and better management theory, which we have suggested could come from an alignment of design science and critical realism.

Insider and outsider researcher perspectives are ultimately required if management research is to address its critics and evolve (Evered and Louis, 1981). The evolving political economy of management research in the UK and elsewhere means that it will increasingly have to justify itself in terms of the needs of multiple stakeholders on both sides of the practitioner–academic divide. The problem is even more complex than previous commentators have suggested. Non-academic stakeholder groups are at least as diverse and fragmented as their academic counterparts. A one-size fits all policy is therefore wholly inappropriate.

Mode 1 was too rigidly identified with methodological rigour at the expense of expansive or useful theorizing. We would stress the importance of theory and the significance of useful theorizing, accepting Kurt Lewin's (1951) argument that there is nothing as practical as a good theory. Management researchers must re-evaluate their conceptual and methodological armoury in order to ensure the field continues to be both scholarly and relevant to a diverse array of constituents, with a renewed emphasis upon co-production. Carefully defined concepts, judiciously assembled in a logical and systematic fashion, according to criteria of elegance and parsimony, will allow us to bring rigour to bear on the analysis of problems, both basic and applied. Strong theory is thus a key ingredient in the development of robust solutions.

While we recognize the relevance/impact challenge, unfortunately the field also faces a theory challenge. Not all theories are carefully thought through. All too often, concepts are ill-defined and thus lacking in precision. Furthermore, they are all too frequently assembled in a logically inconsistent fashion, often with a redundancy of terms, thus yielding incoherent explanations of the phenomena at hand. In consequence, such theories are of little value for practice or science. In short, we need a greater capacity in the field for relevant and rigorous theorizing (Weick, 1995, 1999). References, data, variables, diagrams and hypotheses are no compensation for a lack of theory (Sutton and Staw, 1995). If theory can be elaborated in the form of clear, testable propositions with design implications, this increases the chances of offering clear insights for those seeking to take action. We are sympathetic to Bazerman's (2005, p. 27) argument that '[c]ombining novel ideas with prescriptive advice is a recipe for impact'. We are less sympathetic to his dismissal of knowledge accumulation as a fine pursuit for its own sake. Rigorous, 'pure' research (not *yet* aligned with practice) needs to remain part of our research landscape. Bazerman (2005) pleads his own case – behavioural decision-making – too strongly in terms of its impacts. While this field is rightly receiving more attention, its influence on society, one of Bazerman's main claims, is still more aspirational than actual, as again is illustrated in the recent financial crisis. The social organization of the field still prevails.

## Concluding remarks

Relevance is a condition that exercises more than the management research profession. It may be a surprise to hear an economist, a respected econometrician, writing more then 30 years ago, that '[t]he opinion that econometric theory is largely irrelevant is held by an embarrassingly large share of the economics profession' (Leamer, 1978; cited approvingly by Lawson, 1999). The hegemony that economists hold in relation to policy and to social status (Ferraro, Pfeffer and Sutton, 2005) is a matter that should concern management scholars, especially given the (adverse) impact of economic theory and practice.

We suggest that we need a programme of research to quantify and understand the nature and extent of the various gaps not only between researchers, policy-makers and practitioners, but also among the various academic subgroups within and between BMS, the wider social sciences and beyond. Proponents of design science, and of evidence-based management, have yet to gather high quality evidence concerning the nature and size of the various gaps alleged between the diverse array of policy-makers, practitioners and academic researchers, and a sufficient body of compelling cases illustrating their resolution and bridging. We need to illuminate the generative mechanisms that contribute to gap widening and gap narrowing in all directions in order to develop a more nuanced approach to the advancement of the field as a whole.

We also need to recognize that, while many contemporary problems do indeed require closer engagement between researchers and the researched, others require a greater distancing, especially given the accusation of business school complicity with particular, and ideological rather than scientifically justifiable, views of how the world is supposed to work, views which recent events have demonstrated to be at variance with how the world actually works in practice. We still, on balance, tend to agree with Tranfield and Starkey (1998, p. 353) that '[t]he problems addressed by management research should grow out of the interaction between the world of practice and the world of theory, rather than out of either one alone'. Looking back over the past decade, there is no question that the British Academy of Management and the BJM have been instrumental in developing an important and signature debate which is helping in the realization of this fundamental goal.

The work awaiting is complex. We still lack a 'mainstream' in management research, such as that which prevails in economics (Fullbrook, 1998; Lawson, 1997; McCloskey, 1993; Pfeffer, 1993). We have suggested critical realism as a possible ontological basis for management research that completes the circle between theory and relevance. Just what should be the basis of this research remains to be defined. As Lawson (1999, p. 4) argues, 'it is not a part of the critical realist project to uncover or investigate the *specific* structures, including totalities or processes . . .. Such work is down to the individual sciences themselves.' We have suggested, for example, that psychology has much to contribute here. But, from a design perspective, the most severe criticism of business schools 'is that we are in a period of diminishing returns to research. That is because we have ploughed away at figuring out everything within narrow disciplines and the only way we can study those narrow disciplines is to assume away all the complexity and make them narrower and narrower' (Dunne and Martin, 2006, p. 517). We need more work on making explicit the relationship between management research and the base disciplines and how the alignment of disciplines can contribute to a holistic view of the multi-faceted phenomenon of management.

There remains, then, the task of developing a more convincing science, or a 'pre-science', of management, which, as Tranfield and Starkey (1998) argued, is as much an ontological as an empirical issue. This will involve management research addressing open but deeply structured systems of management as they impact upon the social world, and vice versa. As such, it will be 'concerned to identify and illuminate the structures, powers, mechanisms, processes and tendencies that produce or facilitate such actualities as the events, including human actions, that we experience' as management (Lawson, 1997, p. 287). We have suggested that conceiving of management as a design science can help us in this endeavour. From a critical realist perspective, management can be construed as a generative mechanism, comprising theory, practice and structures of reality. A social science of management will be focused on deepening understanding of effective management theory and practice. Of course, how to define effectiveness remains a contested terrain. Its elucidation promises a significant step change in our understanding of the economic world. A critical realist explanation will need to be structural, historical and generative if it is 'to account for the complex interplay between "structure" and "agency" as it works its way through reproducing and/or transforming the institutional arrangements that temporally preceded them and the dialectical interaction between them' (Reed, 2001, p. 216).

In concluding we should also make a point about context. We write in the context of a debate about rigour and relevance that is firmly Anglo-American. Our view is that the US notion of rigour is increasingly becoming the norm – in as far as policies for publication in academic journals are concerned – beyond the UK. Even French researchers are being encouraged to publish in US journals to prove they are 'world-class'. There is a danger here that this reduces variety and a US norm drives out innovation, a point made by various leading US scholars themselves in recent years. But in terms of the debate about what constitutes world-class this is certainly the direction of travel. This is likely to make the issue of developing alternative research paradigms – Mode 2, double hurdles, and the like – more difficult. We are aware personally of the difficulties of balancing our interests, for example in European social theory, with this trend. We hope we have in this paper demonstrated, at least implicitly in our discussion of an alternative design science paradigm and critical realism, that we are strongly rooted in continental European traditions of debate as both of these have European origins. We also hope that the future will allow more variety in management research than is currently visible.

However, we still have some way to go, both in developing understanding of the generative mechanisms that promote effective interaction between the worlds of the management researcher and of the manager (and the managed), and in developing appropriate techniques and processes for doing so. We welcome recent calls for engaged scholarship (Van de Ven, 2007; Van de Ven and Johnson, 2006). We end with a point stressed in previous *British Journal of Management* papers (Grey, 2001; Kilduff and Keleman, 2001; Weick, 2001), namely that independence and critical awareness are crucial to enduring relevance, particularly if we are to promote reflexivity about our own assumptions and to challenge too easily accepted modes of inquiry.

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