# Rhetoric and Evidence: The Case of Evidence-Based Management

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## INTRODUCTION

Evidence-based management has become a particularly hot topic, now, in the early years of the twenty-first century. And this chapter is in line with some of its ambitions; for example, it is intended, in part, to be a call for academics (and practitioners) to use more forms of evidence to inform practice. However, the chapter is also concerned to provide a critical exploration of some of the ideas about 'evidence' and 'management' that have become associated with evidence-based management. Or at least, evidence-based management in its most prominent form – which is to say the version that promises applications of science to management through which we can: 'hear and act on the facts [in order] to make more informed and intelligent decisions' (Pfeffer and Sutton, 2006: 14).

Curiously, however, for a movement that is so 'now', this promise is based on a version of science and evidence that is somewhat dated. For in today's intellectual environment, it is only rather traditional versions of science that fail to question the idea that scientific facts are the products of impersonal, disinterested, and rigorous observations concerning the evidence. This traditional view of science and evidence was most influentially propounded by Robert Merton (1973/1942) for whom science's 'truth-claims, whatever their source, are to be subjected to *preestablished impersonal criteria*; [t]he acceptance or rejection of claims entering the list of science is not to depend on the personal or social attributes of their protagonist; [o]bjectivity precludes particularism' (1973/1942: 270; italics in original). Evidence, in this traditional view, can be misunderstood, it can even be falsified or invented; nevertheless, the only judge we

evidence – according to which (of many, often contested) criteria'? Thus, the absence of any revealed awareness of the political positioning of this statement allows Rousseau's assertion to lionize managers as 'experts', while suggesting that, when associated with science and evidence, managerial decisions are above dispute (Willmott, 1997). Her assertion can be understood, therefore, as an example of how, following Grey (1996: 601), 'the ideological nature of management is obscured by the way in which it appears to be based upon objective knowledge independent of political or social interests and moral considerations'. In Rousseau's account then, the ideological nature of management is obscured by the rhetoric of 'evidence' – an evidence constructed in line with the interests of the people in charge of organizations, while giving itself out to be neutral and universal (Learmonth and Harding, 2006).

Nevertheless, while it is important to make these theoretical objections, my primary concern in this chapter is *not* to contest evidence-based management on intellectual grounds. Of course, contesting evidence-based management on intellectual grounds is worthwhile – I have attempted it myself (Learmonth and Harding, 2006), as have others, most recently Morrell (2008). However, I am de-emphasizing such critique here, largely because I suspect that the advocates of evidence-based management are not particularly concerned to be thought intellectually credible by social scientists. Indeed, it seems to me that Van Maanen's (1995) (gentle) comments on an earlier article by Pfeffer (1993) might equally apply to Pfeffer's own (and much other similar) more recent work on evidence-based management – at least, that is, if we were to read it as an intellectual project:

...insufferably smug; pious and orthodox; philosophically indefensible; extraordinarily naïve as to how science actually works; theoretically foolish, vain and autocratic; and – still being gentle – reflective of a most out-of-date and discredited father-knows-best version of knowledge, rhetoric and the role theory plays in the life of any intellectual community (Van Maanen, 1995: 133).

Instead, my concern is to contest evidence-based management as a *political project*. As I have argued elsewhere (Learmonth, 2006: 1090), proponents like Rousseau appear to be set on using their own versions of 'evidence' and 'management', 'as a means to further a particular set of interests and values in organizational life while doing so under cover – the cover provided both by the prestige of science and by the enthusiasm, in certain quarters, for (a narrow rhetoric of) evidence'. Indeed, once we read the pronouncements of proponents of evidence-based management as political manoeuvrings (and, what is more, under cover) they no longer seem so 'extraordinarily naïve'; in fact, I suggest, they become rather sophisticated. Thus, while winning the argument against evidence-based management may be necessary, it will hardly be sufficient, given that the rhetoric of evidence-based practices is being recognized by its proponents to provide them with a new political resource, which they can deploy (more or less surreptitiously) to resist the philosophical and ideological pluralism to which they object. Indeed, Rousseau (2006b) has made this resource explicit; for her, the promotion of evidence-based management 'would *counter* the current organizational research bias toward novelty and fragmentation' (2006b: 1091; italics added). All in all then, the rhetoric of evidence-based management seems capable of nurturing and reinvigorating an optimistic faith in (a managerially-orientated version of) 'science' – a version of science, we should not forget, that still remains influential (and often dominant) in many business schools throughout the world, despite the more radical theories of scientific knowledge advanced during the last generation.

## Evidence-based medicine: A model for evidence-based management?

Pfeffer and Sutton (2006: 13) claim that their 'interest in evidence-based *management* was inspired and, to some extent, guided by the evidence-based *medicine* movement'

(italics in original). So what is evidence-based medicine?

According to Harrison (1998: 15), it is, 'the doctrine that professional clinical practice ought to be based upon sound biomedical research evidence about the effectiveness of each diagnostic or therapeutic procedure'. This doctrine has an intuitive, seemingly incontestable appeal, and during the 1990s its worldwide popular success among health care professionals was rapid and dramatic (Trinder, 2000). Evidence-based medicine has now enjoyed well over ten years of popularity in most fields of health care across the globe, such that today, it seems virtually axiomatic that all clinical practice can, and should, be evidence-based. That is to say, that practice should integrate individual clinical expertise 'with the best available external clinical evidence from systematicresearch' (Sackett et al., 1996: 71). Furthermore, evidence-based medicine *can* work – at least when work means (quite specifically) that aggregate clinical outcomes get improved. Indeed, in this specific sense (and taking account of the problematizations that are beginning to emerge from social scientists (Lambert et al., 2006) certain clinical evidence-based practices are widely acknowledged to work impressively. However, its overall successes are often overdrawn (evidence-based medicine rarely, if ever, provides categorical, recipe-style answers for individual clinical situations) such that much optimistic hype is accumulating about it, with unintended consequences.

For example, whether or not clinical practice can be said to be evidence-based is taking on a significance that is wider than its mere effectiveness. As Morse (2006: 80) suggests, today, '*[e]vidence* and *evidence-based practice* have become the new mantras for health care' (italics in original). So, to be able to claim that practice is 'evidence-based' typically provides a significant source of prestige and legitimacy that contributes to the construction of professional identity (Traynor, 2004; Green, 2000). This also seems to be the case, incidentally, for health care managers. The Department of

Health in England, for instance, produced a guide for management development called the *National Health Service* (NHS) *Leadership Qualities Framework* that contains a prominent claim that the framework is 'evidence based, grounded in research with 150 NHS chief executives and directors' (Department of Health, 2004). So the same sorts of ideas that underpin evidence-based medicine have come to be central, not only merely in guiding individual clinicians' practice, but also in shaping research agendas, formulating health policy (Gordon, 2006), and allocating resources (Lambert et al., 2006). In sum, evidence-based practices have taken on a legitimatory and symbolic role that has created a cultural environment in health care in which *any* problematization of using 'evidence' – virtually regardless of context – can be constructed (and dismissed) as reminiscent of early arguments against evidence-based medicine.

But leaving aside the debates about evidence-based medicine, I now want to turn to the question of whether this enthusiasm for evidence makes sense in the context of the rather different discipline of management and organization. And I do so using the work of a thinker who has proved influential within the social studies of science – Thomas Kuhn.

## Normal science?: Medicine and management

The desire to make any kind of practice evidence-based relies on a particular intellectual framework being widely accepted by those involved. Significant disagreements about fundamental issues, and especially disagreements about what counts as 'evidence', would self-evidently make appeals to evidence ineffective as a persuasive device, and probably incoherent. However, Kuhn (1970) argued that a state of consensus about such fundamentals characterizes what he called *normal science*. For him, at any given point in history, the question of what counts as evidence and other matters basic to the conduct of science are usually more or less uncontroversial within a particular

discipline of natural science. Kuhn's claim was empirically based:

> [I]n the early stages of the development of any science different men confronting the same range of phenomena ... describe and interpret them in different ways. What is surprising, and also unique in its degree to the fields we call science, is that such initial divergences should ever largely disappear. For they do disappear to a very considerable extent and then apparently once for all (Kuhn, 1970: 17).

And to explain the disappearance of divergence, he argued that shared intellectual frameworks develop among natural scientists. For Kuhn, scientists typically come to share what he called a paradigm – a set of axiomatic conventions, which 'some particular scientific community acknowledges for a time as supplying the foundation for its further practice' (Kuhn, 1970: 10). The accepted conventions of the scientific community (such as what counts as evidence) are the more or less unexamined rules that enable scientists to come to a consensus about both what counts as a problem and the conditions to be met for a problem to be seen as solved. Ordinarily then, scientists do not question the very rules or foundations of the game, only certain moves within it – disagreement and competition are within certain parameters and limits that are unquestioned, at least for the time being (Fuchs and Ward, 1994). So normal science is able to produce what is widely accepted to be knowledge, Kuhn argues, by virtue of the fact that at any given point in history, members of the relevant scientific community can normally (more or less) agree on the foundational rules concerning how knowledge should be constituted.

Following Kuhn, it is apparent that a claim that clinical practice should be evidence-based only makes sense given the existence of a more or less unified paradigm that clinicians and researchers in a given health care discipline acknowledge as forming the foundation for their own knowledge. Indeed, proponents of evidence-based health care typically aim to ensure that only those studies produced within their paradigm can influence clinical decision-making. The principal method for assuring this – critical appraisal (for a description see Newman and Roberts, 2002) – can be interpreted after Kuhn, as a means of policing paradigmatic boundaries to ensure that only studies operating within the paradigm count as 'evidence'. That critical appraisal is generally represented as testing the quality of a study in what appear to be absolute terms, reflects the status and taken-for-grantedness of the paradigm in scientific and clinical thinking.

*Outside* normal science, on the other hand, the lack of an agreed paradigm means that disputes and divergences over foundations (for example about what counts as evidence and quite possibly about what count as problems) are likely to arise with such regularity that appeals to evidence as a means to resolve these disputes would become worthless. Outside normal science, criticism is not restricted to routine disagreement – it turns principled and radical (Fuchs and Ward, 1994). And Kuhn explicitly limited his empirical observations of the operation of normal science to the natural sciences. Indeed, he mentioned that as a natural scientist himself, he:

> [W]as struck by the number and extent of the overt disagreements between social scientists about the nature of legitimate scientific problems and methods ... [T]he practice of astronomy, physics, chemistry or biology [might we add medicine?] normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists or sociologists' (Kuhn, 1970: viii).

As Burrell and Morgan (1979) developed at length after Kuhn, organizational research is framed within a range of conflicting understandings of foundational knowledge – the nature of the social world, what is worth knowing, judgements about appropriate moral conduct and so on. (For a recent reformulation of that work see Deetz, Chapter 2, this volume.) In management theory, therefore, as in psychology and sociology, 'controversies about fundamentals' are endemic. The human sciences, including management, can never rely merely on instrumental reason, because

they always and necessarily connect with the contested politics, values, and beliefs that arise from particular ideas about the good society and different ways of being in the world that precede empirical inquiry and shape what is seen 'out there'.

So while some orientations to the study of management and organization are more popular and influential than others, as in sociology and psychology, there is no single, unified approach to any matter that is widely accepted by scholars within the discipline. There are voices in management studies articulating theoretical stances that include, for example, varieties of critical theory (Alvesson and Willmott, 1996), neo-Marxism (Thompson and Smith, 2001), postmodernism (Linstead, 2004), social constructionism (Harding, 2003), and feminism (Walby, 1986), that have a prominence that is far greater than in medicine or most natural sciences. As Burrell (1996: 394) has put it:

[T]he normal state of organizational science is pluralistic. This does not mean that organizational analysis is 'immature' or is awaiting its normal science phase with bated breath. It is simply that a plurality of legitimate and competing perspectives is to be expected.

Of course, my own representations of the controversies in the disciplines of medicine and management are likely to reproduce my perspectives and interests, positioned as I am in particular ways within the debates. As a management academic on the 'critical' wing of British business schools (Fournier and Grey, 2000), I no doubt have a self-serving predisposition to emphasize the extent to which management studies is fractured by political controversies. And I am aware that a small number of medical academics have expressed concerns about the dominant assumptions on which medicine as science is built (for example, Greenhalgh and Hurwitz, 1998; Tonelli, 1998). Nevertheless, I believe this picture of the level of contestation within the two disciplines is broadly fair, such that a range of contrasts between the academic study of the clinical health care disciplines (especially medicine) and

the study of management arise from the differences between the two fields in the levels of contestation surrounding their knowledge claims.

For example, it is widely believed that students in health care professions can be taught many techniques that allow for a high degree of confidence about the probability of their effects; for managers, techniques of agreed effectiveness are few in number, largely because there is no settled idea of what constitutes management effectiveness (Grey, 2004). Relatedly, and in a further contrast to the health care professions, managers are not obliged to undergo any kind of university-based training; indeed, countries that make extensive use of university business schools for management education like America and Britain do not seem to gain any particular advantage when compared to those that do not – say Germany or Japan (Grey and Willmott, 2002).

I submit, therefore, that within organization theory, it is hazardous, if not perverse, to expect a plurality of legitimate but competing theoretical perspectives and political orientations to converge in ways that enable (à la evidence-based medicine) the conscientious, explicit and judicious use of current best evidence in making decisions. Not because organizational questions are too complex to be susceptible to the sort of evidence-based measures now seen as axiomatic in clinical fields, but because, in organizational theory, what counts as evidence and how it should be understood are never merely technical questions. These sort of questions are posed outside a normal science framework so they inevitably have controversial epistemological, moral and political dimensions that make radical dispute – including dispute about what counts as evidence – nearly ubiquitous.

## Evidence as catchphrase

Nevertheless, in spite of these arguments, the new *slogan* of 'evidence-based practice' is proving attractive to all sorts of people – including, perhaps particularly important, bodies responsible for the governance and

funding of social research. For example, Britain's leading social science research funding body, the Economic and Social Research Council has a Centre for Evidence-based Policy and Practice (http://www.evidencenetwork.org/). And though (presumably) most of the social scientists who deploy terms like 'evidence-based practice' appreciate the complexities of the debates surrounding what evidence might be, the impact of this slogan-like rhetoric on others should not be underestimated. In particular, the idealized and overly optimistic representations of evidence-based medicine that the 'evidence-based' catchphrase seems to encourage are beginning to heighten expectations among politicians, policymakers, and others about the extent to which social research evidence can deliver useful technical fixes for them. Policymakers are starting to ask: 'if medical research can do it, why can't social research provide us with similarly categorical answers?'.

So, it is not entirely surprising that the Coalition for Evidence-based Policy (2006), for example, a body which advises American federal and state policymakers, claims at the head of its web-site:

> In the field of medicine, public policies based on scientifically-rigorous evidence have produced extraordinary advances in health over the past 50 years. By contrast, in most areas of social policy – such as education, poverty reduction, labor and employment, crime and justice, and health care financing and delivery – government programs often are implemented with little regard to evidence, costing billions of dollars yet failing to address critical needs of our society. [So the Coalition has organized] one of the leading web-sites on evidence-based programs – *Social Programs that Work* (www.evidencebasedprograms.org) – which provides policymakers and practitioners with clear, actionable information on 'what works' in social policy, based on evaluations that meet the highest level of scientific rigor.

Of course, these sorts of claims, concerned as they are with social settings, are vulnerable to a range of criticisms. One such criticism might follow Fox (2003: 81) in his critique of similar evidence-based

practices – the proponents of which he sees as typically seeking to claim an 'unmediated knowledge of reality'. Furthermore, and more directly political, an emphasis on 'what works' typically assumes that the current circumstances are a given. Thus, subjecting the circumstances *themselves* to sustained and detailed critical examination is beyond the ambitions of the model of science taken for granted in the Coalition's statement. Indeed, we might go as far as to argue that this inability to scrutinize the political interests served by the rhetoric of 'what works' can give politicians cover to pursue their partisan objectives, while appearing to be gathering objective, reliable, generalizable evidence (Majone, 1989) – a critique made particularly prominent in response to conservative regime during the Bush era (Denzin and Giardina, 2006; Mooney, 2005). And in a British context, evidence-based practices are, at the least, as Pawson (2006: 2) points out 'strongly associated with the so-called pragmatic, anti-ideological turn in modern [British] politics', that is, since New Labour came to power.

These sorts of objections to a natural science model being transferred to social settings have, of course, been well rehearsed in the social sciences over the years. But I want to emphasize again that the cultural context created by the evidence-based movement has rendered these arguments more or less redundant – at least in terms of the practical business of contesting a misplaced confidence in 'science' amongst those attracted by evidence-based practice. As Torrance (2006: 127) suggests, government agencies that commission social research now share 'an almost global "new orthodoxy" [which] seems perversely and wilfully ignorant of many decades of debate over whether, and if so in what ways, we can conduct enquiry and build knowledge in the social sciences, pausing only to castigate social researchers for not being more like (supposedly unproblematic) medical research'. Furthermore, the evidence-based rhetoric reinforces other longer-term trends – in particular the trend for the governance of social scientific research to be conducted as if the social sciences were

just another natural science. For, as Donovan (2005: 611) argues:

> [T]he point here is not what social scientists do or do not believe to be genuine social science, but the fact that at the policy level this decision has been taken out of their hands and is being externally regulated by non-social scientists. The policy preference is to model social science upon idealized natural science practice, favouring user-orientated, fact-finding, 'positivistic' approaches and their associated (and preferably quantitative) empirical methods.

Consequently, as far as people like those who control research funding, policymakers and practitioners are concerned, it would hardly be surprising if attempts to find evidence-based solutions became increasingly popular. A new twist, perhaps, on the debates about relevance – Mode 1 and Mode 2 knowledge, and all that (Gibbons et al., 1994; Grey, 2001; Starkey and Maddan, 2001; Starkey, 2001).

Practicing managers are unlikely (to say the least) to be much influenced by the critiques of science in critical theory, and from their perspective one can easily understand the attractions of a promise like that of the Coalition for Evidence-based Policy, to provide 'clear, actionable information on "what works"'. One can also understand why 'evaluations that meet the highest level of scientific rigor' would be attractive, in that the commonsense understanding of 'scientific rigor' might reasonably be assumed (however paradoxical this assumption might sometimes turn out to be) to result in advice untainted by ideology or sectional interests (Donovan, 2005). While one would not want to overstate the appetite managers and policy makers will ever likely have for academic evidence, nevertheless, as Trinder (2000: 5) argues, the central concerns of the evidence-based movement resonate with and mirror 'significant contemporary issues and concerns, namely those of risk, audit and effectiveness, rationalism, transparency, professional accountability, consumerism, empowerment, and the needs of the information society'.

So, to return to the proponents of evidence-based management, their optimistic claims for the efficacy of evidence start to make more sense when understood within a cultural context that has been influenced by the over-hyped successes of the evidence-based movement in medicine and elsewhere; an influence reinforced by trends in research governance, and by ideas about relevance, that continue to influence the research environment for organizational studies. Thus, on the back cover of the book by Pfeffer and Sutton (2006), David Kessler, the Dean of the School of Medicine at the University of California, San Fransisco writes:

> This book convinced me that the time is ripe for an evidence-based management movement. Just as medical decisions are better for patients when they are based on sound evidence, this same idea ought to be applied to management. Understanding the effects of your actions is critical, and both physicians and leaders who take this scientific approach will do a superior job of practicing their craft.

## Evidence-based management's effect(s)

In the light of these developments, I suggest that the growing confidence in science and evidence for management – as if management were more or less analogous to medicine – may well start to have tangible effects on organization studies. Of particular importance, the sort of environment that encourages practitioners to believe certain types of academic research can directly improve decisions would, presumably, increase the monies that research councils and other bodies (such as commercial companies) were prepared to invest in management research. This is an observation unlikely to be lost on the advocates of evidence-based management. Indeed, one of Pfeffer's (1993: 599) key justifications for his original proposal to enforce paradigmatic unity was to improve the ability of organization studies, 'to compete successfully with adjacent social sciences such as economics in the contest for resources'. So it is, perhaps, not entirely cynical to read Pfeffer and Sutton's (2006) book on evidence-based management as a sales pitch for what

academic management research can offer business executives.

Of course, any increases in research funds made available via evidence-based management would represent an important development for *all* organizational researchers – whatever their stance on evidence-based management – especially in the current climate in universities, where an individual scholar's ability to attract external research funding brings considerable institutional and personal advantages (Cheek, 2005). However, funding increases from evidence-based management would, no doubt, continue to be administered through the now well-established model for commissioned research; competitive tenders for projects that are specified in some detail, with evaluation procedures following the mantra that only 'useful' research should be funded (Donovan, 2005; Cheek, 2005). As Pawson (2006: 3) comments:

> In the UK, much if not most, policy inquiry is conducted by units and centres that perch on the edge of mainstream university departments and whose existence depends on winning the *next* contract. Oftentimes, this means that the policy-research relationship is financially circular, with one arm of government providing the funds for another to supply the evidence base ... [T]he increasing role of the private sector should [also] be noted, as both recipients and providers of information. Unhappily, one also observes that this new function for auditors coincided with the outbreak of corporate scandals about their traditional role as independent regulators.

It is hard, therefore, to be optimistic about the effects of such increased funding on management researchers who might want to resist the impact of managerialism. Indeed, it seems likely that we would either become increasingly torn between the demands of funders and our preferred methodological and political orientations – or face a greater marginalization because of a relative failure to generate research income. The key point about either scenario is that they would both represent a surreptitious move toward the paradigmatic unity that Pfeffer (1993) proposed; surreptitious because there would be no need for individual scholars to be seen

directly imposing their views – rather the imposition would occur *de facto* – largely through the demands of research funders and the incentives they have in their gift to encourage (and discourage) various forms of research.

Such processes are already particularly advanced in much of the research carried out, for example, in American education policy research and in organizational research in British health care. If we want to see the potential effects of evidence-based management on a pluralistic, critically-orientated organizational research we might start by looking in these areas. In American education, in spite of a strong tradition of qualitative research in this sector in the past (House, 2006; Lather, 2006), federally funded policy research is now being dominated by randomized control trials and the other quantitive, experimental designs that are widely used in medical research. Today, such methods effectively more or less exclude funding from any form of qualitative research – a phenomenon that Lincoln and Cannella (2004: 7) call 'methodological fundamentalism'. This is a fundamentalism imposed by politicians with the encouragement of those educational scholars whose interests are served by the domination of quantitive methodologies. Indeed, House (2006: 103) points to the moral fervour with which some American education researchers object to certain qualitative methods:

> Experimental advocates regard such studies as blasphemous and lament the disrespect to scientific truth. In their view, they are re-establishing the authority of social science, particularly the authority of the methodology they learned as graduate students and nurtured as professors.

Another, somewhat different, example is health care in Britain, where the government research-funding agency, the NHS Service Delivery and Organization (SDO) Programme, has since 2000 been commissioning, 'research evidence directed at improving the organization and delivery of health services and to promote the uptake and application of that evidence in policy and practice'

(SDO, 2006: 1). SDO work has become important to organizational research in British health care because of the relatively large amounts of additional funding it has made available to researchers. While the agency often does commission qualitative work, its explicit policy is to develop research that *managers* find useful (Edwards, 2003). Thus, a senior health care executive chairs the programme board, and other health managers are well represented on it (SDO, 2006: 3). Furthermore, its research funding is administered through closely specified tenders that give researchers little freedom in the sorts of reports that they produce (Learmonth and Harding, 2006). Perhaps, then, in place of the *methodological* fundamentalism of American education policy research, what can be seen in health care organizational research in Britain is a managerialist orientation that effectively produces an *ideological* fundamentalism. As Learmonth (2003: 110–11) has argued, the SDO:

> ...appear[s] to be orientated towards an uncritical acceptance of managerialist literature, including 'popular' management titles [such that] it is not implausible to believe that the [largely pro-management] orientations [of organizational research in the UK health sector] are caused by the demands of funders rather than scholars' preferred intellectual commitments.

Indeed, in a recent personal conversation about some of the arguments in this chapter, the head of a university research centre, heavily reliant on funding from bodies like the SDO, told me that he felt a certain sympathy toward much of my analysis. However, he believed that his current institutional position effectively precluded him from publicly criticizing the funding bodies on which the centre relied. And anyway, people in bodies like the SDO seem to me to have become more or less impervious to any criticism of their stance on evidence-based management. My guess is that their very *raison d'être* has become so closely aligned with evidence-based management that it is now difficult for them to do anything other than stonewall or dismiss all invitations to rethink their position.

## DISCUSSION AND CONCLUSION

So what would I like to see instead? Perhaps the most important thing, in contrast to developments in America and Britain, would be the encouragement of a radical heterogeneity, both in the nature of research questions, and in the research traditions employed. So it is not that I am denying the importance of running organizations efficiently and effectively. Indeed, I think that evidence will always have an important role in helping us to change organizations in beneficial ways. However, against the current trend to homogenize evidence within conservative frameworks – conservative because they do not examine received ideas about organizational realities – I am advocating that forms of evidence should be as broad as possible, including those forms that explicitly challenge managerialist beliefs and assumptions. Such challenges, it seems to me, are likely to provide opportunities for bringing new sources of creativity to organizing as well as encouraging more open debates that represent wider constituencies and interests.

However, the practical difficulties faced in adopting this position are considerable, not least because of the resistance that powerful groups with an interest in evidence-based management might offer. Governments, managers, and others usually look to academics to give them straightforward answers to their difficulties and dilemmas; they may well be reluctant to fund research that does not provide these answers, or that explicitly opposes their interests. On the other hand, however, should academics acquiesce to pressures to produce work that simply serves management purposes, then in the longer term, I think we risk failing society at large. Such acquiescence could even remove incentives to comment outside institutionally approved discourses. And if it were to do so, academic work would ultimately not be in the interests of *anyone* in organizations, including its top managers. So my concern – that the popularization of evidence-based management has the potential to bring strong incentives into play for critically orientated researchers

to compromise their work – is a concern that has significance well outside universities themselves.

For example, I already come under (gentle) pressure to seek grants from bodies like the SDO. Indeed, it seems to be standard advice to someone in my position, that for the purposes of career progression, one should apply for grants from these bodies – provide them with the managerially orientated reports they require – then write separate, critical academic papers from the data. But one of the problems with this advice is that reports to such bodies are in the public domain; indeed, they are considerably more likely to be read by practitioners than articles in scholarly journals (Learmonth and Harding, 2006). It seems, therefore, that the political risks of this action suggest that critical researchers should think long and hard before compromising, though our promotion prospects would undoubtedly be enhanced by compromise.

So, I have been pondering ways of resisting the worst of the negative possibilities that any further popularization of evidence-based management might have in store for people like me. I suggest that 'attack' may be the best form of defence. For as Fox (2003: 97) points out, the logic of evidence-based practice 'does not demand that all research stops if it is not immediately "relevant"'. In fact, rather the opposite is the case – the rhetoric of 'evidence' can, in principle at least, provide new opportunities for all types of empirically-orientated researchers to get more of a hearing – research and evidence are open equally to be constructed as subversive as to be constructed managerially. Even Pfeffer and Sutton (2006: 230) (though they 'hesitate to recommend' it) briefly discuss the value of what they say 'might be called *evidence-based misbehavior*' (italics in original). They realize that the logic of their position on evidence-based management means that it would be legitimate for subordinates, more aware of the evidence than their managers, to subvert orders in the (evidence-based) interests of the company.

Thus, one response to evidence-based management, in line with more radical sensibilities, would be to develop Pfeffer and Sutton's own suggestions about evidence-based misbehaviour, only with less hesitancy. Indeed, Fox (2003: 89) commends what he calls 'transgressive research' to researchers confronted with evidence-based rhetoric, arguing that it represents 'practice-orientated research that is constitutive of difference, challenges power and constraint, and encourages resistance and new possibilities'. Transgressive research seems to be potentially of great value for activists and others involved in organizations (including, perhaps many people with manager in their job title), unhappy with the status quo and seeking ways to resist the norm and bring about radical change. However, the results of rigorous social scientific work will seldom provide evidence that gives activists evidence-based advice about how to 'misbehave'. As Weick (2001: 73) argues: 'when people experience uncertainty and gather information to reduce it, this often backfires and uncertainty increases. As a result … the more information is gathered, the more doubts accumulate about any option'. Therefore, if research is 'relevant' it will probably be in its ability to change thinking about so-called 'transgressive' activities, or about the acceptability of standard managerial practices. I suggest that practicing managers and others in organizations need to be rather wary of academics presenting what they do as if it were consultancy.

Nevertheless, I wonder whether conducting forms of transgressive research might be one way for academics to survive within evidence-based discourses while having the chance to subvert its political intentions, and still carry on doing the kind of work that I think is important. It might even be a way, furthermore, following Grey (2001: 32), in which to:

…reimagine relevance so [that business school academics] see themselves at the centre stage of working with all the complexities of knowledge, free from the demands of relevance – or, more accurately, free from the current restricted, persecuted and persecutory imaginations of what relevance might be.

Nevertheless, I am not naive enough to imagine that transgressive research will be the quickest route to career and other conventional forms of institutional 'success'. I guess we would have to work very hard indeed for it to become acceptable to bodies like the SDO, even though it produces 'evidence' that many (nonmanagerial and marginalized) groups would find 'relevant'. But I think, politically, it is imperative to keep trying.

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