

EVIDENCE-BASED MANAGEMENT: THE VERY IDEA

Authors:

Mark Learmonth, University of Nottingham, UK

Nancy Harding, University of Leeds, UK

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Contact: Mark.Learmonth@nottingham.ac.uk

ABSTRACT

This essay critically evaluates the recent phenomenon of ‘evidence-based management’ in public services that is especially prominent in health care. We suggest that the current approach, broadly informed by evidence-based health care, is misguided given the deeply contested nature of ‘evidence’ within the discipline of management studies. We argue its growing popularity in spite of the theoretical problems it faces can be understood primarily as a function of the interests served by the universalisation of certain forms of managerialist ‘evidence’ rather than any contribution to organizational effectiveness. Indeed, in a reading informed by the work of French geographer Henri Lefebvre, we suggest that in the long term the project is likely to inhibit rather than encourage a fuller understanding of the nature of public services. We conclude with a call for forms of organizational research that the current preoccupations of the evidence-based project marginalize if not write out altogether.

INTRODUCTION

Today, it is widely accepted in Western health services - both by governments and at local level - that evidence enhances the rationality of clinical decisions and that clinical decisions should therefore be made on the basis of ‘the conscientious, explicit and judicious use of current best evidence’ (Sackett *et al* 1996). Raine has rightly characterized evidence-based practices as the current ‘zeitgeist in health care’ (1998 p.251), in that, despite problematising voices (for example, Wood, Ferlie and Fitzgerald 1998; Green 2000; Trinder and Reynolds 2000; McLaughlin 2001;

2004; Traynor 2000; 2004; Wood and Ferlie 2003; Lather 2004) mainstream debate and government policy have come to focus upon the means by which clinical decision-making can incorporate scientific evidence; the aim in itself having become almost axiomatic.

In parallel with this development, evidence-based approaches are increasingly being commended for policy and management decision-making in the public sector - especially (though not exclusively) in health services (Hewison 1997; Axelsson 1998; Homa 1998; Øvretveit 1998; Leicester 1999; Kovner, Elton and Billings 2000; Iles and Sutherland 2001; Walshe and Rundall 2001; Stewart 2002). Just as evidence-based medicine is now officially sanctioned as the best way to reduce uncertainty in clinical practice, so the proponents of evidence-based management believe that evidence about organizational phenomena can reduce managers' uncertainties. As Stewart put it 'evidence-based medicine has led to ... a way of thinking that can and should be applied more generally in management' (2002, p.17). Indeed, in the UK, a government agency called the NHS Service Delivery and Organisation (*sic*) (SDO) Programme has existed since 2000, and in 2003/2004 it spent just short of £3.7 million (SDO 2004, p.32) to provide: 'a national research programme ...to consolidate and develop the evidence base on the organisation (*sic*), management and delivery of health care services'. (SDO 2004, p.6).

Traditionally, medical research has insisted upon a hierarchy of evidence in which the randomized control trial is preeminent (Harrison 1998); evidence-based management in contrast is methodologically and epistemologically considerably more eclectic. So whilst quantitative methods are used, qualitative methods driven by non-positivistic orientations are also significant

if not dominant in the field (Ferlie *et al* 2001; Iles and Sutherland 2001; Murphy and Dingwall 2003). Furthermore, though evidence-based management still seems to be the most popular formulation, terms such as ‘evidence-informed decision-making’ are also used, presumably indicating caution about the capacity of evidence to be the sole basis of management decision-making given the importance in real life of pragmatic and other considerations.

Nevertheless, the basic doctrine of evidence-based management remains one appropriated from evidence-based health care: that a consideration of evidence will increase the rationality and thus the effectiveness of managers’ decisions. Both in medicine and management, approaches basing practice on ‘evidence’ assume a science based on laws that can be elucidated for the benefit of all (Ozcan and Smith 1998; Wood and Ferlie 2003); and in both, ‘the evidence’ tends to be presented as if it were independent of the social circumstances of its production.

However, inattention to the socially situated position of management knowledge has meant that the proponents of evidence-based management typically take for granted certain institutionalized features of organizational life (Meyer and Rowan 1977). In particular, the domination of organizations by management elites and the idea of organizations as manageable entities are both unexamined as potentially problematical questions and debates. And so, in contrast to the long tradition of workplace ethnographies for example, which have typically been concerned with exposing power, conflict and inequality in organizations (Smith 2001) the evidence of evidence-based management tends to support and reinforce *managerialism*. That is, such evidence (usually

unobtrusively) reinforces managerial power by legitimating some forms of 'evidence' whilst obscuring non-managerial ways to understand organizational life.

This paper is therefore a critical exploration of evidence-based management in health care. We first echo certain traditions broadly within the sociology of scientific knowledge (including for example, Latour 1987; Woolgar 1988; Cooper and Law 1995; Potter 1996), which remind us that evidence is never simply out there waiting for the researcher to find; rather our methods for engaging with the world act to construct 'evidence' in particular ways. A central concern is to reveal the unobtrusive but constraining operation of power associated with the dominant ways evidence tends to be constructed - through which alternatives are typically disregarded or rendered invisible (Deetz 2003). So, in contrast to Walshe and Rundall, for whom 'it is surely in the interests of all stake holders to have better, more evidence-based processes for making managerial decisions' (2001 p.453), the aim is to highlight how *particular* constructions of evidence have come to be treated as if they were *universal* - thereby promoting and naturalising conceptions of evidence that tend to perpetuate power, inequalities and forms of practices that allow for the domination and control of some over others.

These arguments lead us to ask what is the effect of a body of theory which collapses as soon as a critical lens is focused upon it? To answer this question we turn to the people who normally remain hidden in the development of evidence-based management - the researchers and those who commission their work. In examining their roles we draw upon the writings of Henri Lefebvre

(1991) which allow us to analyze the spaces of lived organization, and contrast them to those organizational spaces idealized and imagined by the proponents of evidence-based management.

But we do not conclude by advocating the abandonment of evidence in organizational life; indeed, we are proposing rather the opposite view. Against the current trend to homogenise evidence within conservative frameworks - conservative because they do not examine received ideas about organizational realities - we are advocating that forms of evidence should be as heterogeneous as possible, including those that explicitly challenge managerialist beliefs and assumptions. Indeed because we value heterogeneity, we are potentially appreciative of a broad range of the more radical research traditions found in organization and management studies; traditions that problematise the standard readings of organizational life and are likely thereby to encourage new understandings of organizational realities to emerge (Learmonth 2003). We start the debate with a comparison of the rather different intellectual traditions from which the study of medicine and the study of management typically draw inspiration.

NORMAL SCIENCE? MEDICINE AND MANAGEMENT

The desire to make any kind of practice evidence-based relies upon 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) has 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:

in 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 (1970, p.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' (1970, p.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 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 scientists' and clinicians' 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

was struck by the number and extent of the overt disagreements between social scientists about the nature of legitimate scientific problems and methods. ... the 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' (1970, p. 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. 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'.

Admittedly, a significant number of management scholars are attracted to dominant theories such as contingency, and some have argued that there should only be one paradigm (Donaldson 1996). On the other hand, 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.

So whilst 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. Indeed, following Grey and Willmott we would go as far as to suggest that: '[p]eculiar to management is the extent of fragmentation and indeterminacy in its knowledge base ... [t]he extent to which its knowledge claims are open to contest and disruption is highly unusual and possibly unique (2002, p.413).

Burrell has made a similar point in explicitly Kuhnian terms:

the 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 (1996, p.394).

We recognize that our representations of the controversies within the disciplines of medicine and management are likely to reproduce our own perspectives and interests, positioned as we are in particular ways within the debates. As management academics on the 'critical' wing of UK business schools (Fournier and Grey 2000) we no doubt have a self-serving predisposition to emphasize the extent to which management studies is fractured by political controversies. And

we are aware that a small number of medical academics have expressed concerns about some of the dominant assumptions on which medicine as science is generally thought to be built (for example, Greenhalgh and Hurwitz 1998; Tonelli 1998). Nevertheless, we believe our picture of the level of contestation within the two disciplines is broadly fair, such that a range of contrasts arise between the academic conventions and practices in management and those in the clinical health care disciplines (especially medicine) because of the differences in the levels of contestation surrounding their respective knowledge claims.

For example, it is widely believed that students in health care professions can be taught very 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 the US and UK do not seem to gain any particular advantage when compared to those that do not - say Germany or Japan (Grey and Willmott 2002).

We submit therefore, that within management theory, it is hazardous 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 sorts 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.

Furthermore, within management studies not only are there radical debates over ontology or epistemology, but over what Sturdy and Grey have called the 'conceit of manageability' (2003, p.660): that is some management scholars question whether organizations can be regarded as manageable entities at all (Grey 1996; Roper 1996; Burrell 1997; Willmott 1997; Parker 2002). But this debate is not simply obscured by evidence-based management - it is necessarily written out: such radical understandings of 'the evidence' cannot be included in the project of evidence-based management for it to remain coherent.

DE-RADICALIZING DISPUTE

Nevertheless, in the academic literature concerned with promoting evidence-based management *some* of the problems with management 'evidence' are acknowledged. But in our reading of this literature, there is always an attempt to *de-radicalize* dispute in one way or another. Disputation cannot be acknowledged to be over fundamentals, or it would represent a threat to the coherence of the evidence-based movement in management.

For some proponents of evidence-based management such as Axelsson (1998), radical dispute is seen as something that can simply be ignored because it occurs outside the domains of normal science. Though like us he thinks that much academic management research is outside normal science, unlike us, he believes that this kind of research need not be taken seriously. For Axelsson management research is becoming ‘more and more esoteric and largely dissociated from the problems of management ... [having come] to a scientific dead-end, where nothing can be proved or disproved any longer’ (1998, p.307). He commended instead only the evidence that he believed to be legitimate - that which can be contained within a normal science paradigm - as he put it, management research ‘inspired by the practical and scientific developments of Evidence Based Medicine’ (1998, p.308; italics omitted).

More realistically in our view, other proponents such as Walshe and Rundall (2001) devote much of their argument to a discussion of the *problems* associated with adapting from medicine an evidence-based approach for management. For example, Walshe and Rundall believe that compared to doctors, ‘managers make rather fewer but larger decisions [which] ... may take years to be made and implemented, and it can be difficult even to discern or describe the decision-making process or to pin down when a decision is actually made’ (2001, p.444/5). Also, in terms of the research base, they acknowledge ‘the loosely defined, methodologically heterogeneous, widely distributed, and hard-to-generalize research base for health care management is much more difficult to use in the same way [as medicine’s research base]’ (2001, p.444). And furthermore, managers’ attitudes to applying research in their jobs is also seen as problematical - compared to medicine: ‘[p]ersonal experience and self-generated knowledge play a much larger part in

determining how managers approach their jobs, and there is much less reliance on a shared body of formal knowledge in decision making' (2001, p.439). Nevertheless, Walshe and Rundall still believe that efforts should be made in 'outlining an agenda for action to promote the development of evidence-based management' (2001, p.430).

The magnitude of the problems for such an agenda however can be further illustrated by Scott *et al*'s recent attempts '[t]o review the *evidence* for a relationship between organizational culture and health care performance' (2003, p.105; italics added). At the start of their essay, they highlighted their 'striking finding' (2003, p.105): the absence of consensus in the organizational theory literature about definitions for either culture or performance. Indeed, from their discussion, it would seem reasonable to infer from this absence of consensus that a search for 'evidence' in these areas would introduce *more* ambiguity and uncertainty, thereby seriously reducing (rather than promoting) the possibilities for rationality enhancing decision-making. As they put it:

Once we accept that performance is as contested a domain as culture, and that culture and performance are likely to be mutually constituted, then the difficulties of reconciling the two domains through simplistic equations such as 'strong culture equals superior performance' begin to seem insurmountable (2003, p.115).

But perhaps surprisingly, in spite of the theoretical problems to which they draw attention, Scott *et al* do not draw the conclusion that a consideration of the evidence introduces more

ambiguity (though the tenor of some of their comments might be read to hint at such a possibility). Rather their explicit suggestion to deal with the divergent nature of management evidence is to posit a methodological solution: '[c]onsiderably greater methodological ingenuity will be required to unravel the relationship(s) between organisational (*sic*) culture(s) and performance(s)' (2003, p.105).

We interpret this statement as an attempt to de-radicalize dispute, in that to suggest methodological ingenuity is sufficient to resolve disagreements is to suggest (misleadingly) that disagreement among management scholars can be resolved as it is in normal science. That is, within more or less universally accepted parameters and limits, limits that allow for certain 'methodologies' to be axiomatic and beyond dispute - and therefore able to adjudicate between any disagreement. But as Ormrod has suggested, in commenting on the debate about the use of organizational culture in health services policy and research, contrasting ideas about culture 'derive from different paradigms, different ontological and epistemological commitments, which by definition are incommensurable and not liable to synthesis' (2003, p.230).

The puzzle for us lies in trying to explain why so many health management commentators appear to want to play down dispute in order to make evidence-based management seem plausible. After all, at least in the UK (though see Lather [2004] for a contrasting picture of the recent use of 'evidence' in US education policy development), other areas of the public sector more strongly influenced by the social sciences than health care such as social work and education have accepted evidence-based practices in rather more muted ways (Trinder 2000). The question

is then, why has there not been a similarly muted reception in health care management? We submit that the popularity of evidence-based management in health care is due not so much to its utility in addressing effectiveness problems nor to its theoretical coherence, but rather to the intellectual climate in health care and the management interests that are served by dominant beliefs about ‘evidence’ in such an environment. This is the theme to which we now turn.

EVIDENCE-BASED MANAGEMENT AND UNOBTRUSIVE POWER

The intellectual climate of health care today remains one that is dominated by the normal science assumptions of medicine. Among many with influence in and around health services, there seems to be what Giddens (1991) has characterized as a sustained optimism in the ability of scientific experts to find technological solutions for physical ills. And these technological solutions are assumed to be applicable in principle to social fields as well - including management. Thus all aspects of health care have become environments in which ‘evidence’ is saturated with positive cultural valences. In a management context, for example, the English Department of Health has recently produced a guide for management development called the *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’ (DOH 2004, no page number).

Thus, being able to claim practice is ‘evidence-based’ is not merely a technical matter, it also has what might be called a symbolic-legitimatory role (Alvesson 1993) in the sense that representing actions as evidence-based provides a significant source of prestige and legitimacy that can

contribute to the construction of professional identities (Green 2000; Traynor 2004). In such an environment, 'all things should be evidence-based' can become a mantra that no longer glances back sceptically at its premises; in this environment, problematisations are easily and dismissively constructed as being 'reminiscent of early arguments against evidence-based medicine' (Iles and Sutherland 2001, p.75). What is more, we submit, this kind of intellectual and emotional environment is likely to be interest-serving. Indeed, Alvesson and Willmott have argued that the sort of applied management research necessary for producing management evidence is likely to have the effect of 'creating and legitimizing an image and ideal of managers as impartial experts whose prerogative is associated with, if not founded upon, scientifically respectable bodies of knowledge' (1996, p.27; italics omitted).

In health care such an effect is likely to be especially strong because the production and use of evidence in management suggests that management is associated with similar sorts of evidence to that which health care professionals are being encouraged to use in their practice. This represents a particularly significant shift for doctors, who, as Llewellyn has shown, have not generally regarded '[m]anagers ... as possessing abstract, scientific knowledge, and any craft experience that they have is depicted as being derived from careers that started as 'office boys'' (2001, p.605). But evidence-based management might suggest to doctors that management *is* in some way abstract and scientific - informed by the research of academics from prestigious institutions. And as Grey has argued, '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' (1996, p.601).

We want to emphasize the unobtrusive nature of these processes. We are not suggesting that the introduction of the discourse of evidence into management has been a planned managerial intervention; indeed, given the taken-for-grantedness of dominant beliefs about evidence in health services, most managers no doubt understand the incorporation of evidence into their practices as a politically innocent way to help deal with effectiveness problems. The problem is that the normal science ideal of a-social evidence has (perhaps, in part, accidentally) elevated *managerialist* evidence as the only form, thus shaping its format and constraining some of the questions that might be asked, paradoxically causing problems for those whose experience of the complexity of the everyday world of health care does not conform to the model presumed in evidence-based approaches.

MANAGING THE EVIDENCE

To illustrate some of the processes involved, we now turn to a report by Iles and Sutherland (2001). The report was commissioned by the SDO to review popular management ideas for their applicability to managing change in health care in a way that would be accessible and relevant to health care managers and professionals and is broadly similar in tone to other work aimed at communicating the benefits of evidence-based management to a practitioner audience (cf. Homa 1998; Øvretveit 1998; Kovner, Elton and Billings 2000; Stewart 2002; Edwards 2003).

The work is important in terms of evidence-based management for two reasons. Firstly, it is well-known within UK health services, having been widely distributed by the SDO without charge amongst health service managers and health practitioners (Cranfield and Ward 2002). Secondly, it was the first major report commissioned by the SDO and has formed the basis for some of their further work (see for example, Iles and Cranfield 2004).

The SDO specified that the review should include a consideration of the following:

re-engineering; the business excellence model; Total Quality Management; Continuous Quality Improvement; the learning organization (e.g. Peter Senge's *The Fifth Discipline*); the McKinsey 7S model; the Theory of Constraints; as well as theories set out by management writers such as Peters and Waterman, and Kanter (SDO 2000; no page number; in Learmonth 2003, p.111).

Such authors and ideas have enjoyed much optimistic hype from management consultants over the last couple of decades (see Knights and Willmott [2000] or Legge [2002] for critical reviews) such that it is possible to represent these ideas, following Clark and Salaman (1998), as *guru theory*. Indeed, Clark and Salaman's list of gurus and associated ideas (p.138) included almost all of those appearing in the SDO's brief. But for Clark and Salaman most of these ideas would not count as theory in conventional academic settings - for them, guru 'theories' are important to consider, not for their intellectual content, but because they provide a window on

managers' values, and ... describe what [managers believe] organizations are like, how they work and how they must be managed, ... [they therefore] offer a conception of management itself in virtuous, heroic, high status terms. Guru activity ... not only constitutes organizational realities, it constitutes managers themselves (1998, p.157).

However, and in sharp contrast to Clark and Salaman, Iles and Sutherland (2001) were concerned to provide a broadly sympathetic reading of the intellectual content of these theories. They never used the term *guru*, and rather than understanding these ideas as (at least potentially) a set of partial and managerialist representations of organizational realities, they universalized them by examining merely the empirically testable claims these writers made about organizational life. Significantly, this empirical emphasis was in the (ostensibly apolitical) tradition of evidence-based clinical practices. Ignored therefore, was the possibility that this kind of work may have constitutive effects or that it might proceed from managerialist values.

For example, commenting on Peters and Waterman's (1982) excellence ideas, Iles and Sutherland noted criticisms such as the lack of supporting empirical studies; that five years after publication, two-thirds of the excellent companies had slipped from the pinnacle of success; and that the conflict and dissension that also shape an organization's culture is ignored by the work. But much more radical, politically-orientated critique of Peters and Waterman has been voiced. Willmott, for example, has offered an extended and widely cited critique of the instrumental use of culture by Peters and Waterman, which he suggested has gained popularity because it 'aspires

to extend management control by colonizing the affective domain ... by promoting employee commitment to a monolithic structure of feeling and thought' (1993, p.517).

Other commentators in similar traditions (for example, Linstead and Grafton Small [1992]; Parker and Dent [1996] or Strangleman and Roberts [1999]) might also concede Iles and Sutherland's empirical point that Peters and Waterman's ideas on culture have produced ways in which managers might influence change in their organizations. But for them, as for Willmott, the impact would be interpreted as domination rather than an appropriate extension of managers' legitimate functional authority.

But this more critical 'evidence' on organizational culture (along with Clark and Salaman's work) is absent from Iles and Sutherland's report. The absence allowed Iles and Sutherland to opine that in spite of the (presumably relatively minor) acknowledged problems with the approach, Peters and Waterman's (1982) teaching

is considered important by many commentators because of its dual emphasis on 'soft' organizational components ... as well as the 'hard' ... popularizing the notion of organizational culture as the 'normative glue' that holds together the organization (2001, p.28/29).

Citing unnamed 'many commentators' gives the (misleading) impression that management scholars broadly agree about the positive ways in which Peters and Waterman's work is

‘considered important’; we were not told that a significant number of prominent academics consider it important for rather darker reasons. And it is submitted that the plausibility of this misleading impression is enhanced when it is given in the broad context of evidence-based health care. As we have seen, disagreements of a radical or political nature are much rarer among scholars in medicine and other clinical fields; but after reading Iles and Sutherland’s report it might be assumed by people accustomed to health care research that management academics are as ideologically united as commentators in health care disciplines seem to be.

So again there is a puzzle: why was this kind of material omitted? Perhaps, had Iles and Sutherland discussed the more critical material on the work of the gurus they examined they might have produced an unacceptable report, one that implied that a rational consideration of the evidence is not enough to resolve questions of how managers should manage change, because the ‘evidence’ shows that the desirability of the outcomes of these management interventions is disputed. Also, had their report included such alternative evidence, it might not have gained the favorable appraisal the report did in fact receive from the audience of UK National Health Service staff (Cranfield and Ward 2002). Not least because, as guru theories were already officially sanctioned and widely in use within the UK National Health Service at the time, critiquing these theories on ideological and ethical grounds would almost certainly have been interpreted as an attack on current practices and government policy. And as Fuller has noted, client-centred researchers know to be careful not to ‘draw undue attention to the client’s role in maintaining the power relations revealed in the ... report’ (2000, p.11).

THE GEOGRAPHY OF EVIDENCE-BASED MANAGEMENT

In the first section of the paper we have offered a critique of evidence-based management on two grounds. We have shown firstly that the appearance of management as a unitary academic discipline whose models are unimpeachable and thus applicable to health organizations is, to say the least, misleading - management studies are riven by paradigmatic disagreement over what can be regarded as 'evidence'. Secondly we have suggested that the neutrality assumed within a natural science paradigm surreptitiously serves a political agenda when it is applied to management. This critique leads to the question of what effect such a flawed model may have, the question to which we now turn.

Our answer turns the spotlight *away* from the organizations studied by evidence-based management proponents and swings it around to shine upon the people who commission and undertake the research; people who, in the tradition of normal science, typically remain invisible (Woolgar, 1980; 1988). By turning the spotlight in their direction we see them revealed as embodied individuals occupying other spaces (their own organizations), privileged perhaps by the places they occupy, but themselves subject to the powerful discourses which underpin ideas about what counts as legitimate knowledge (Foucault, 1980). The exploration of embodied people occupying spaces and places has been undertaken by critical geography, notably by Henri Lefebvre, and so it is to Lefebvre we turn to explore the possible outcomes of a continued dominance of this 'territorializing spatial practice' (Duncan, 1996). Our analysis following Lefebvre leads us to conclude that evidence-based management as currently understood is likely

to lead in the longer term to a diminishing knowledge of organizational practices and thus weaken policy-making.

Lefebvre (1991) explored the production of space itself. Though a Marxist, Lefebvre's work can nevertheless be read to have had strong affinities with that of Foucault and other French thinkers in similar traditions who were Lefebvre's contemporaries. Lefebvre's ideas have not yet been widely used in political science or organization studies (for exceptions see Hernes [2004] or Ford and Harding [2004]) but we suggest his work is relevant to understanding evidence based management because it allows us to distinguish in new ways between three groups: (1) those undertaking the work of delivering health care services within the particular spaces and places of service delivery; (2) those occupying separate and distinct spaces, who are responsible for undertaking the research that provides the evidence; and (3) those in a further space, who are responsible for commissioning the research and disseminating beliefs about evidence based management. So first we briefly sketch Lefebvre's theory of space and then use it to analyse these 'spaces' in relation to evidence-based management.

Most fundamentally, Lefebvre dismissed the possibility of a transparent, pure and neutral space – for him, all places are understood only through 'the act which recognises parts, and, within those recognised parts, an order' (1991, p. 297). For him, space is a social construct consisting of metaphorical territories and spaces of power (Harvey, 1993); space thus 'hides what it depends on for its meaning: an other' (Rose, 1993, p. 71). Space, for Lefebvre then, is divided into what he called 'a conceptual triad' (1991, p. 33):

representational spaces or the lived space - that which overlays physical space, making symbolic use of its objects.

spatial practice, or perceived space - what can be communicated and thus known about space;

representations of space, or conceived space - that tied to the relations of production and thus to knowledge. This is the dominant space in any society as it *defines* what can be communicated about space. The first and second yield to the third, generating incipient tensions which may transform into absolute antagonism.

Following Lefebvre (1991), representational or lived spaces are the embodied places we inhabit and through which we move - social spaces. There are uncountable numbers of them and they interpenetrate social life and allow ‘actual or potential assembly at or around a single point’ (1991, p. 101). Here, space is something that is actively produced through both social interactions and the impositions of discourses of space. For example, we make the lecture theatre or GP practice lived spaces by making them the places in which the performances of lecturing or consulting take place. Through this process we can call them the lecture theatre or surgery, arrange artefacts (including bodies) within them so as to conform to the lived aesthetic of each place, and direct ourselves within them to produce these spaces. Importantly for our analysis of evidence-based management, these lived spaces are highly complex - they are not simply juxtaposed but may be intercalated and in collision (1991, p. 88); or they may interpenetrate or

superimpose themselves upon one another (1991, p. 85). Lefebvre's lived space is therefore the outcome of past actions, it permits fresh actions to occur whilst suggesting or prohibiting others. In this sense then it is *produced*, but not necessarily within conditions of the participants' own choosing; lived space is the bearer of norms and constraints, and so tends to be repressive (1991, p. 358).

The second of Lefebvre's aspects of space is perceived space - what can be communicated or known about space. Its function is to reduce 'real' space to the abstract. To continue our examples of the lecture theatre or GP practice, we make these spaces within an image of how we think a university or health service should be and how we should work within them. These images are thus projected into a space, become inscribed there, and so the possible spaces that may emerge in specific places are limited by the possibilities of thought: alternative ways of thinking about how space may be used, are 'the unthought aspect of the thought that has now pronounced itself sovereign ruler' (1991, p. 397). We make lived spaces through our interactions, but the possibilities for defining those lived spaces are inhibited by the actions of perceived space which prescribes limits upon the possibilities of what these spaces might become.

Finally, conceived or representational space, dominant space, the third of Lefebvre's three types of space, defines what can be known and communicated about space. Conceived space imposes 'the primacy of the written words, of "plans", of the visual realm' (1991, p. 307). It 'subsumes and unites scattered fragments or elements by force' (1991, p.307). So in conceived space, what we might take to be ordinary things, acts and situations are, 'forever being replaced by

representations' (p.307). Thus, conceived space is the space that makes politics thinkable because it is inhabited by 'a total subject which acts continually to maintain and reproduce its own conditions of existence' (p. 94). For Lefebvre the state was the absolute political space: for him it represented 'that strategic space which seeks to impose itself as reality despite the fact that it is an abstraction, albeit one endowed with enormous powers because it is the locus and medium of Power' (1991 p. 137). So conceived space, Lefebvre argued is the tool of domination, what he called a 'lethal space' (1991, p. 370).

Applying Lefebvre to Evidence-based Management

For Lefebvre, all space must be *read* to be understood (Elden 2004). We now offer a reading of the three spaces of evidence-based management through Lefebvre's lens. First, the space which is read by evidence-based management - the lived space occupied by those who work in the delivery of services; next, the perceived space - occupied by the researchers who read the lived space; and third, conceived space - constructed through the discourses that make things sayable and thinkable about the lived space.

Lived space is where people undertake their work of delivering services. All of us will have undertaken some form of 'reading' of a health 'space' - for example, when visiting a hospital or a GP - visits to a very material place. Moving through the rhythms and aesthetics of this territory, the visitor will have been aware of the many processes occurring between the numerous people gathered within the locale. These constellations of relations make up a territory experienced as a

material artefact, but this experience is mediated by more or less taken for granted metaphorical and psychological meanings that allow the complex social processes of space construction to occur and thus allow space to be experienced as such an artefact (Harvey, 1993).

Thus, when studied by researchers, the very complexity of lived space means that researchers can grasp only the most superficial picture of it and must inevitably abstract that ‘reality’ according to some simplifying theoretical model (MacDonald, 2004; Parker, 2004). Hay has recently made this point in a policy analysis context, arguing that the rational choice theory which underpins much economic and political science views of public sector organizations suffers from a parsimonious simplicity of analytical assumptions which abstract so much from the complexity of the realities they claim to represent that they ‘guarantee implausible analytical assumptions’ (2004, p. 45). Lefebvre called these simplifying analytical assumptions the quasi-logical presupposition of an identity between conceived space (the space of the philosophers and epistemologists) and lived space, arguing that an abyss exists ‘between the mental sphere on one side and the physical and social spheres on the other’ (1991, p. 6). Such theories, we suggest, are like maps which permit us to ‘grasp an outline, a shape, some sort of location, but not the contexts, cultures, histories, languages, experiences, desires and hopes that course through the [social] body’ (Chambers, 1993, p. 188).

Researchers occupy their own lived spaces, but when developing evidence-based management (trying to make sense of the complexities of the lived spaces of the organizations they observe;

committing those complexities to the linear reality of the written page) they work in perceived space. Interpreting Lefebvre, Soja claimed that this process:

is entirely ideational, made up of projections into the empirical world from conceived or imagined geographies. This does not mean that there is no material reality, [no lived place], but rather that the knowledge of this material reality is comprehended essentially through thought, as *res cogito*, literally ‘thought things’. (1996, p. 78).

Furthermore, Soja argued that this imagined geography tends to become seen as the ‘real’ geography in the sense that ‘the image or representation come[s] to define and order the reality’ (1996, p.78). Thus evidence-based researchers may state, say: ‘these are the relationships between culture and performance’ - fitting abstracted variables into abstracted simplifying models so that, for them, the multiplicity of micro-practices undertaken in those forums disappear beneath the seeming calm of the model.

However Soja assumed occupants of perceived and lived spaces were the same, but in evidence-based management the manager who is attempting to manage culture and performance is in a very different space to the researcher who writes of these processes. The manager occupies a site of multiple, overlapping and interacting discourses of space, place and identity not the ‘model’ identified by the researcher. Should managers attempt to reduce the complexity of the lived space to the simplicities of the perceived space of evidence-based imagination they will create

confusion for they will see something that exists in the minds of others rather than in the places they attempt to manage.

Finally, conceptual space is the space which contains the power to control how space is thought about. It is here that are found the theories which dominate at any particular time. For Lefebvre this was the space of capitalism, occupied by capitalists. We prefer an understanding that sees discourses imbued with power/knowledge as they speak through government or SDO officials, dictating that researchers investigate only the type of space which they, in this space, believe exists. Thus organizations come to be constructed as ‘things’ to be worked upon, moulded and adjusted to achieve ‘objectives’, and officials fund research which confirms them in this model of organization. When such research fails it becomes plausible to blame the managers for the failure rather than the model used to abstract space, for the model has achieved the status of the taken-for-granted. The possibility that it is the model to blame does not enter thought.

Our position then is that evidence-based management, as it is currently constituted, claims to describe a world which does not exist in lived space: its world ‘exists’ only as a series of concepts. And unfortunately, these conceptual spaces have little relationship to the lived, material spaces its proponents claim they represent. They claim to provide maps of aspects of health care organization that enable managers to produce the most efficient and effective organizational forms. But these maps are so devoid of an understanding of the very thing they claim to represent, that they are maps not of the lived world of people in health services, but of other spaces (Parker 2004).

CONCLUDING DISCUSSIONS

So how might we start to produce research that gives us maps of organizational spaces that are more likely to represent the lived worlds of people in health and other public services - and what might we do with such maps? We submit that the encouragement of *radical heterogeneity* both in the nature of research questions and in research traditions employed is likely to be an important starting point.

For example, issues such as organizational change and culture have no necessary connection to questions of how such things might be managed and controlled - nor are they the exclusive concern of those who have management responsibilities. So we welcome research traditions that consider these and other organizational themes in ways that are (often explicitly) divorced from questions of how to manage. Such work typically focuses on the views of groups with little conventional prestige and finds inspiration in varieties of social and political theories that offer sharp counterpoints to traditional managerialist ideas on these matters. Smith has summarized the contribution of such work as being: 'squarely positioned to detect how power is exercised, control asserted and maintained, conflict and resistance expressed, and social inequalities manipulated and recreated' (2001, p.224). For examples of such work in a health care context see for example, Porter (1991); Allen (1997); Fox (1997); Traynor (1999); Carter (2000); Lee (2004) McDonald (2004).

But taken together, this sort of work tends to be messy and contradictory - in line with what we might expect given the complexities of lived space in Lefebvre's analysis. Furthermore, being outside normal science, different papers can have very different theoretical underpinnings and serve contrasting interests. It is therefore very hard to read such a body of literature (if we can allow it to be put together as a 'body') to provide answers to the difficulties and dilemmas of managers' - or anyone else! Indeed, this characteristic of the evidence challenges the assumption that a consideration of evidence will necessarily decrease uncertainty - that is, that 'the evidence' will converge to suggest a more or less clear best way forward. In an organizational context at least it is much more likely to be the case that the evidence will typically be *divergent*. The implications of this stance are summarized by Weick: 'when people experience uncertainty and gather information to reduce it, this often backfires and uncertainty increases. As a result ... [t]he more information is gathered, the more doubts accumulate about any option' (2001, p.s73).

So what might we do with this kind of 'evidence'? The fundamental problem we have with the currently dominant approaches to evidence-based management is that they tacitly reinforce and legitimize managerialism. However, this legitimization has only been achieved by marginalizing the sort of 'evidence' we are commending here. If such evidence were to be introduced within the mainstream of evidence-based approaches, it is likely that it could become a means to unsettle and destabilize what otherwise might comfortably be assumed about the nature of organizational life and public policy-making. Whilst such destabilizing *could* involve the risk of paralyzing action, such risks are not inevitable and we believe they are worth it for the opportunities likely

to be provided for bringing new sources of creativity to policy making and encouraging more open debates that represent wider constituencies and interests.

We are not unaware, however, of the practical difficulties likely to be faced by adopting the position we advocate. Governments and public managers usually look to academics to give them relatively straightforward answers to their difficulties and dilemmas; they will no doubt be reluctant to fund research that does not appear to provide these answers. Nevertheless, we submit that academics who are attracted to researching within public sector organization but outside managerialist perspectives, are ultimately likely to lose more than we will gain should we acquiesce to the pressures to produce work that simply serves instrumental managerialist purposes, however substantial such pressures may be. Although some pragmatic compromises might be inevitable, such acquiescence seems likely to take us down the slippery slope towards loss of academic freedom and may perhaps remove any incentives to comment outside institutionally approved discourses. This is a position that, in the end, would hardly be in the interests of public services as a whole, including their top managers and policy-makers.

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