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Health (London) 2003; 7; 267

DOI: 10.1177/1363459303007003002

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Resisting evidence: the study of evidence-based medicine as a contemporary social movement

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ABSTRACT Evidence-based medicine (EBM) emerged relatively recently to describe the explicit process of applying research evidence to medical practice. The movement was high profile, yet not overly successful: many clinicians do not use up-to-date evidence in their everyday work. This article shows how a social movement perspective can be used to analyse the emergence of EBM and shed light on power struggles between segments of the medical profession. It draws on Blumer's (1951) essay on social movements to demonstrate the continued salience of this approach. The article also presents empirical data from a qualitative study of English and American surgeons to illustrate how EBM provides a focus for segmental conflict within medical practice between 'art' and 'science', 'practice' and 'evidence'. Together these data and the social movements perspective provide insight into the dynamics of this struggle and help to explain why clinicians continue to resist EBM.

KEYWORDS *evidence-based medicine; social movements; surgical practice*

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Introduction

The presence of coalitions or segments within medicine that can be understood as social movements was established in the early 1960s (Bucher and Strauss, 1960; Bucher, 1962; Bucher and Stelling, 1969). However, there has been relatively little work exploiting the insights of Bucher and her colleagues to extend the analyses of social movements in medicine.¹ This article attempts to address this gap in the literature by demonstrating how

a social movement perspective can be used to analyse the emergence of evidence-based medicine (EBM). Social movements highlight areas of contested power. Giddens (1985) has argued that at the centre of labour and occupational movements there typically lies a struggle for defensive control of the workplace and the work within it. The case of EBM highlights just such a struggle, not between workers and managers, but between segments within the medical profession. This struggle also includes groups outside the medical profession such as health and policy researchers, non-medically trained epidemiologists and academics.

Earlier work on social movements drew on theoretical ideas developed by Salomon (1946), Blumer (1951) and Heberle (1951), typically centred on political and religious movements. While sociological interest in social movement theory appeared to wane in the 1970s as the student protest and various revolutionary movements crumbled, it has undergone something of a revitalization in both Europe and the USA in recent years (see, for example, Lyman, 1995) with the growth of social protest around ecological, community and gender issues. One of the critiques of the sociology of social movements to emerge in this more recent period is that it lacks a conceptual framework for understanding the processes by which groups construct and reconstruct power (Benford and Hunt, 1992). Benford and Hunt themselves offer a dramaturgical framework – using ideas about scripts, staging, performing and interpreting – to show how actors within social movements develop conceptions of power. While Benford and Hunt's approach is seductive, as they concede, it inspires a different set of research questions – centred for example on how actors (participants in movements) successfully use dramatic techniques. In this article I deliberately draw on earlier work by Blumer (1995 [1951]), which can be used to provide a framework for understanding EBM as a social movement. Although written over 50 years ago, Blumer's essay describes the characteristic features of social movements. He manages to do this while retaining the idea of social movements as dynamic, and their development as processual rather than linear, by showing how the evolution of a movement is dependent on the strength and effectiveness of its ideology, tactics and so on. These ideas resonate with the contemporary movement around evidence-based medicine. Finally, it is worth adding that Blumer's contribution to medical sociology extends well beyond his theoretical ideas about social movements: he invented the term 'symbolic interactionism' and was key proponent of naturalistic research methods. Given the qualitative empirical work presented in the latter half of this article, I therefore confess at the outset some prior sympathies with Blumer's theoretical ideas and methodological outlook.²

Blumer defined social movements as 'collective enterprises to establish a new order of life' (Blumer, 1995: 60). He outlined three broad types of social movement referred to as general, specific and expressive movements. My focus here is on a specific social movement. Specific social movements have a well-defined objective or goal (Blumer, 1995 [1951]) and may be

further sub-divided as reform or revolutionary movements. Reform movements accept the existing social order but seek to reform some specific or limited area of that order. Blumer cited the anti-slavery movement of the 19th century as an example of such a reform movement. Here I suggest that EBM is a contemporary example of this type of social movement by examining the evolution of EBM using Blumer's description of the five mechanisms by which social movements grow and become organized.

The evolution of the EBM movement

In the 1990s 'evidence-based medicine' became the fashionable coinage to describe a process of self-consciously incorporating research evidence into medical practice. The idea that scientific research should be a component of medical knowledge was, of course, not new. Modern medical training draws heavily on the scientific knowledge of such disciplines as biology, anatomy and biochemistry. Latterly it has also incorporated the relatively newer science of epidemiology, the discipline concerned with the investigation of the causes and natural history of diseases in populations. In embracing epidemiology, medicine took on board a range of research methods for measuring disease in populations and evaluating the impact of medical interventions on groups rather than individuals, including one, the randomized controlled trial (RCT), which has become especially significant, and is frequently cited as a 'gold standard' within medical research. The focus of this article then is on how EBM as a new social movement has organized to ensure that a particular (explicit) form of evidence, epitomized by the randomized controlled trial, is incorporated in medicine. While the desire to make practice evidence based is not exclusively concerned with questions of effectiveness, and therefore with evidence from trials, many of the things associated with EBM, for example guidelines and the delineation of a hierarchy of evidence, are closely bound up with this project. Moreover, much of the resistance to the EBM movement is located around its specification of evidence – and therefore the knowledge base of medicine – as rational/technical rather than contingent/experiential. In order to understand EBM and the nature of this resistance, it is necessary to understand how it evolved: Blumer's work on social movements provides a way of unpacking this process.

Blumer suggests that agitation:

operates to arouse people and so make them possible recruits for the movement. It is essentially a means of exciting people and awakening within them new impulses and ideas that make them restless and dissatisfied . . . For a movement to begin and gain impetus, it is necessary for people to be jarred loose from their customary ways of thinking and believing. (Blumer, 1995: 65)

One such moment in the genesis of EBM was a lecture by the British epidemiologist, Archie Cochrane, published in 1972 as *Effectiveness and*

efficiency. Regarded as something of a maverick among his peers, Cochrane's thesis was that much contemporary medical practice was ineffective, or at worst, harmful. He advocated the use of randomized controlled trials to test the effects of medical interventions. RCTs, he argued, allowed the effects of an intervention to be tested by randomly allocating research subjects to either an intervention group (who received the experimental treatment) or a control group (who received another treatment or a placebo). The effect of the intervention could then be judged by comparing measures taken from both groups before and after the intervention. Ideally, both the research subjects and those carrying out the intervention should not know which group received the intervention: this is the blueprint for the double blind RCT. Research evidence from such trials could be used, Cochrane argued, to ensure that only effective treatments were administered, and that precious financial resources for national health care could be used more efficiently.³ Similar arguments were voiced in the USA and elsewhere and over time the RCT became a yardstick by which other sources of information were judged and ranked within a hierarchy of evidence.

Blumer's second mechanism involved the growth of some notion of common or shared experience among a select group – the development of an *esprit de corps*. Cochrane's ideas, and this emphasis on evaluating treatments by subjecting them to RCTs had a profound impact on medicine, particularly on segments of public health and epidemiology. As Willis and White (1999) have pointed out, this field of medicine was not, traditionally, a high-status area, largely because of its focus on population rather than individual or clinical aspects of medicine. The term *clinical epidemiology* was first used in the 1930s to signal attempts by some epidemiologists to move their expertise closer to the bedside. However, it came to prominence in the 1980s with a group of doctors who shared a conviction that the scientific base of clinical practice (and medical education) should be strengthened. The preface to a key textbook, *Clinical epidemiology* (first published in 1985), provides an indication of the shared experience of these doctors:

it dawned on each of us that there was, in fact, a science to the art of medicine. For [Sackett], this realisation came when the Cuban missile crisis transformed him, a tenderfoot nephrologist and renal tubular physiologist, into a reluctant field epidemiologist in the U.S. Public Health Service. Although obligated to learn epidemiology (most of it for the first time), he remained a clinician at heart and was repeatedly surprised by the extent to which his growing knowledge of epidemiologic principles could shed light both on the illnesses of his patients and on the diagnostic and management behavior of their clinicians. Moreover, it dawned on him that applying these epidemiologic principles (plus a few more from biostatistics) to the beliefs, judgements, and intuitions that comprise the art of medicine might substantially improve the efficiency of diagnosis and prognosis, the effectiveness of management, the efficiency of trying to keep up to date, and of special importance, the ability to teach others how to do these things.

... For [Haynes], the need for an additional basic science for clinical medicine entered his consciousness during a preclerkship lecture on Freudian concepts of psychiatric illness. When he meekly requested the evidence for one of these concepts, the speaker expostulated that the purpose of the lecture was to transmit content, not defend it (and then admitted that he didn't believe it himself).

... As a medical student in England [Tugwell] was attracted to clinical epidemiology by the challenges of trying to apply the principles of population epidemiology to the care of individual patients. However, when he sought career guidance from a world-renowned London epidemiologist, he was informed that it was 'amoral' to combine clinical epidemiology with clinical practice! (Sackett et al., 1991: ix-x)

It is clear from this account that the idea of clinical epidemiology was not, initially at least, widely supported within epidemiology, or the wider medical profession. Nonetheless, a very similar account of this esprit de corps can be found in another key text, Fletcher et al.'s *Clinical epidemiology: The essentials* (1982). What these accounts identify is a common, shared experience that united this segment of the medical profession. The features of their shared experience included dissatisfaction with clinical practice or with education for practice, a knowledge, however rudimentary, of 'epidemiologic principles' and a recognition that practice might be strengthened by the application of this epidemiological knowledge. It is not insignificant that the development of this esprit de corps was especially associated with a group of doctors at McMaster Medical School, Canada, renowned for pioneering a unique approach to medical education. Sackett and his colleagues shared both a common experience and a particular perspective about that experience.

Shared experience is insufficient of itself to sustain a social movement. A third mechanism in the growth of a movement is the development of an enduring sense of purpose, group will or morale within which the convictions of a group can be articulated. This mechanism was apparent in the mid- to late 1980s when, notably in Canada and North America, the ideas of these 'clinical epidemiologists' and medical academics began to converge on EBM. Public health doctors, epidemiologists and health services researchers increasingly advocated the use of RCTs, and considerable research effort was directed to the synthesis and dissemination of trial evidence. There was a growing sense of self-belief and rectitude among these groups within the medical profession. Increasingly this group of clinicians was identified, not with 'clinical epidemiology' or even 'epidemiology', but with 'evidence-based medicine'. This term encompassed the goal of the movement – to root medical practice in scientific evidence.

By the mid-1990s this goal was articulated in a clear ideology (Blumer's fourth mechanism) for the movement, for example in articles by the Evidence-Based Medicine Working Group (1992), Rosenberg and Donald (1995) and Sackett and Rosenberg (1995).⁴ The ideology of this new social

movement was defined as 'the ability to track down, critically appraise (for its validity and usefulness), and incorporate [the] rapidly growing body of evidence into clinical practice' (Sackett and Rosenberg, 1995: 249). Articles began to delineate strategies by which the movement could achieve its goals – the fifth and final mechanism in the growth of a social movement. These strategies included incorporating EBM in formal medical education and encouraging practitioners to use summaries of evidence in the established medical press and new journals set up to support the movement. This activity spawned another movement to generate systematic reviews that would collate, synthesize and disseminate evidence. The early Cochrane Collaboration groupings formed for this purpose had an almost evangelical mission to recruit group members systematically to review existing trials. Another tactic used by the movement was the adoption of practice protocols or guidelines. These were systematically developed statements about appropriate care or treatment, designed to assist clinical decision making (Battista and Hodge, 1993: 385; Delamothe, 1993: 218). Guidelines enabled evidence about treatments to be distilled into recommendations or algorithms for 'good' practice. The nature of these guidelines varied; some were simple advisory checklists, while others were more rigid and prescriptive. The regulatory potential of guidelines made them particularly attractive to health care policy makers, purchasers and administrators, notably in the USA where they were closely linked with private sector insurance and governmental cost containment strategies. Perhaps unsurprisingly given this regulatory aspect, practice guidelines became a focus for opposition to EBM (Grimley Evans, 1995; Hopkins, 1995; Naylor, 1995). Much of this opposition was rooted in arguments about the differences between the research evidence base of EBM and the everyday practice of medicine. The social movement organized around EBM provided a focus for this conflict and an opportunity to rehearse other arguments about the tension between the science and the art of medicine and between individual patient needs and aggregate data.

Responding to the threat

By the mid-1990s the ideology and tactics of the EBM movement were clearly articulated. In addition to the clinicians who took these ideas on board, EBM was also 'adopted' by government and health care policy makers. Both the Department of Health strategy for the English National Health Service, and the US National Institute for Health and the Agency for Health Care Policy and Research had mandates which stressed the principles of EBM. Yet, while 'evidence-based medicine' and 'evidence-based practice' frequently appeared in policy documents and medical journals, published studies of medical practice continued to suggest that the findings of scientific research often failed to penetrate everyday medical work (Haines and Jones, 1994; Davis and Howden-Chapman, 1996; Harrison,

1996; Rosen, 2000). In addition, the response of many clinicians to the EBM movement appeared negative or hostile (see, for example, Blavias, 1995; Grimley Evans, 1995).

The critics of EBM suggested that there were fundamental differences between the science encapsulated in EBM and the nature of everyday medical work. Many of the 'good reasons' doctors gave for resisting EBM were closely tied to the 'hegemony of the double-blind, randomized controlled trial' (Charlton, 1991: 335). The central problem, according to these critics, lay with the nature of RCT evidence, essentially with the issue of internal versus external validity. Randomized trials provided an overview of the effect of an intervention on all the patients included in a trial. Making the shift between this type of evidence and medical practice was problematic. Trial evidence described the benefits from an intervention applied to a *group* of people, and had external validity, but the clinician had to decide whether these average benefits applied to an individual patient who consulted them. The opponents of EBM argued that the EBM approach ignored the complex variation and individuality inherent in medical work: for example, Sullivan and MacNaughton's comment that 'the doctor does not deal with illnesses alone but with people who are ill, and for each individual the illness is unique in terms of his or her experience of it and in its presentation to the doctor' (Sullivan and MacNaughton, 1996: 941).

These critics argued that, in order to make sense of the unique circumstances of the individual case, doctors used a form of practical knowledge or judgement quite different to the knowledge offered by EBM. The evidence base of EBM was technical: it was capable of formulation, and might be written in journals or specified as rules or guidelines. By contrast, the opponents of EBM claimed that medicine drew on a more nebulous type of knowledge variously referred to as the 'other kind of medicine' or the 'grey zones' of clinical practice (Balint, 1961; Naylor, 1995; Sullivan and MacNaughton, 1996). Sociologists recognize this knowledge type, following work by Knafl and Burkett (1975) and Freidson (1986) as 'clinical judgement'. Many critics of EBM argued that this clinical judgement or 'art' was a feature of all medical work. Some claimed that the tension between art and science was especially acute in surgical work (Russell, 1996: 1480) and this led me to explore this issue further using data from a study of a small group of English and American surgeons.

Surgeons' accounts of their 'art'

The research project from which these data are drawn was not about EBM. Rather, it was concerned to investigate the nature of everyday surgical work, and, in particular to explore surgeons' accounts of practice variation. The research involved 34 surgeons practising in urology and gynaecology/pelvic surgery. All the surgeons were involved in treating women with urinary incontinence, all but two were in senior posts (English consultant

or American attending status) and just under half worked in teaching hospitals. Twenty-nine surgeons were based in hospitals located in two English regions; the remaining five were based in three hospitals and a private clinic in three eastern states of the USA. Eight of the surgeons were women. The data presented here come mainly from interviews with these surgeons about the nature of their everyday work treating incontinence, although the study as a whole used both interview and qualitative observational methods. These accounts describe and defend the notion of medicine (or rather surgery) as an art and these ideas about individual versus group data. I suggest that they represent an occupational defence against EBM: this talk serves to articulate a response by one segment of the medical profession (a group of surgeons) to the threat posed by a reform movement (EBM).

When talking about surgical work these surgeons described a wide range of highly variable factors which influenced their decisions and actions. Some of these related to patients, for example anatomical peculiarities or complex co-morbidities (co-existing illnesses or conditions) in the presenting case. Other variables centred on the surgeon and included such things as personal preferences, abilities and emotional state on a given day and so on. Still further variation might be introduced by the external environment surrounding surgical work – such as differences in the type of surgical equipment available or the skill of a surgical assistant. Elsewhere I have argued that these variables constitute contingency because they are at once conditional and affected by chance factors (Pope, 2002).

The interviews explored how surgeons dealt with contingency. Many of them suggested that their practice was guided by rather nebulous, instinctive ‘hunches’ or ‘gut feeling’. For example, one claimed that: ‘the only thing that can guide you is how much dissection you are prepared to think you can get away with’ (G2.1;3:14). This personal knowledge was not formally taught. Another interviewee said he responded instinctively to events in the operating room:

I don't know where it comes from, instinct maybe. It's something that can't be taught. With some people they just don't tell you how to, you know, 'how did you do it?' They say, 'it felt right, just something about it'. Who knows what visual clues your mind is reading that are not really coming out, like 'hey, there's a bleeder there'. Who knows what you're seeing, what's all the stimuli, all the visual things that are going into your brain at that moment and coming out and telling you 'hey, slow down there's a pothole in the road'. (USG1.1;11:2)

The ways a surgeon might respond to contingency could be at variance with formal training. This surgeon, himself a trainer, explained that:

When you are teaching someone to do the Pereyra [operation], it's all in the feel, putting the sutures up, and putting your fingers against the bones and breaking through, it's quite a difficult thing to do, it is very scary when you do it and you are not quite sure where you are. (U1.1;3:32)

The contingencies of everyday practice often meant that descriptions of

surgical procedures as they appeared in journals or textbooks (the evidence base for EBM) did not match with the procedures these surgeons actually performed.

I've read textbooks, I've read [names surgeon] textbook on gynaecological surgery, he talks about picking up fascia on the actual vaginal flaps that you've dissected. I find it very difficult to sort of do really. What I do is . . . (G9.1;4:1)

Experience was held to be vital in developing non-technical, instinctive responses and abilities. Practical skills were seen as emerging out of a process of personal learning and 'trial and error', as in these examples:

I think it's just trial and error. Prolene is a suture because it's inert and I tried the Neurolon because I thought it would be stronger and it wouldn't snap but it developed sinuses and was unacceptable so I've gone back to using Prolene. (G12.2;7:13)

There is no model that we have, standard, to take you from the step of seeing a procedure and trying it out without having to do it on a live patient. There is no standard procedure . . . you actually go and see it, assist and the time constraints are that probably that's as much as you will get before you then have a go at doing it and of course like most procedures even if you assisted at lots and lots it is not until you actually do a procedure for yourself on your own that you discover all the difficulties. . . . you have to make the mistake, make the errors to learn. (G2.1;7:55)

The ability to act in the face of contingency did not derive from formulated technical knowledge of the kind embodied in EBM. Everyday practice as described by the surgeons was at variance to the model of surgery offered by formal (written) sources of evidence in which all procedures were standardized and performed on a broadly similar group of patients. Inclusion criteria for trials, for example, meant that patients with complications or co-morbidities, the very patients often seen by this group of surgeons, were typically excluded. As the quotations presented earlier about the use of textbooks show, formalized accounts or evidence were often dismissed by surgeons. Instinctive responses to contingencies were acquired from exposure to concrete surgical problems. This model of experientially learned practice, centred on practical knowledge of 'how' to perform procedures and was demarcated from the technical knowledge base of EBM characterized by 'what' to perform. This position was augmented by further arguments about surgical individuality. The aphorism 'your life in their hands' held especial resonance for surgeons. They were concerned that surgical work centred on the particular pair of hands of an individual surgeon:

I think we both do our own operation, what is called a colposuspension I'm sure will vary up and down the country and within any one unit, to a greater or lesser degree . . . I can't assume that everybody is doing it like me, God Forbid! (G16.1;7:35)

You know, not all anterior colporrhaphies are created equal. What we do, and what the next gynaecologist will do using chromic, which is put in a couple of Kelly type sutures, that's not an anterior repair . . . (USG1.1;5:39)

One has to accept that no two surgeons do the same procedure exactly the same. We're not automatons and we're not robots . . . In reality an anterior repair is as individual as the individual that tackles it; wherever you get variables like patients, it's not like mending your TV, where it's the same procedure, you take your bit out and put another bit back, the same bit every single time. It isn't like that. It never will be. (G2.1;9:15)

This individuality poses a problem for EBM as it highlights the problem of internal validity. Just as it is difficult to reconcile the aggregate results of a trial to a specific case, it is difficult to average the individual acts of individual surgeons. For surgeons, comparisons between different pairs of surgical hands were highly problematic:

The thing is that you are bringing up a very important point: what are all these things like in the hands of an average person? And is there an operation that is better in the hands of an average person? That I think is very very difficult to assess, the reason it's difficult is there are so many variables. (USU2.1;15:47)

Opportunities to observe others operating served only to make surgeons more aware that their own practices were at variance with those of other surgeons, as these two American surgeons explained:

I went and visited M at [hospital] who did most of his fellowship at the [specialist centre] and they actually have a different approach to vaginal surgery than F. (USG2.1;7:40)

It's interesting. Actually I started in residency learning how to do Burch procedure. I went to a residency where we have about 60 different private physicians that I operated with at one time or another so I saw a tremendous variation of ways to do things, and I scrubbed on a couple of Marshalls, but basically people did Burch procedures. When I went out to do my scholarship, they do a specific procedure in the [specialist centre] which is the Z modification of the MMK procedure. What they do is unique. (USG3.1;4:48)

Personal experience and this variation between surgeons provided a justification for rejecting scientific evidence propounded by the published literature:

Whatever they say is coming from various studies, the fact remains that a surgeon might get better results with one particular procedure because he's just good at it, so it doesn't represent the true difference in results for the procedure as such. (G3.1;4:3)

They [two experts in published article] are putting permanent suture on the Burch colposuspension. They are using four sutures and they are probably, I don't know what their anterior repair was, it is so variable. I don't know what an anterior colporrhaphy is. I have no idea. Most people tell me that's exactly what they do. (USG1.1;4:53)

When you have a hands on approach to patients it is different than they write in the literature. (USU2.1;2:20)

The surgeons' problems with using evidence of the type embodied in the literature is not only related to problems of internal validity. Inconsistencies or apparent conflicts within the literature served to heighten distrust of such evidence. As Wood et al. (1998) have pointed out 'evidence' is often discussed as if it is distinct and fixed, but all too often it is ambivalent or confused. The surgeons recognized surgical controversy and the inconclusiveness of much of the research on surgery and used it to defend their rejection of 'science':

I'm not very consistent on that. Every time I read a new article in a journal I get confused and so sometimes I'm more pro than others. At the moment I'm fairly pro, so this patient did have gentamicin. (U5.1;2:10)

The presence of conflicting evidence encouraged surgeons to fall back on the one thing they 'really knew' – their own experience. In the last instance, experience was the arbiter of evidence:

You do things in a way that you think is right, all consultants tend to do this, and whether or not techniques are perpetuated depends on the judgement of the juniors as to whether they work or not . . . if you think someone's technique is good you use it, or if you don't think too much of a particular technique you don't use it and you find another one. (G1.01:6:38)

EBM as a site of occupational conflict

The surgeons' accounts of the instinctive and individualized nature of their work can be used to provide 'good' reasons for rejecting research evidence. In talking about surgery they stressed the experientially based, practical knowledge required for surgical work. Their accounts invoke ideas about the tension between art/science, experiential/technical knowledge and practice/theory. These dualisms have long been central to philosophy (see, for example, Ryle, 1945; Polyani, 1961; Oakeshott, 1991) and in sociology, Jamous and Peloille (1970), Knafl and Burkett (1975) and Nyiri and Smith (1988) have drawn on these distinctions to understand medical practice. In addition, much of Freidson's work has explored the notion of clinical judgement as an experientially based, tacit knowledge at the heart of medical work, and quite distinct from the formal knowledge offered by science (1986, 1988). Freidson argued that clinical judgement has been an important source of power for the medical profession, legitimating its autonomy and dominance and ensuring freedom from external interference. However, as he was quick to point out, there is certain vulnerability in this position at a time when politicians, payers and the public demand accountability from the professions and medical work is increasingly the subject of external scrutiny. A reliance on tacit, nebulous knowledge means that one cannot use explicit, formally specified knowledge to defend work practices.

It also separates medicine from the world of science and all the kudos that entails. By privileging technical knowledge that can be formulated and specified, and which is allied so closely with science (albeit with a rather narrow experimental version of science) EBM thus presents a significant threat to clinical judgement and ultimately control over medical work.

The EBM movement has constructed and defined the nature of medical work as technical and has emphasized the scientific aspect of the art/science dualisms presented earlier in this article. Social movements advance or create a 'frame' as a way of organizing ideas to challenge established models or positions (Snow and Benford, 1988). As a reform movement, EBM has provided a formulated, explicitly rational frame that directly challenges the experiential model of practice offered in these surgeons' accounts. From a social movements perspective, what is fascinating about this challenge to professional dominance is that it has not come wholly from outside the profession. Although undoubtedly used by external parties, such as state regulators, policy makers and insurers, the EBM reform movement has been driven to a great extent by a segment of the medical profession. EBM provided leverage and prestige to clinical epidemiology, shifting power to a segment that traditionally has not held high status in medicine. It gave prominence to academic rather than 'hands on' medicine. This shift was substantial and rapid.⁵ Much of the debate about EBM has focused on its role in external challenges to professional dominance, yet this internal struggle for power is also worthy of consideration.

Social movement theories and ideas about professional segmentation provide a way of looking at EBM as a site of intra-professional conflict. Both the surgeons' accounts presented here and the wider anti-EBM literature can be characterized as discourses of occupational resistance. In resisting 'evidence', surgeons and other clinicians use the 'art of medicine' rhetoric and ideas about individual versus collective, defensively. Their emphasis on experiential knowledge has strong parallels with the attempts by other, different occupational groups, to resist rationalization and the formal specification of work practices (see, for example, Fine's work on cooks (1996) or Barley (1996) on technicians). The reasons for this seem obvious. Not only can technical work practices be subjected to external control and surveillance, they can also be routinized. Routine work is susceptible to standardization and may be repetitive and boring. It also loses its prestige and mystery and, thus, power.

On one level, it is possible simply to view the data on surgeons presented here as segmental resistance to the EBM reform movement – a 'last stand' by a previously dominant group. Some of the resistance to EBM has undoubtedly been concerned with maintaining power in the face of external threats from regulators and the like. But there seems to be more to it than that, at least for these surgeons. Contingency appears central to much surgical work. Freidson has argued that it is a component of wider medical practice (1986). Embodied, tacit knowledge allows the individual surgeon

to manage each individual patient and to deal with contingency. The rational-technical thrust of EBM means that it has not addressed the contingent nature of medical practice. The problem facing the EBM reform movement is that it has asserted the dominance of a particular branch of science and has been reluctant to recognize other equally vital components of medical knowledge. In setting itself apart as a reform movement around this technical knowledge, EBM has thus sown the seeds of its opposition which lie in everyday, contingent, clinical practice. Blumer's analysis of social movements reminds us that there is no inevitable linear progression in the life of a movement towards the achievement of its goals. The success of a reform movement is itself contingent on the dynamic relationships between segments and particular contexts. Looking at the evolution, thus far, of the EBM movement, its project remains problematic.

Concluding remarks

This is not to say that evidence has no place in medicine. Scientific, technical knowledge drawn from research clearly must inform practice. The difficulty for the EBM movement – despite changing the nomenclature to evidence-based *practice* – is that it has helped to create and sustain the idea that evidence and practice are diametrically opposed. As Wood et al. (1998: 1736) point out, the complex and dynamic nature of everyday medical work means that it is vital that evidence is balanced with practice. Within the terms of the EBM debate, much has been made of the regulatory potential of EBM for external bodies such as managers and administrators. Resistance to EBM by clinicians has often been dismissed as protectionist, and within sociology there is a danger that such resistance will only be analysed as an attempt to maintain professional dominance in the face of external threats. Theories of social movements and professional segmentation provide a way of analysing the internal dynamics of this struggle and may help us to explain the wider EBM project and why clinicians continue to resist EBM.

Notes

1. Research by Pawluch (1983), and Halpern (1990) in pediatrics is a notable exception.
2. The scope of this article does not extend beyond Blumer's work on social movements, and Hammersley (1989) provides an excellent critical consideration of Blumer's methodological contribution.
3. Although, as Dingwall et al. (1998) have pointed out, Cochrane's ideas have been used somewhat selectively in the debate about EBM. Cochrane made it clear that there are areas of medical care where the RCT is an inappropriate or unsuitable method. More importantly, he suggested that while the RCT can measure effectiveness its results may not be directly replicable in clinical practice (Cochrane, 1972: 2).
4. Sackett and Rosenberg's 1995 article was published in several journals, a

practice usually discouraged by journal editors. The multiple publication of this article was criticized in a letter by Holdsworth and Crampin (1996) *Health Economics*, 5, 271–2, which also printed a defence from the authors.

5. As Willis and White (1999) have pointed out, the rise of clinical epidemiology on the back of the EBM movement mirrors the rapid ascendancy of Genito-Urinary Medicine following the emergence of AIDS.

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