



# Insufficient evidence: the problems of evidence-based nursing

Gary Rolfe

Evidence-based medicine was first proposed in the early 1990s as a means of integrating clinical expertise with the best evidence from research. It has recently gained a foothold in nursing, where despite calls for a broad and nursing-oriented definition of what should count as evidence, it appears to be propounding the randomized controlled trial (RCT) as the gold standard.

This paper challenges the wisdom of basing nursing practice on the findings of large-scale statistical research studies, and offers a number of logical objections to the underpinning philosophy of evidence-based nursing and the randomized controlled trial. It concludes by arguing for a rethinking of what should count as evidence, and suggests a quasi-legal model based on reflection rather than research, in which evidence is employed to understand and justify practice after the event rather than being used deterministically to plan practice in advance. © 1999 Harcourt Publishers Ltd

## Introduction

There have been periodic calls since the 1970s for nursing practice to be based on research findings (DHSS 1972, Hunt 1981, DoH 1991, DoH 1993), although recently there has been a subtle shift in rhetoric from *research*-based to *evidence*-based practice. This shift originated in the medical profession with the Evidence-Based Medicine Working Group (EBMWG 1992), and is now spreading to nursing and other practice-based disciplines. Evidence-based medicine (EBM) has been defined as:

...the conscientious, explicit, and judicious use of current best evidence in making decisions about the care of individual patients. The practice of evidence based medicine means integrating individual clinical expertise with the best available external clinical evidence from systematic research. (Sackett et al. 1996)

Despite this acknowledgement of individual clinical expertise, the medical profession has usually interpreted EBM as the application to practice of the findings from large-scale quantitative research. Similarly in nursing, many writers have adopted the terminology and rhetoric of evidence-based practice without embracing its full consequences. To take just two examples published in the week of writing this paper, Nolan et al. (1998), in an article entitled Evidence-based care: can we overcome the barriers?, spent the entire paper discussing the barriers to *research*-based care, with neither the word 'evidence', nor any of its other components, appearing after the second paragraph. Similarly, Thompson (1998) took exactly the same approach in his paper 'Why evidence-based nursing?'. Part of the difficulty might be that, whilst evidence-based practice 'is about integrating research evidence with clinical expertise, the resources

**Gary Rolfe** PhD, MSc, BSc, RMN, PGCEA, Principal Lecturer, School of Health and Social Care, University of Portsmouth, St James Hospital, Lockaway Road, Portsmouth PO4 8LD, UK

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available and the views of patients' (Thompson 1998), these latter sources of evidence appear well down in the hierarchy (Rosenberg & Donald 1995). As White (1997) pointed out, 'The best evidence is thought to be that obtained from controlled experimental work, and the least value is attached to authority and clinical experience of the practitioner'. Mulhall (1998) has responded to this devaluing of 'soft' forms of evidence, arguing that: 'tiptoeing in the wake of the movement for evidence-based medicine, however, we must ensure that evidence-based nursing attends to what is important for nursing.' Thus, she advocated not only quantitative research but qualitative research; not only large-scale RCTs and meta-analyses, but small-scale ethnographies and grounded theory studies; not only a focus on outcomes which are easily measured, but also on the complex day-to-day issues of nursing practice. In short, nursing must adapt EBM to its own ends.

However, whilst Mulhall's sentiments are to be commended, there is little evidence that they are being acted upon. For example, shortly after Mulhall's paper was published, French (1998) defined evidence-based practice in nursing as 'the process of systematic identification, rigorous evaluation and the subsequent dissemination and use of the findings of research to influence clinical practice', with the gold standard being the RCT. This position, ironically, is even more extreme than that advocated in medicine, which does at least acknowledge the role of clinical expertise.

If we accept Mulhall's recognition of the differences between nursing and medicine, then it is likely that we will run into difficulties if we attempt to base nursing practice on the model advocated above by French. But even supposing that it is possible to adapt EBM to the needs of nursing, it is still perhaps relevant to question whether it is in our best interest (and more importantly, that of our patients) to do so.

This paper will argue that evidence-based medicine is not fulfilling Sackett's promise of 'integrating individual clinical expertise with the best available external clinical evidence' (Sackett et al. 1996), and that its application to nursing and midwifery has seen a regression back to what Schön (1983) described as the technical rationality paradigm of practice based solely on the findings of quantitative research.

This argument will be developed by critically examining the espoused philosophy of EBM for logical and practical inconsistencies, and will explore the work of French (1998) in nursing and Phillips (1994) in midwifery as exemplars of the extreme technical rationality interpretation of evidence-based practice for nursing and midwifery. The paper will conclude by suggesting that evidence-based nursing and midwifery might be more appropriately formulated on a model of reflective practice rather than on a model of research-based practice.

## **Five objections to evidence-based practice**

### **1. The two faces of evidence-based medicine**

Evidence based medicine was 'launched' in 1992 as a new paradigm for medical practice (Evidence Based Medicine Working Group 1992), and quickly acquired the kind of sanctity often accorded to motherhood, home, and the flag (Feinstein & Horwitz 1997). Its presentation has been carefully engineered to appeal simultaneously to traditionalists and progressivists alike (and, arguably, to these two tendencies within individual practitioners) by claiming, on the one hand, that 'the use of evidence in medicine is certainly not new' (Davidoff et al. 1995), whilst on the other, that 'the change ... is profound enough that it can appropriately be called a paradigm shift' (EBMWG 1992). Similarly, the proponents of EBM have been careful to emphasise the importance of 'traditional' professional judgement, such that:

...external clinical evidence can inform, but can never replace, individual clinical expertise, and it is this expertise that decides whether the external evidence applies to the individual patient at all and, if so, how it should be integrated into a clinical decision. (Sackett et al. 1996)

However, this reassurance stands in contrast to other statements such as: 'Evidence-based medicine de-emphasizes intuition [and] unsystematic clinical experience ... and stresses the examination of evidence from clinical research' (EBMWG 1992). Evidence based

medicine is, at the same time, selling itself as 'more of the same, only better', and as a radical break with the past, a new paradigm (indeed, the Evidence-Based Medicine Working Group explicitly invoked Thomas Kuhn's notion of a paradigm shift, which Kuhn (1962) described as a Gestalt switch and a scientific revolution).

This dual aspect is echoed in nursing and midwifery, with Mulhall's vision of evidence-based nursing 'grounded in experiential knowledge gained from being a nurse, and doing nursing' (Mulhall 1998) standing in contrast to the view by Phillips (1994) that 'optimal midwifery care can only be achieved through research-based theoretical knowledge and clinical practice'. In support of this interpretation of the only acceptable form of evidence as coming from research findings, French (1998) complained that treatment may not be offered on the basis of what is effective for a given health care problem, but according to the preferences of clinicians'.

French and Phillips, it would appear, are offering none of the reassurances of their medical colleagues that clinical expertise will continue to be valued: optimal care can *only* be achieved through practice based on research, and clinical expertise is relegated to being merely 'the preferences of clinicians' (French 1998). For French and Phillips, then, the only valid evidence is derived from research, and as we have seen, they take the term 'evidence-based practice' to refer almost entirely to the process of applying research findings to practice, particularly the findings from randomized controlled trials. In medicine, the inconsistencies in the presentation of EBM as simultaneously based on clinical judgement and on research findings have at least been recognized, and to some extent exploited. Nurses and midwives, however, have yet to reach a consensus even on what evidence-based practice is, let alone how it should be done.

## **2. The limitations of statistical generalization**

Feinstein & Horwitz (1997) have highlighted this duplicitous presentation of evidence-based medicine, claiming that, beneath the rhetoric, it is little more than the application of findings from Randomized Controlled Trials (RCTs) and meta-analyses, and concluding that it could ultimately result in 'a new form of dogmatic

authoritarianism'. From this perspective, then, it could be argued that nursing and midwifery writers such as French and Phillips have seen through the presentation of EBM as based on clinical expertise, and that Mulhall is being rather naive and gullible in supposing that evidence-based medicine is any more than practice based on the findings of RCTs.

However, Feinstein and Horwitz (1997) take the argument further: their objection is not simply that EBM is making false claims about its identity, but that its true identity as a front for the Cochrane Collaboration promotes research findings which are of little use to practitioners. Findings from RCTs, they argue, result in data about the average patients' and 'were not intended to answer questions about the treatment of individual patients' (Feinstein & Horwitz 1997). Thus, whereas average results obtained from RCTs are useful for 'policy makers and pharmaceutical manufacturers', for making decisions about whether, on average, one treatment is better than another, they are of little value to practitioners faced with specific clinical decisions about unique individuals, and cannot fulfill the promise made by Sackett et al. (1996) that EBM can be employed in 'making decisions about the care of individual patients'.

This is something which has long been known by most practitioners, and even by some academics and researchers. For example, the DoH guidelines on detecting people at risk of suicide, highlighted a number of demographic and clinical factors associated with increased suicide risk, before acknowledging that:

All these factors are well-known statistical correlates of suicide and must not be ignored. They do, however, present problems in the day-to-day clinical situation. Many individuals will possess these characteristics yet not commit suicide, and suicide can occur in people of very different characteristics. (DoH 1994)

What, then, is the confused practitioner to do? The guidelines continue:

Suicide risk in any individual can only be assessed effectively by full clinical evaluation consisting of a thorough review of the history and present illness, assessment of mental state and then a diagnostic formulation.

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This view that what practitioners require is specific knowledge about individual patients rather than general knowledge about trends and populations runs counter to the explicitly stated philosophy of EBM that:

the concept of a single study, *although it may provide the truth*!, is often not enough. The whole truth may require a synthesis of the evidence from all the best studies, optimally through the use of meta-analysis. (Davidoff et al. 1995, my italics)

Similarly, French (1998) argued that nursing practice should not be based on individual studies, but that:

...findings from individual studies can be weighted in relation to the quality of the research [bearing in mind her earlier statement that the gold standard is the RCT], and combined using statistical techniques such as meta-analysis and DiCenso et al. (1998) added:

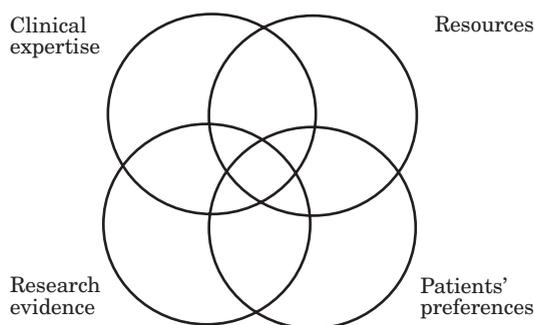
The statistical combination of the results of more than one study, or meta-analysis, effectively increases the sample size and results in a more precise estimate of treatment effect than can be obtained from any of the individual studies used in the meta-analysis.

These sentiments that big is beautiful, that by using larger and larger samples we can come to know everything about everyone, is, as Feinstein and Horwitz (1997) pointed out, based on the optimistic assumption that all the data being synthesized are of the same high quality. But it is also *logically* flawed, in terms of what can meaningfully be inferred from statistical generalizations. There are, in fact, two logical weaknesses in the argument: the first is in the inductive generalization of findings from a sample to a population; and the second is in the deductive application of those general findings to an individual.

Starting with the issue of generalizing from a sample to a population, Davidoff et al. (1995), French (1998) and DiCenso et al. (1998) are assuming that the bigger the sample, the more accurate the findings, such that the 'whole truth' can only be arrived at by pooling samples in a meta-analysis. However, one of the problems of sample selection is in choosing a sample with just the right degree of homogeneity. If the sample is

too homogeneous (for example, including only women aged 20–30), then the data are very precise, but can accurately be applied to only a narrow strata of the intended population. If, on the other hand, the sample is too heterogeneous (for example, everyone aged between 10 and 80), the data are relatively imprecise, but can be applied to a much wider group. The claim made above by DiCenso et al. that meta-analyses tend to increase the precision with which estimates of treatment effects can be made is clearly contentious. In fact, the danger is that, as the sample is increased with the addition of more and more studies, we come to know less and less about a larger and larger population, until we eventually know nothing about everyone. The strategy advocated above by Davidoff et al. of pooling a number of statistical studies in order to arrive at 'the whole truth' therefore tends to have the opposite effect of *diluting* the truth, if by truth we mean the best information to make clinical decisions about well defined groups.

But once we have made the statistical generalization from a sample to a population, we are now faced with the problem of applying those general findings to individual patients. Unfortunately, there seems to be a widespread misunderstanding amongst practitioners and researchers alike over exactly what statistical generalizations can tell us about individual patients. The finding, for example, that nursing intervention A produces a better outcome than nursing intervention B, gives us no indication *whatsoever* about the relative merits of the two interventions for patient P. What it tells us is that, if we pick 10 patients at random, then (say) eight of them are likely to respond better to intervention A. But that is not to say that if we randomly choose one patient from those 10, that the chosen patient has an 80% chance of responding better to intervention A. In fact, from the information we are given, we can say *nothing* about which intervention patient P is most likely to respond to, any more than we can say, given the results of an opinion poll that 80% of the population will vote Labour, that voter V has an 80% chance of voting Labour. Each individual voter has her own individual probability of voting Labour, just as each individual patient has her own individual probability of responding to intervention A, and we can only discover what it is by coming to *know* the patient as a unique and



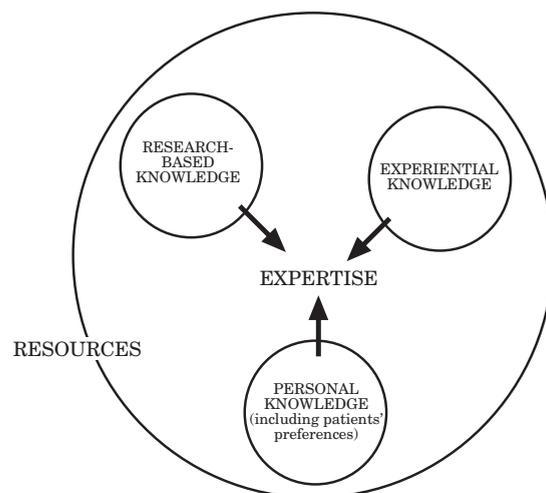
**Fig. 1** A model for evidence-based clinical decisions (after DiCenso et al. 1998).

individual person. As the writers of the guidelines on the assessment of suicide risk discovered, statistical data can provide us with useful information on the macro level of whole populations, but not on the micro level of individual patients.

### 3. The nature of expertise

Advocates of EBM might object at this point that this critique is placing too much emphasis on the application of external evidence and not enough on the expertise of the practitioner; after all, as Sackett et al. (1996) pointed out, 'good doctors use both individual clinical expertise and the best available external evidence, and neither alone is enough'. But this statement is misleading: it is what the philosopher Gilbert Ryle (1963) referred to as a 'category mistake'. Ryle gave an example of a category mistake with the story of a visitor to Oxford who did not realize that the University was the sum total of the various colleges. Just as, in Ryle's example, it makes no sense for a visitor to Oxford to say 'I've seen all the colleges, now show me the University', so it makes no sense for the practitioner to say 'I've applied the external evidence, now I'm going to use my expertise'. Evidence and expertise belong to two different logical categories, and whilst external evidence (in the form of research papers) is a tangible object which can be waved in the air and banged on the table, clinical expertise is a process which Sackett et al. (1996) described as proficiency and judgement.

Much of the confusion arises over the indiscriminate and interchangeable use of the



**Fig. 2** An alternative model of expert decision-making.

terms 'experience' and 'expertise' throughout the EBM literature. An experienced practitioner is someone who has accumulated a body of experiential knowledge from many years of practice. But like any knowledge, experiential knowledge can be used poorly or wisely, and there are many practitioners with 20 years or more experience who fail to make good use of it. What separates the experienced nurse from the expert is the *wise application* of experiential knowledge. The expert does not apply her experiential knowledge indiscriminately, but forms a professional clinical judgement based on experiential knowledge, research-based knowledge, and personal knowledge about each individual patient.

Clinical expertise is therefore the process by which the practitioner formulates a wise professional judgement which involves 'the *integration* of a variety of facts regarding a patient's condition with an extensive store of medical knowledge' (Kassirer et al. 1988, *my italics*). Sackett et al. (1996) are confusing the *process* of expertise with the *content* of experiential knowledge in suggesting that we can employ clinical expertise without external evidence or vice versa, since the *process* of clinical expertise entails the application of the *content* of best evidence.

DiCenso et al. (1998) made a similar category mistake when they presented a model of evidence-based nursing decisions comprised of the four components of clinical expertise, patient

preference, clinical research evidence and available resources (Fig. 1). This model places clinical expertise in the same logical category as research evidence and even resources, in which any one component can take precedence over all the others.

In keeping with the alternative view of expertise suggested in this paper, we might reconceptualize the model of clinical decision-making as the *process* of expertise which combines scientific, experiential and personal knowledge within the constraints of available resources (Fig. 2).

This distinction between experience and expertise is more than mere semantic nit-picking, since the traditional EBM model from Figure 1 leads to such statements as:

Time was when expert opinion – authority – carried as much weight as the clinical scientific record, and often more ... But practising physicians increasingly expect, and are expected, to base their decisions on the evidence rather than on authority.  
(Davidoff et al 1995)

although

...clinical expertise and patient preference may override the other components of the model for a given decision. (DiCenso et al 1998)

Is Davidoff really rejoicing in the decline of *expertise* or of clinical decisions based on *experience* rather than research? And is DiCenso really arguing that nurses should only sometimes rely on their expertise, or does she mean that, when formulating an expert decision, clinical *experience* sometimes overrides science?

If we accept that experience and expertise are not interchangeable terms, but belong to different logical categories, then it would seem that, despite Sackett's claim for a new paradigm, evidence-based medicine really is nothing new. What has changed is, as Davidoff et al. (1995) pointed out, 'the very nature of clinical evidence itself'. The *process* of clinical expertise is largely untouched by the so-called 'paradigm shift' of EBM; it is just that the emphasis on what counts as evidence has shifted from the internal store of experiential knowledge which every practitioner carries around with her to the external evidence from research journals. In the terminology of Karl

Popper (1973), the emphasis has shifted from World 2 (internal, subjective) knowledge to World 3 (external, objective) knowledge.

#### **4. The evidence for evidence-based medicine**

A fourth objection to EBM is that it is not only logically *mistaken*, but logically *inconsistent*, in as much as it violates its own criteria for adoption into practice. If all practice is expected to be based, where possible, on the 'gold standard' of RCTs, then any practitioner considering EBM as the foundation for her practice might reasonably ask to see the RCTs on which it is founded. But, of course, there are none: EBM was launched, not following the recommendations of a research study (controlled or otherwise), but as a position paper, a statement of belief. The original proponents of EBM clearly recognized this, claiming that, until evidence is gathered for its effectiveness, adoption of EBM should be restricted to two groups of practitioners: those who '*believe* that use of the evidence-based medicine approach is *likely* to improve clinical care'; and those who '*believe* it is very unlikely that deterioration in care results from the evidence-based approach and who find that the practice of medicine in the new paradigm is *more exciting and fun*' (EBMWG 1992, my italics).

This is a quite remarkable statement from an eminent group of medical researchers and practitioners, particularly a group which emphasized the importance of practice being based on sound research evidence. What they are saying, in effect, is that the only groups of practitioners who should base their practice on external evidence are those who *believe* that this approach is likely to improve patient care and those who find this approach to be more exciting and fun! It would appear, on these criteria, that we might advocate that practitioners treat all their patients whilst standing on their heads, claiming that, whilst waiting for the research evidence for its effectiveness, it can still be carried out by those practitioners who believe it to be effective and those who find it to be fun. And, of course, the RCT which shows that standing on your head improves practice is no less likely to be forthcoming than the RCT which shows that evidence based practice is effective. As the EBM Working Group (1992) itself admitted: 'This proof

is no more achievable for the new paradigm [of EBM] than it is for the old, for no long-term randomized trials of traditional and evidence-based medical education are likely to be carried out'.

Phillips tied herself in similar knots when advocating research-based midwifery practice. She claimed that 'optimal midwifery care and consequent personal and professional pride and job satisfaction *can only be achieved* by being aware of – and enacting – research in practice' (Phillips 1994), and backed up this statement by citing such eminent bodies as the International Confederation of Midwives, the UKCC and the Department of Health. She continued by warning of the dire consequences of not basing practice on research, such that:

putting new 'folklores' into practice ... without first establishing their efficacy or potential harm through sound research, runs counter to the notion of good, reflective, research-based midwifery practice and should not be adopted or condoned. (Phillips 1994)

What Phillips singularly failed to acknowledge, or even recognize, is that research-based practice, the notion that optimal care can only be achieved by basing practice on research findings, and in particular on RCTs, is itself a prime example of a 'new folklore'. Whilst she is worried by the '*belief* of some midwives that midwifery research lacks value' (Phillips 1994, *my italics*), she does not recognize that the view that research *has* practical value is itself no more than a belief which is not supported by research.

## 5. The educational imperative

Given the rather shaky foundation on which EBM is built, and the extreme caution suggested by its advocates regarding its uptake, it is perhaps alarming that it appears to be exerting such an influence not only on practice, but also on education. In fact, it would appear that, rather like the man who tried to pull himself up by his own bootstraps, we are faced with a logical *impasse*: in order to evaluate EBM, we must first introduce it; but, by its own criteria, in order to introduce it, we must first evaluate it. Thus, the evolution of EBM:

...will be enhanced as several undergraduate, postgraduate, and continuing medical education programmes adopt and adapt it to their learners' needs. These programmes, and their evaluation, will provide further information and understanding about what evidence based medicine is and is not. (Sackett et al 1996)

What Sackett et al. appear to be saying here is that in order to determine whether EBM is effective, we must first initiate a large-scale educational programme so that we can evaluate whether students who have been taught in this way provide better medical treatment and care. And what if they do not? Despite the authors' earlier caution, this hardly seems to be considered as an option; in any case, 'evidence based medicine ... will continue to evolve' (Sackett et al. 1996). This 'turn to research' by medical education can therefore be seen not as an attempt to *develop* best practice through EBM, but rather as part of a strategy to *test* whether EBM actually does produce best practice. Educationalists and practitioners alike are being used as guinea-pigs in a large-scale quasi-experiment whose outcome is uncertain.

The emphasis which EBM places on the skills of retrieving and reviewing the relevant research literature is already having a particularly strong influence on postgraduate education across all the health care disciplines, and has led to the instigation of a large number of research-based courses across the country, spearheaded by the Government-funded Research & Development Support Units. This growing trend towards teaching the skills of research critique rather than research practice is now beginning to gain acceptance in nursing, and follows on from the recommendations of the Department of Health that 'research, done properly, is a highly professional and specialist activity and not suited to every practitioner; but every practitioner needs to be involved in using the results of research' (DoH 1995). Thus, French (1998) argued against the current approach to teaching research 'by participation in the research process' in favour of teaching the skills of accessing and evaluating published studies, although it could, of course, be argued that the ever-growing body of published meta-analyses obviates the need for practitioners to possess these critical skills.

Despite French's assertion of the growing need to teach the critical appraisal of research rather than how to do research, she rather misses the point of the established approach to teaching research through participation in the process. The emphasis is often not, as she implies, 'on the creation of new knowledge', but rather on understanding through doing, such that 'we learn by doing and realising what came of what we did' (Dewey 1938). This, of course, is such a basic educational principle that it hardly needs stating: that in order to fully understand, and hence to critique, a form of practice, we must first gain experience of that practice. Many (perhaps most) nurses would object to their clinical practice being assessed by someone who had never practised as a nurse, and yet French is suggesting that people who have never undertaken a research project are able to offer an informed critique of the research practice of others. We should resist the notion that research is a purely technical procedure which can be appraised by non-researchers, as vigorously as we would (I hope) resist the notion that nursing is a purely technical procedure which can be appraised by non-nurses. As Schön (1983) pointed out, *all* practice is a messy business which can only be fully understood and appreciated by other practitioners.

### **Towards a new model of evidence-based practice**

In considering the relevance of evidence-based practice, the question that every experienced practitioner should ask herself is this:

Faced with a clinical situation in which the external evidence of a RCT clearly and unequivocally indicates that intervention A produces the best outcome for the 'average patient', but where my clinical experience and knowledge of patient P suggests intervention B; which should I choose?

The advocate of evidence-based practice as it has been described above would *have* to advise the practitioner to choose intervention A. To do otherwise would be tantamount to saying that we should only practice according to the best external evidence when that evidence agrees with our internal experiential and personal knowledge, and it is then but a short step to

doing without external evidence altogether. Thus, DiCenso et al. (1998) suggested that:

History has shown numerous examples of healthcare interventions which, on a patient by patient basis, might appear to be beneficial, but when evaluated using randomised trials have been shown to be of doubtful value, or even harmful.

For DiCenso, the evidence from a RCT (intervention A) outweighs the evidence from individual practice (intervention B).

But in choosing intervention A, the practitioner is rejecting the very sources of knowledge which define her expertise. If we view expert practice as described in Figure 2 above, as the process which integrates and synthesizes relevant scientific research-based knowledge from books and journals, experiential knowledge from the practitioner's own past experiences and the experiences of others, and personal knowledge of individual patients and of herself (Kassirer et al. 1988, Clarke et al. 1996, Rolfe 1996, 1998), then in suggesting that scientific knowledge obtained from the 'gold standard' of the RCT should take precedence in determining good practice, there is a very real danger that these other valid and important sources of knowledge will be underemphasized or completely overlooked. In DiCenso's example above, the personal and experiential knowledge gained 'on a patient by patient basis' is rejected in favour of the scientific knowledge gained from the RCT.

If we take DiCenso's advice literally, evidence-based practice not only rejects expertise, but promotes its very opposite. Good nursing practice (in terms of the application of the findings of research) is to be determined by researchers who may never have practised nursing themselves. Furthermore, the performance of these researchers (in terms of the soundness of their methodology) is to be judged by nurses who may never have practised research themselves. As the educationalist Lawrence Stenhouse (1978) pointed out:

...without understanding why one course of action is better than another, we could prove by statistical treatment that it is. The vision is an enticing one: it suggests that we may make wise judgements without understanding what we are doing.

The imperative of evidence-based practice, at least in the crude form advocated by French, Phillips, and a host of others, suggests that researchers can make wise judgements about nursing and nurses can make wise judgements about research from a purely theoretical knowledge-base. But of course, Stenhouse was making the point that these 'wise judgements' are not as wise as they might first appear. As Benner (1984) argued, practice based entirely on scientific research findings is novice practice; experts practice from experience, and only revert to 'book knowledge' when faced with a novel situation. Thus, as Gadamer (1996) noted:

Once science has provided doctors with the general laws, causal mechanisms and principles, they must still discover what is the right thing to do in each particular case, and this is something which hardly seems to be predictable or knowable in advance.

### Evidence from reflection

One solution to this dilemma might be to reconsider what is understood by the term 'evidence'. As it is currently employed, it is taken to mean the knowledge-base on which practitioners make their clinical decisions. The difficulty with this definition is that, at the time of acting, the experiential and personal knowledge of the practitioner might not be consciously available, leaving only scientific research-based knowledge on which to make a judgement. That is not to say, however, that clinical judgements *cannot or may not* be based on experiential and practical knowledge, particularly the clinical judgements made by experts. Rather, as Benner pointed out, the expert practitioner might not be consciously aware either of the process by which she makes her clinical judgements or of the experiential and personal knowledge which informs those judgements *at the time she makes them*. It is only after the event, during reflection-on-action or clinical supervision, that the practitioner is able to identify the experiential, personal and scientific knowledge which went into the expert clinical decision. By insisting that practice is based only on the evidence which is consciously to hand at the time, there is a danger that evidence-based practice will be restricted to research-based practice, and that the largely

unconscious knowledge-base of past experiences which informs the 'intuitive grasp' of the expert (Benner 1984) will be ignored.

In order to overcome this difficulty, it might be more appropriate to reformulate our notion of evidence in line with the criminal justice system and the detective novel. Criminal evidence is not gathered in advance of a crime; rather, it is collected after the event in order to discover how and why the crime was committed. Similarly, if by evidence-based practice we mean practice which is *understood and justified* by evidence after the event rather than practice which is *determined* in advance by evidence, then the expert practitioner is free to make on-the-spot, unconscious 'intuitive' judgements which incorporate experiential and personal knowledge as well as research-based knowledge. To claim that her practice is evidence-based would then entail being able to justify her actions after the event, through critical reflection or clinical supervision, with all three types of knowledge being afforded equal status. Novice practitioners would still have to stop and consider the available research evidence before acting, but experts would be freed-up to:

perceive the situation as a whole, use past concrete situations as paradigms, and move to the accurate region of the problem without wasteful consideration of a large number of irrelevant options. (Benner 1984)

The evidence for clinical judgements would be uncovered through reflection-on-action, and evidence-based practice would be associated not with research-based practice, but with reflective practice.

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