Journal of Nursing Management, 2008, **16,** 486–493

Feedback

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Evidence-based practice: a debate

'Opposition is true friendship'

William Blake

Towards the end of a paper in this issue critiquing 'Best Practice' guidelines, Holmes and colleagues asserted that 'anyone who believes in the possibility of intellectual progress agrees that a prerequisite for such progress is frank and open debate'. Continuing in the same vein, they go on to pose the questions: 'Why is it that a discipline such as nursing finds it difficult to accept nuances and debates? Why is it that members of a discipline – particularly scholars – refuse to engage in philosophical, political and theoretical debates?' I can only agree that there appears to be a great deal of caution and resistance in the nursing academy towards debate, particularly when this involves specific critique of the ideas of named colleagues.

This month, the 'Feedback' section of the *Journal of Nursing Management* attempts to challenge this attitude. It comprises what I hope is a 'frank and open debate' in the form of a series of email exchanges between Roger Watson and myself on the philosophical, political and theoretical implications of evidence-based practice (EBP). Roger and I have exchanged views in various journals over the years and it is fair to say that our views about EBP do not exactly coincide. I hope that our discussion serves the purpose not only of initiating a wider debate about EBP in this section of the journal and elsewhere, but also acts as an example or model for further discussions on other topics. Most of all, however, I hope that this piece demonstrates the possibility of one-to-one critical engagement in a spirit of friendship and mutual exploration and learning. I would like to think that a critical discussion **about** evidence-based practice can also serve as a source of evidence **for** practice.

GARY ROLFE

Dear Roger

It is now 15 years since the first paper outlining the 'new paradigm' of evidence-based medicine was published (Evidence-Based Medicine Working Group 1992), although looking back, it feels like it has been with us for much longer. This might therefore be an appropriate point at which to review the current state and status of evidence-based practice (EBP) in nursing.

I find it quite remarkable how, within only 3 or 4 years, EBP had been embraced by the medical profession and has 'acquired the kind of sanctity often accorded to motherhood, home and the flag' (Feinstein & Horwitz 1997). What I find even more remarkable is how the nursing academy, eager I suspect to emulate their medical colleagues, seamlessly assimilated EBP into the dominant discourse of nursing with barely a word of dissent (but see, for example, Mulhall 1998, French 1999, Rolfe 1999, for some early critiques). My incredulity at the apparent hegemonic status of EBP is based on a number of concerns.

First, the original paper by the Evidence-Based Medicine Working Group (EBMWG) included the caveat that, until this self-styled 'new paradigm' had itself accumulated evidence for its own effectiveness, its application should be provisional, or what Rorty (1989) would term 'ironic', that is, in the full knowledge that it is based on belief rather than scientific proof. Eight years later, Trinder and Reynolds (2001) reiterated this caveat when they pointed out that:

> 'it has not escaped the notice of either critics or champions [of EBP] that there is not, nor is likely to be, any empirical evaluation of the effectiveness

DOI: 10.1111/j.1365-2834.2008.00868.x

> of evidence-based practice itself. The lack of any empirical justification for the approach has meant that advocates have relied upon intuitive claims' (p. 213).

To the best of my knowledge, the situation remains unchanged; there is still, as far as I am aware, no research evidence to support the claim that the practice of medicine or nursing which is based on research evidence is any more effective than that which is based on (say) intuition or reflection.

We might describe this situation as doubly ironic, in so far as a form of practice which places so much emphasis on the findings from research has itself no research evidence to support its effectiveness. Or, put another way, an approach which vociferously rejected the 'old paradigm' of practice based on the authority of key influential figures in the medical profession continues, after 15 years, to sustain itself on the authority of key influential figures in the academy. Hence, the irony: an approach which claims to challenge uncritical, unquestioning and unevaluated practice is itself (in my opinion) uncritical, unquestioning (at least of itself) and unevaluated.

My second concern is that the principal tenets of evidence-based medicine have been assimilated into nursing with very little consideration of the huge differences between these two spheres of practice. Whereas there are many medical and surgical interventions that might well benefit from interventions based on the findings from statistically generalizable research, it seems to me that nursing is, in essence, a series of unique and largely unpredictable interactions between unique and largely unpredictable individuals. The key tenet of evidence-based medicine, which is to base practice on what research has generally found to work best in most situations, might prove successful in, say, prescribing a drug for a known medical condition. However, it is logically flawed when it comes to working out how nurse A might build a therapeutic nursing relationship with patient B. Although a number of nurse academics have raised this point, most working definitions of evidence-based nursing still make reference to the randomized controlled trial and the systematic review as the highest form of evidence.

My third and perhaps most pressing concern is that the major political reason for introducing EBP in the UK was to standardize care; to ensure 'best practice' wherever and by whomsoever health care is provided. Apart from the philosophical question of whether 'best practice' is necessarily achieved through standardization (see Rolfe 2006), universally standardized practice requires a universally shared and understood definition of EBP. This, I would suggest, (Rolfe & Gardner 2006) is in reality far from the case. Rather, there appears to be little consensus on what counts as best evidence (or indeed, what distinguishes evidence from authority, opinion or intuition), how evidence is to be applied by practitioners and for which aspects of practice it is relevant.

GARY ROLFE

Dear Gary

I find a great deal to agree with in your summary of the state of the art in evidence-based practice in nursing. There is no doubt that anything, especially something so visible on the practice and political agenda, can be criticized and you do your usual trick of establishing that familiar species of canine, the straw dog and then suggesting it was time to take it to the vet for its final visit. There is no doubt that there are excesses regarding evidence-based practice. However, this has arisen because some of its most ardent supporters are ignorant of its precepts – they are called politicians. This has led to, what I would consider to be, a well-intentioned baby being steeped in particularly murky bathwater. But, to extend that analogy, you would have us discard the proverbial baby with the bathwater. I contend that the baby is good and that most of the bathwater must go.

You suspect that nurses have adopted evidence-based practice in emulation of our medical colleagues. Well, that would be a first! Almost since I have been involved in nursing – and that predates evidence-based practice – nurses have been revolting against the medical model and this has been especially demonstrated by the methodological focus on qualitative research and the eschewing, on the whole, of quantitative methods and the particular vilification of the randomized controlled trial. The randomized controlled trial lies at the top of the hierarchy of evidence and at the heart of evidence-based practice, so, I would be most surprised if nursing, in as much as evidence-based practice has been adopted, has been done to emulate our medical colleagues.

I totally agree with you that the evidence for the efficacy of evidence-based practice is not available. However, how would it be obtained? The phenomena, the alternatives against which it would be tested are, presumably, intuition, reflection and authority. The question, naturally, arises of whose intuition, whose reflection and whose authority? Are these not just acronyms, indeed euphemisms, for opinion and the logical question is whose opinion? Reflecting on my own days in practice I remember the great authority figures: the charge nurse and the consultant, and they were often wrong – demonstrably so. These, usually

unchallenged, authority figures, full of opinion masquerading as reflection but possibly a little short on reflection were often responsible for perpetuating practices that flew in the face of common sense, knowledge of biology and the developing evidence base. I realize that 'common sense' is a dangerous term to introduce to the debate but why, for example, did we ever think that egg white dried off with oxygen would be helpful in treating pressure sores? And it is no coincidence that wound healing, including pressure sores, has been the focus of a great deal of evidence-based activity. Nevertheless, I concede that this is a relatively easy area to study and may not be truly representative, with clear treatment strategies and a very obvious outcome, of a great deal of the interstitial and almost invisible and complex work of nurses.

On the other hand, I sense a 'get out clause' in your assertion that nursing is so complex that it is not possible to study it through the paradigm that evidence-based practice would apply. In other words, the quantitative, reductionist and experimental approach is less applicable. I would contend, however, that this is based on a 'found difficult and not tried' mentality rather than a 'tried and found wanting' one. This 'get out clause' is familiar from the homeopaths and alternative medical practitioners who also claim that their methods cannot be tested by conventional (i.e. scientific) methods. But there are those who would say 'why not?' and why should we use them if they cannot be (Watson & Deary 2003)? If something purports to be effective and has a stated intervention (a treatment) and a desired outcome (the patient gets better) then why is that not amenable to testing? The same applies to nursing. Yes, it is difficult to test what nurses are doing, especially when nurses develop therapeutic relationships which are different depending on the patient. But wait! That would surely be the equivalent of a doctor saying that it is impossible to test medical interventions because patients are all different – some patients have infections and some are in pain and there is a range of medicines available to treat them and it is just too difficult even to think about sorting out the mess. We have to do better than that in nursing, whether it leads to the accusation of emulating of our medical counterparts or not, and if the randomized controlled trial is not the best way to test nursing then let us strive to develop methods that are suitable – I find plenty to criticize in the randomized controlled trial and it should not be assumed that those of us who advocate evidence-based practice are welded to the randomized controlled trial. Rather than losing the baby of evidence-based practice, some are working to develop new hierarchies of evidence and ways of aggregating qualitative approaches (Evans 2003, Finfgeld-Connett 2007).

The danger in your approach to evidence-based practice in nursing or evidence-based nursing, is that we are in danger of undermining nursing itself. If nursing is all about the untestable in pursuit of the invisible then why bother with it? It has to be said at some point that, without an evidence base for nursing, how do we advocate it – assuming that we do advocate it – in a world of increasing demand and diminishing resources for health care? You would have us stem the tide of evidence-based nursing at its source and I argue that we should let it thrive. The problem is not the attempt to produce evidence and then seek the best; the problem is that the vast majority of health care and nursing in the UK takes place within a Stalinized monolithic National Health Service where other well-intentioned initiatives such as NICE lead to excesses of state control over the very length of our lives through the restriction of demonstrably effective treatments. Finally, on the basis of evidence, which lies at the heart of criminal conviction system, I would ask you, would you rather be convicted – or freed – on the basis of evidence or intuition? I concede that evidence can sometimes mislead but I contend that intuition does so more frequently.

ROGER WATSON

Dear Roger

You suggest that I am setting up evidence-based practice as a 'straw dog' (I think you probably mean 'straw man') only to knock it down, and even that I wish to 'throw the baby out with the bath water'. Or perhaps I am throwing the straw dog out with the bathwater? You also say that, in any case, it is all the fault of the politicians and all in your first paragraph!

But let us be serious. While there is no mention of straw dogs in my dictionary, a straw man is defined as a flimsy argument set up by the writer for the sole purpose of knocking it down. You claim my suggestion that nursing has accepted EBP 'in emulation of our medical colleagues' is such a straw man, and that on the contrary, 'nurses have been revolting against the medical model' and in particular, have 'vilified' the randomized controlled trial ever since you have been in nursing. The whole point of setting up a straw man is that it should be very easy to knock down. In order for my claim that nurses have emulated evidence-based medicine to be regarded as a straw man argument, it would need to be insubstantial and unsubstantiated. However, published

evidence suggests that this claim is far from unsubstantiated. Indeed, the mainstream opinion is very much in support of the medical model view of EBP with the randomized controlled trial at the top of the hierarchy of evidence. For example, the editors of the journal *Evidence-Based Nursing* state quite categorically in an editorial that 'the randomized controlled trial (RCT) is the most appropriate design for evaluating the effectiveness of a nursing intervention' (DiCenso *et al.* 1998, p. 39), a view supported by, amongst others, the Department of Health in the UK and the Joanna Briggs Institute in Australia, whose hierarchies of nursing evidence both place RCTs and systematic reviews at the top.

Additional support for the idea that nursing has borrowed the philosophy of EBP more or less wholesale from medicine is provided by many other writers. To take just a few of the many examples, evidence-based nursing has been variously described as 'a derivative of the concept of evidence-based medicine' (French 1999, p. 72), and as 'derived largely from the concept of evidence-based medicine' (White 1997, p. 175). Ingersoll (2000) refers to 'nursing's widespread acceptance and adoption of the principles of evidence-based medicine' (p. 151), and the most often cited definition of evidence-based practice in the nursing literature is Sackett *et al.*'s (1996) definition of evidence-based **medicine**. If my aim was to set up a feeble and unsupported argument that would be easy and uncontroversial to demolish, it appears that I have failed miserably. Despite your claims of a widespread nursing revolt against the medical model, the argument that nursing has adopted the medical model of evidence-based practice and the gold standard of the RCT is very strong and very well supported at the highest levels.

Moving on to your next point, I am pleased to see that you agree with my view that EBP is not itself based on research evidence of any kind. However, your own 'get out clause' for not subjecting EBP to a RCT or other experimental research programme is that the 'alternatives against which it would be tested' are not amenable to such rigorous research methods. This, however, is a pretty feeble get out clause of the 'found difficult and not tried' mentality of which you accuse me. No scientist would argue that a new drug is not amenable to testing in a clinical trial because the alternative homeopathic remedy cannot be similarly tested. If we wish to test the efficacy of a drug, we simply compare its outcomes with the outcomes from a placebo, or with the outcomes of no intervention. Similarly, if you wish to test the efficacy of EBP, you should do likewise and compare it with practice that is not based on evidence from RCTs.

However, this 'get out' hides the deeper problem of your glib dismissal of intuition, reflection and authority. 'Are these not just acronyms, indeed euphemisms, for opinion?' you ask. You answer your own question by reflecting back on your own days in practice to how, in retrospect, 'the great authority figures' often got it wrong. Well, of course we can all play at that game. It is not difficult to reflect back on all the scientific research studies that also got it wrong; on how practice based on the best scientific evidence of bygone days now looks hopelessly out of date or even downright dangerous. All knowledge is provisional, and the point of scientific practice and reflective practice alike is to learn from our mistakes, build on our successes and to do it better the next time.

But this masks the more fundamental issue, which is your misrepresentation of intuition and reflective practice. Of course, at one level, intuition can be presented as 'mere opinion'. But as a number of writers tell us, it is opinion based on many years of clinical experience and systematic reflection on that experience. The **real** problem, which you completely overlook in your wholesale rejection of intuition as mere opinion, is how to distinguish authority based on sound and considered experience and expertise (that is, being **an** authority) from the type of false authority which you describe in your example of the charge nurse, which is based on nothing but the irresponsible imposition of power (in other words, being **in** authority). This is a very difficult task and one that needs urgent attention, but to reject all intuition, reflection and authority out of hand is to reject the accumulated wisdom of many expert practitioners. That, in my opinion, is truly to throw the baby out with the bathwater.

You also, I believe, misunderstand my other main point. You argue that just because nursing is a complex process of unique interpersonal relationships, there is no reason not to test the outcomes. I agree entirely: my point was not that nursing interventions should not be evaluated or tested, but that RCTs which tell us about populations at the expense of individuals are not the best way of evaluating individual nursing interventions, nor are they of any real help in deciding which type of intervention to employ in any particular unique nursing encounter. Of course, as you say, the RCT is the best method for evaluating the effectiveness of drugs, and of course nursing is straying more and more into the territory of medicine, including the prescription of medication, but I am discussing the 'core' of nursing practice,

which I take to be the therapeutic benefits of the individual nurse–patient relationship.

Granted, you do concede that the RCT might not always be the best way to test nursing interventions, claiming that 'some are working to develop new hierarchies of evidence and ways of aggregating qualitative approaches'. However, your example of David Evans' paper is a curious one. He started from the assumption that different types of nursing interventions require different research methodologies for evaluation and identified three broad types: effectiveness, appropriateness and feasibility, each with their own hierarchies of evidence. However, all three of his hierarchies looked more or less identical: each had systematic reviews and RCTs at the top, and each had qualitative research and expert opinion at the bottom. We can perhaps see the extent to which Evans is working on 'ways of aggregating qualitative approaches' if we look at the bottom of one of his hierarchies of evidence, which aggregates 'descriptive studies, case studies, expert opinion and **studies of poor methodological quality**' (Evans 2003, p. 79). The fact that descriptive studies and case studies, not to mention expert opinion, are all given equal status with studies of poor methodological quality rather gives the game away regarding Evans' true opinion of qualitative research. And while you now appear to support the use of qualitative research as a way of evaluating nursing interventions, you stated only 5 years ago that:

> 'What precisely has qualitative research contributed to patient care? I am not saying that it has contributed nothing but the list will not be very long' (Watson 2002, p. 274).

And so to your final objection, where you again miss the point: 'Rolfe would have us stem the tide of evidence-based nursing at its source…'. This is not at all what I am saying. You and I are not disagreeing on our support for EBN, but only on what should count as best evidence. As I said at the outset, the problem with evidence-based nursing is that it has borrowed too heavily from evidence-based medicine, particularly in the adoption of the RCT as the gold standard of evidence. You accuse me of undermining nursing, but I believe that I am doing the exact opposite: I am elevating nursing as separate and distinct from medicine; as being founded on unique and individual inter-personal relationships; and therefore as requiring its own gold standards of evidence which are very different from those of medicine. At its core, nursing is **not** a branch of medical science; it is a social science, a science of social relationships. That is not to say that the RCT should be completely rejected, only that it should know its place.

Finally, then, you ask me whether, in a court of law, I would rather be judged on the basis of evidence or intuition. If by 'evidence' we are referring to generalizable statistical evidence and if by 'intuition' we mean decisions taken on the accumulated authority and expertise of many years of reflective practice, then I will take my chances with intuition any day. We need look no further than the recent case of the woman who was convicted of the double murder of her two babies on research evidence based on the statistical probability of two cot deaths in the same family (and later acquitted when that evidence was found to be suspect) to realize the dangers of basing decisions about individual cases on general evidence from populations.

GARY ROLFE

Dear Gary

First of all, I must defend the existence of straw dogs – I have no greater authority than Google to back me up on this. However, both straw dogs and straw men must be getting a bit soggy with all this bathwater being sloshed around.

As we both allude to, we are not that far apart regarding evidence-based nursing and I am glad that my response elicited such a thorough and robust defence of your original piece and explication of some points that I felt left you vulnerable. I say this, even if I do feel that I have just gone 10 rounds with Mike Tyson – without the presence of the referee.

Moving swiftly to a point of agreement, I agree that all knowledge is provisional and this is especially true of the kind of knowledge that is generated by the 'gold standard' clinical trial. One trial shows X works better than Y and the next shows Y works better than X, and so it goes on with that mysterious phenomenon of regression towards the mean taking its toll on both sides of the argument. And, let us face it, the supposed panacea – the meta-analysis – is really just a method of establishing, usually beyond reasonable doubt that regression towards the mean exists. How many meta-analyses actually show anything useful (or have I been reading an alternative *BMJ*?) and how often is the answer just the proverbial lemon? Therefore, as I made clear and you were kind enough to acknowledge, I am not wholly convinced by the evidence-based nursing phenomenon.

On the other hand, I do defend myself over the nature of the adoption of a medical concept into nursing – I really do not think that we have bought into this wholesale. However, a relatively small and influential group has and, with evidence-based practice so high on

the NHS agenda (those politicians again!), the impression is conveyed of something being more widely adopted than it probably is. Nevertheless, I have seen a tendency in my own journal (*JCN*) for frequent and gratuitous reference to evidence-based practice to justify a study and, presumably, to persuade me regarding publication – when the paper is perfectly interesting otherwise in terms of its findings. I am beginning to have my suspicions.

On the other hand, and nobody reading this would expect us to agree wholeheartedly, I remain slightly confused – a deficiency on my behalf, probably – over which part of evidence-based nursing you still object to. Is it the very concept or is it the way it has been implemented or is it just the clinical trial part? While acknowledging the deficiencies of the clinical trial under many circumstances I am left wondering how we test the core nursing practice: the individual nurse–patient relationship. There must be a level at which it can be put to the test, alternatives compared and future actions guided by such knowledge. Otherwise, what is the point? Again, I contend that if we just say that it cannot be tested conventionally then we are probably wasting our time trying to do things better. I do not think that a qualitative approach is the way. You cast up my words from a previous debate but, surely, most qualitative research is undermined by the very people who write and publish it. Nearly, every paper has the rider that the results are not generalizable beyond the particular participants and that, by means of the research method, it is not possible to test or compare interventions. What use is that in an evidence-based era; how does that take things forward for the next practitioner? I concede that a great deal of quantitative research is published with the same rider but where do we look for a way of testing complex interventions in a way that helps to know what is best and what best to do next time? Perhaps we could use a realistic approach looking for internally generated regularities – what works for whom under what circumstances. However, despite some applications to clinical practice and strong encouragement from the Department of Health to use it in research proposals – there is not a lot to show for this approach either. The same applies to the single-case experiment: great fun, nice graphs but – what next?

The tragic court case you refer to, which had an even more tragic outcome in the end had me thinking. However, on what basis was the original – wrong – decision overturned? It was certainly my intuition that the accused could never have done such a thing but it was evidence – or the refutation of faulty evidence by new evidence – that led to the conviction being quashed. This may just be an extension of the regression to the mean phenomenon but I remain unconvinced that intuition is any safer than evidence.

ROGER WATSON

Dear Roger

I notice that our responses are becoming shorter as we converge on a position that we can both live with. The more I think about our debate, the more it seems that, at times, we are writing about two very different conceptions of evidence, and hence, of what it means for practice to be 'evidence based'. On the one hand is the evidence which is derived from what might be termed the 'pretesting' of therapeutic interventions. In other words, evidence about the efficacy of a nursing intervention is generated from a research study prior to that intervention being introduced into practice (for example, a clinical trial of a new drug). Clearly, if the intervention is to be widely adopted, the research evidence needs to be generalizable from the sample on which it was tested to the population on which it is to be deployed. Thus, a degree of experimental control, decontextualization and depersonalization needs to be introduced into the study, which would ideally take the form of a double-blinded randomized trial. This notion of evidence from 'pretesting' would appear best to fit your use of the term 'evidence-based practice'.

One of my objections to this model of evidence **for** practice is that, despite the scientific provenance of the evidence, there is no guarantee that it will translate smoothly into practice. The problem with evidence derived from pretesting, as I have said, is that its generalizability severely limits its application to unique and specific nurse–patient encounters. Nevertheless, because it is 'high-quality' evidence from RCTs it is likely that it will be incorporated into clinical guidelines and that practitioners will continue to base their practice on it despite its possible ineffectiveness in the messy and unpredictable world of everyday clinical practice. We might therefore easily arrive at a situation where nurses continue with inappropriate or ineffectual practice in the same way that their predecessors did, except that their justification for doing so will be 'because it is evidence based' rather than 'because that is how we have always done it'. I see from your comments that you are also beginning to have your suspicions about 'frequent and gratuitous reference to evidence-based practice' as a justification and guarantee of quality.

However, as you rightly say, we need some indication that our interventions are effective. As you point out, 'if we just say that it cannot be tested conventionally then we are probably wasting our time trying to do things better'. My response would be that if we cannot test individual nurse–patient relationships conventionally using either quantitative or qualitative methods, then we should test them unconventionally. Thus, in contrast to evidence **for** practice derived from pretesting, I am advocating evidence **from** practice derived from post-testing, that is, from evaluating the effectiveness of actual individual interventions employed by practising nurses in the course of their everyday work.

Because 'best evidence', or what we might call evidence **for** practice, cannot predict with any certainty whether a given intervention will be effective in a given situation, we are forced to rely on reflective or post hoc evidence **from** practice, obtained by evaluating interventions after they have been applied. As Dewey (1938) puts it, in the uncertain and unpredictable world of practice, 'we learn by doing and realizing what came of what we did'. Such evaluation should be ongoing and structured, so that practitioners' evaluations of their work are fed back to improve their practice, which is again evaluated and so on. This cyclical generation of evidence from practice should not be mistaken for simple trial and error, and might take the form of action research, structured reflection or even (to take your example) single-case experimental research (graphs and all!). However, the point is that it is undertaken as part of practice itself and its aim is not to influence the practice of others, but to enable the practitioner/researcher who is engaged in it to improve her or his own practice. Thus, there is no requirement for this evidence **from** practice to be generalizable.

I suppose that, to some extent, I have addressed my own challenge posed earlier: how are we to distinguish practice based on expertise and experiential 'intuition' from that based on the unreflective imposition of power. My answer is that the truly effective evidence-based practitioner is the one who, through personal action research, structured reflection, single-case experiments or other evaluative methods, generates evidence of the effectiveness of her or his own practice directly from that practice itself. It goes without saying that this reflexive model of EBP requires a great deal of skill and experience, both in deciding on the initial nursing intervention and also in reflecting-in-action to elicit and respond to the feedback from the patient as the nurse–patient encounter continues. I hope you can see that, far from demeaning the role of the practising nurse, I am attempting to elevate its status and autonomy.

GARY ROLFE

Dear Gary

As you say, our positions to some extent converge: neither of us is totally convinced about EBP, both know that the evidence of its efficacy is lacking and we both acknowledge the difficulty in testing complex and individualized interventions such as those in nursing. However, there are aspects of nursing practice – the more practical aspects such as skin care – where evidence is surely available to guide practice but still lacks implementation. Also, there is abundant evidence for the harmful physiological and psychological effects of restraint and specific evidence for the harmful and potentially lethal effects of cot-sides, yet they are routinely and ritually used with older people in hospital. One of my papers of which I am most proud was not published in a refereed journal but in a geriatric nursing magazine and resulted from a decision to ban the use of cot-sides in one ward in a hospital – we then audited the extent of falls out of bed over a year and there was only one which, incidentally, cot-sides would not have prevented. I am pretty sure, by most definitions that the above counts as evidence. However, nearly 20 years on, this is not being put into practice. We cannot wait until each nurse or each unit discovers such truths for themselves – not that you are suggesting that. But without a policy of EBP how else do we achieve evidence-based practice, where the evidence exists?

Therefore, I think my desire for EBP is to see such evidence as we have incorporated into practice, especially where the evidential and the ethical aspects (and restraint is a perfect example) converge. As we have already agreed, the generation of unequivocal evidence, if indeed it is ever possible, is much harder for a great deal more of what nurses do. On the one hand, we do not want to be involved in the research equivalent of testing the efficacy of parachutes using the RCT method – there are some things we just know; on the other hand, we need to seek evidence where it is available and, when it is good – and this is the crucial step which often leaves us frustrated – get it into practice.

As for the rest – the hard stuff – your vision of evidence generation and testing in practice and the use of action research is alluring and surely a desirable situation. However, how many nurses are fit to practice like that and how many nursing professors like you do we have to do the rounds and facilitate this kind of work?

I think 'not many' is the answer to both. I think your vision should be pursued but I think, and I speak mainly from the UK perspective, we are heading in the wrong direction educationally to achieve this, even in the long term. At the risk of opening up on a new front I would say that the development of a much more graduate intensive profession, possibly an all graduate profession at registration, would be a move in the right direction. Your vision requires a level of competence, capability and confidence that most of our nurses currently do not have.

ROGER WATSON

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