Physiotherapy research activity, and the extent to which our clinical practice appears supported by good science, has grown at a dramatic rate. There are now over 19 000 clinical trials and almost 4000 systematic reviews relating to physiotherapy practice published worldwide (PEDro 2013) and this is likely to continue to expand. The exponential growth that has already occurred would have amazed physiotherapy researchers of 40 or 50 years ago who, while publishing the results of their diverse research studies in a wide range of general medical and physiotherapy journals, were seldom reporting randomised controlled trials.

It is appropriate that physiotherapy interventions should be based upon a strong theoretical framework and the best evidence available. Some authorities believe that this comes only through the conduct of systematic reviews of well-controlled clinical trials; specifically, from the meta-analyses of experimental studies with narrow confidence intervals (Joanna Briggs Institute 2000). While not the opinion of all experts in evidence-based physiotherapy practice (eg, Herbert 2005), there has clearly been an enthusiastic uptake of the idea within our profession. In the most prestigious international physiotherapy journals the reporting of clinical trials and systematic reviews has been growing over the last ten years, as exemplified by the Journal of Physiotherapy and presented in Figure 1.

![Figure 1. Research content published between 2003 and 2012 in (Australian) Journal of Physiotherapy.](image)

Based upon this, we might feel that our clinical practice is increasingly informed by just the sort of research evidence that creates unambiguous direction. Sadly, however, that is not always the case. Examination of 143 systematic reviews published in Journal of Physiotherapy, Physical Therapy, Journal of Orthopaedic and Sports Physical Therapy and Physical Therapy Reviews over the last decade found that 81 of them (57%) were unable to arrive at a conclusion, either one way or the other. Even where reviews led to conclusions, these were typically couched in terms such as ‘moderate effect’, ‘few high quality trials’ and ‘there is a need for further, well-designed trials.’ The equivocation shown by so many authors is, of course, understandable. That further information and evidence is desirable is a truism and a non-committal conclusion has become almost obligatory in systematic reviews. Is it, however, always appropriate to conduct a systematic review? A systematic review is a time-consuming matter, not uncommonly taking from six to 12 months to complete. Where it becomes clear that minimal evidence exists (as opposed to a substantial number of well-conducted trials leading to an unclear result) one wonders whether the reviewer’s energy might have been better spent in other ways. Perhaps inconclusive systematic reviews of randomised trials, where the reader is left with no idea whether a treatment works, should include an expanded ‘Discussion’ section with a broader gathering of information from the literature and from clinical reasoning and other study designs to at least provide a synopsis of the evidence as it exists.

What then of the other high level source of evidence, the randomised controlled clinical trial? Here too, publication rates in the major physiotherapy journals have increased over the years, with this journal leading the way. It is certainly extremely encouraging to see such growth in this type of research, yet there are traps for the reader and the researcher here, too. One danger is that the reader travels no further than the authors’ conclusions with, perhaps, a nod in the direction of the methodological rating through the PEDro score. Often this is the message the reader takes away. However, in one investigation of outcome studies, 70% were found to have conclusions related to causation that were unjustified by the research design used (Rubin and Parrish 2007). Even in randomised trials, the authors’ conclusions may not always be valid. The PEDro score provides a service of enormous value, but is constrained to assess to what extent the design of the trial threatens the internal validity of the study, not the overall validity of the question or choice of design and, as the originators of the instrument themselves note, they can only rate what the researchers are prepared to disclose (Moseley et al 2008).

In many randomised trials the primary hypothesis is the only hypothesis tested or reported. There are few examples in which subsequent analysis has been published or where further exploration of the data seems to have occurred. The researchers often seem to consider that, when a randomised trial is published, they can draw a line under that and move on to the next study. By and large randomised trials only answer the question ‘What?’; seldom do they explain ‘Why?’ – yet only infrequently is there further analysis of the data to explore the depth of the question and then the extra analysis may often be ignored.

As an example, we published a paper detailing a moderately large randomised controlled trial (PEDro score 9/10) which
tested the hypothesis that customised foot orthotics were no more effective than sham orthotics in people with painful pes cavus (Burns et al 2006). We found a positive effect in terms of pain reduction (the primary outcome) from the customised orthotics compared to the slightly smaller pain reduction found with the sham. We subsequently continued our analysis in an attempt to explain these findings and reported that, while the experimental group did demonstrate significantly greater pain relief, we could not attribute this to any change in the patterns or magnitudes of pressure distribution under the foot (Crosbie and Burns 2007). As the whole point of the orthotic was to redistribute pressure away from painful areas, this led us to conclude that the findings of the original study were the result of something other than a mechanical change, possibly a simple placebo effect. Sadly, although our original paper has been cited 26 times, the important explanatory paper has attracted only four citations, two of which were by one of the original authors. Perhaps greater support for the proposal made by Herbert (2008) that researchers make their data more accessible for others to explore will help make explanatory analysis more widespread, but the evidence to date seems unconvincing.

What message does a focus on randomised trials to the exclusion of other designs send to the next generation of physiotherapy researchers and those mentoring them? Research training, whether as part of a formal degree or an informal process, needs to offer as wide an experience as possible and to develop skills that are not confined to one specific research design. The Council of Australian Deans and Directors of Graduate Studies (2007) opined that ‘... a best practice doctoral program should include but not be limited by ... development of new research methods and new data analysis .... and ... research that makes a significant and original contribution to knowledge. It should therefore be necessary for original and significant research to be undertaken in order to earn a doctorate in an Australian university.’

The systematic review and randomised controlled trial have become, in effect, the sine qua non of many (but thankfully not all) contemporary physiotherapy PhD theses. One must question whether this is limiting the potential to produce original thinkers. Is there a better time in the career of a researcher to explore truly new lines of enquiry than during doctoral level studies? Herbert and Higgs (2004) made the important point that ‘a variety of research paradigms ... is needed to explore the range and intersection of social and physical phenomena that concern health professionals ... readers of physiotherapy research need to learn to understand and evaluate different approaches from within the rules of those paradigms rather than from rules that apply to other paradigms.’ By restricting the embryonic researcher’s horizons to a limited definition of ‘best research evidence’ we are narrowing our focus too much and stifling the creativity of some of the outstanding physiotherapy researchers of the future? Further, are randomised trials actually the appropriate design for the question being asked? Prognostic studies, for example, are seldom best dealt with in this way.

A dilemma for the consumer of research, whether clinician, teacher or researcher, who wishes to translate research findings into treatment directions, is that research evidence is situated somewhere on a continuum and although one end of that is represented by the conclusive and comprehensive synthesis of information from the highest level studies, there may be other levels of evidence that can provide assistance in formulating effective treatments (Hjørland 2011). We have perhaps rejected the broader, more exploratory research models because the highest level of evidence is perceived to be the Holy Grail of clinical research, but in the absence of such evidence, what do we do? The prominence given to ‘high’ levels of evidence means that researchers may be coerced into carrying out clinical trials without the benefit of solid theoretical bases and a comprehensive understanding of operational mechanisms. If the experimental question is flawed, the trial will be irrelevant. Examples of alternative models for the development of best practice guidelines do exist. In the ‘Kaufman Best Practices Project’ approach, what we tend to define as evidence-based practice was not applied as the sole criterion, but rather as part of a wider matrix, in which a treatment could achieve ‘best practice’ status only if it could also demonstrate a sound theoretical base, general acceptance in clinical practice, a substantial body of supporting anecdotal or clinical literature, and absence of adverse effects or harm (Kaufman Foundation 2004).

Are we in danger of creating an environment in which clinical and academic physiotherapists are unwilling to go anywhere unless there is a narrowly defined body of ‘evidence’ to support them? If so, our collective research output will become less ground-breaking and our professional practice more robotic. We should remember that much of what has become our best clinical practice originated through eclectic and far-reaching surveys of relevant science. The Motor Relearning Program (Carr and Shepherd 1987) began through a comprehensive collation of up-to-date information from neurophysiology, biomechanics, human ecology, behavioural science, and many other areas. This synthesis led, in turn, to the development of a provisional theoretical framework and the generation of testable hypotheses. Over years this has evolved and was supported through fundamental research (including establishing the reliability and validity of the testing procedures) and, eventually, through the conduct of clinical trials. A similar story might be found in other areas such as manual therapy. Such theoretical constructs generally allow for a degree of flexibility in their application that can account for individual variability and the co-existence of other factors that may impact upon the patient’s response and seldom leave us with nowhere to turn if one line of investigation proves fruitless.

I believe that we need to encourage researchers, clinicians, and researchers-in-training to broaden their analysis of existing literature, the synthesis of which provides them with deeper understanding. There is need also to embrace a culture of enquiry based upon original, novel investigation rather than seeing the systematic review and clinical trial as the only legitimate vehicles for the serious physiotherapy researcher.

Seeking the strongest possible basis upon which to make clinical judgements is a desirable and admirable aspiration and I have no doubt that, as time passes, we will get closer and closer to establishing best practice guidelines across the enormous breadth of our profession. As Hjørland (2011) remarks, however, research-based practice is probably a better aspiration (and does not exclude the concept of levels of evidence) than a narrow focus on the shibboleth of
evidence-based practice as it may currently be understood or interpreted. Physiotherapy research is, relatively speaking, still in its infancy. By the time physicians started to embrace evidence-based medicine (around 1972) they had a hundred years of research providing a theoretical basis (think of Pasteur, Lister, Koch, Charcot). Perhaps physiotherapists should be prepared to invest in the scientific and theoretical basis of their professional practice before chasing evidence to support it.

References
Websites
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Paper of the Year 2012

The Editorial Board is pleased to announce the 2012 Paper of the Year Award. The winning paper is chosen by a panel of members of our International Advisory Board who do not have a conflict of interest with any of the papers under consideration. The Award is given to a paper published in the 2012 calendar year which, in the opinion of the judges, has the best combination of scientific merit and application to the clinical practice of physiotherapy.

The 2012 Award goes to Neural tissue management provides immediate clinically relevant benefits without harmful effects for patients with nerve-related neck and arm pain: a randomised trial by Robert Nee and colleagues from The University of Queensland.

This elegant randomised trial involved 60 people with non-traumatic nerve-related neck and unilateral arm pain. The experimental group received education, manual therapy, and nerve gliding exercises in four treatments over two weeks. At four weeks participants in the experimental group reported substantially greater reductions in neck pain, arm pain, and self-reported activity limitations over those participants who only received advice to remain active.

Reference