

# Some Thoughts on Methodologies and Practice Research Networks

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Stiefel, Renner & Riordan (2003: pp. 49–50) in this column have continued our discussion from the last issue. In essence they note that the production of evidence can proceed in a number of directions following a range of methodologies. In so doing they direct our attention to the plausibility and legitimacy of conducting research at the 'coalface' into issues that are of concern to us as clinicians and service providers.

As Stiefel and her co-authors point out so well, the most obvious problem for clinicians about the current conceptualisation of EBP is that the methodology is too expensive in money, time, and return. Most clinicians argue, quite correctly in my view, that there is little value in the information generated by Randomised Controlled Trials (RCTs). As a result, the issue of research is frequently neglected by clinicians. This reflects the very limited, and very limiting, effect of the current conceptualisation of what constitutes 'good' research. Stiefel et al. make the point that there is a range of sources of information that can be considered evidentiary. But, there continues to be a dominant belief in a hierarchy of evidence, the 'gold standard' or 'royal road' to scientific 'truths'. Though proponents of Evidence Based Practice (EBP) may acknowledge the legitimacy of other methodologies, there is a consistent theme that such sources are of a coarser grade and of less value than the RCT.

This is very evident in the current dominance of cognitive-behavioural therapy, which is based on the claim to an 'evidence base'. These claims are very powerful because they reside within the dominant assumptions of the medical profession as to what constitutes 'good science'. At this point in time, these assumptions have considerable political influence and CBT is now promoted to general practitioners throughout Australia as the treatment of choice for a range of psychological problems. Health service policy at a federal level is strongly influenced by this argument and most official practice guidelines in this area recommend CBT. All of this is due in large part to the relatively unexamined assumption that RCTs represent a higher level of evidence, allowing previous research on the effectiveness of psychotherapies to be devalued.

Stiefel and her colleagues suggest that there is the multiple bottom line to be considered in evidence based research. This is a very neat way of encapsulating the

concept that no single method can provide answers to the infinite range of questions that we can generate about the activity of therapy. It is folly to assume that we need to serve only one master in conducting research in this area, as this would severely limit the interesting and useful questions that could be asked. A multiple bottom line reflects the reality that there are many valuable questions to be asked in this field and that only a subset of these can be usefully explored utilising a RCT approach. It also nicely indicates that each methodology has a place, and that none should claim to be 'better' than all the rest.

As Sackett and Wennberg (1997) have pointed out, a great deal of time and energy is wasted in the debate about the 'best' research design for evidence based practice. The most obvious and basic premise is that *the question being asked determines the most appropriate research strategy*. Obviously any research agenda should never be restricted to one design. In reality many different designs will be used to explore fully any area of interest. Restricting a research effort to RCTs really limits the type of research questions that can be answered.

There is no 'royal road' to the production of knowledge through an empirical method. There are as many byways, detours, and woodland tracks as there are freeways, highways, and expressways between the questions we want to ask and the information that we use to find answers. The current belief that there is only one true scientific method is rather like suggesting that travelling from Launceston to Hobart via the East Coast is a waste of time. I suppose if you don't like scenery and only want to get from point A to point B as fast as possible, there is some truth in this. But, by analogy, the current environment is one in which people who arrive in Hobart, having travelled via the Coast road, are met by a party of hecklers who deprecate and condemn the values and interests that led to their choice of the scenic route.

At least three broad research strategies are available. Qualitative, efficacy (RCT), and non-experimental cohort (effectiveness) research. But the number of possible research



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tactics is probably approaching infinity and limited only by the knowledge, skill, intuition, and funds of a research team. Research programs with true 'depth' will tend to range over the area of inquiry for many years, utilising a full range of research methods to explore the questions as they develop. Ultimately the informed judgement of the researchers in the design and planning of research to answer a particular question is probably the single most important ingredient in interesting and useful research (Cohen, 1992).

Clinicians and the teams within which they practise are in unique positions so far as research is concerned. Many clinicians will remain members of teams for years. Therefore it is possible for long term research agendas to be fostered and developed. Even more than this — there are many clinical teams that practise in the same or similar areas. Such teams have unique opportunities to establish research-based links that can be very long term indeed. This is a paradigm that Margison et al. (2000) call 'practice-based evidence', which is a way of gathering good quality data from routine clinical practice.

One of the most significant blocks to the development of major research programs around 'real world' clinical practice is funding. Research is often seen as an add-on for services and is generally not accorded a central role. Also research is often an onerous task particularly if there is only one, or a few, members in a clinical team wanting to do it. When you consider that the data generation capacity of a single individual or team is relatively small, the amount of effort required to generate even small data sets can be a real barrier.

A very interesting way of overcoming these obstacles is to develop Practice Research Networks (PRN). A similar concept is commonly used in medicine to generate information about the efficacy of treatments but, in that context, these are incredibly centralised, hierarchical and focused entirely on the RCT methodology. Practice research networks usually involve a large number of clinicians (or clinical teams) who agree to collaborate to collect and report data. They can be defined as '... a network of clinicians who collaborate to conduct research to inform their day-to-day practice' (Margison et al., 2000). The main difference between PRNs and the medical clinical trials is that data is gathered in the 'real world' over long time periods rather than being time limited for specifically orchestrated clinical trials. Generally these networks are linked through academic centres to enable good dissemination of information and literature reviews. The main advantage of a PRN is the generation of very large data-sets (Margison et al., 2000).

There are few examples of clinicians forming PRNs, but there are some obvious advantages. There is an economy of scale to PRNs. Assume one small team of four clinicians opens 100 new cases each year. Realistically, it will take several years before any good longitudinal data has been generated within the team. If however, these four clinicians are part of a PRN with ten other teams, there is the

potential for good representative data-sets to be generated in a much shorter space of time. I wonder whether more clinicians would be interested in doing research as part of their clinical practice if they could see results more quickly?

The internet offers practising clinicians another exciting way to begin to develop PRNs which are not necessarily tied to academic centres (which have potentially conflicting political and economic agendas). There is a range of software tools now becoming available which enable virtual 'teams' to come together, exchange ideas, plan and develop projects and share data no matter where they are geographically located. Obviously there are problems with this sort of venture, not least the learning curve for teams as they explore new technologies. I intend to discuss this area in more detail in a future issue.

One step that you might like to take is to think about the issue of PRNs and consider how they might be applicable within your team, specialisation, or area. Are there any natural alliances that can be developed between your team and other teams practising in the same way or with similar types of clients? What are the questions that could be asked in your area that haven't been asked or answered to the satisfaction of the clinicians? Are there existing practice support networks that could be extended in to research networks? Are there organisations at your local level which intersect sufficiently at the clinical level to make a research network practicable (e.g. Child Protection and CAMHS teams)? Are there any funding sources for this type of effort? How difficult would it be to attract funding if the PRN was big enough?

If there is a great divide between research and clinical practice, it exists because we put it there. As students, educators, clinicians, and researchers, we are responsible for the territory that we map. I know that most clinicians' eye glaze the moment that the issue of research is raised. I wonder how many clinicians even read this section? But this is an area of critical importance for those of us who do not associate exclusively with the CBT tradition. It is all too easy to be 'rolled' and manipulated by others waving the placard of science. It is therefore incumbent on clinicians to have thought enough about this area to know that the issues are not so straightforward.

## References

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