

Editorial: The Place of Proven Models

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A recurrent theme in this Journal will be reports on evaluations of services designed to reduce impairments in children's development or otherwise enhance their well-being.

There is now a growing bank of evidence on what are called 'proven models'. There is no consensus over what this term means, but for the sake of discussion it can be defined as a service – typically a structured programme – that has been demonstrated in one or more experimental evaluations to produce better outcomes for the children served.

This evidence has been assembled in a number of databases, examples of which are listed at the end of this editorial, and is also brought together in what are called 'systematic reviews' which analyse the combined results of several trials of the same programme or series of programmes aimed at specified impairments to children's development. The article in this edition by Jane Barlow and her colleagues does this in relation to services designed to prevent or ameliorate child physical abuse and neglect.

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Some of proven models become well known. As highlighted by John Coughlan's article, recent government investments in parenting programmes in England, for example, have brought attention to programmes like *Triple P*, discussed in the previous edition of the Journal, and *Incredible Years*. There is a strong message that investments in most of the myriad of parenting programmes to have emerged in recent years will bear less reward than investments in one or more of the 'proven' models.

Although proven models are more prominent now in professional discussion than even a few years ago, their actual implementation in European, North American and Australasian children's services remains rare. Most proven models have been designed in the US but even there their application remains very much the exception rather than the rule.

In this context the case for proven models seems straightforward. It is hard to imagine their greater use being a problem and there are clearly many potential benefits. The obvious gain will be better outcomes for children. The websites listed at the end of this editorial contain programmes shown to have reduced emotional, social, educational and behavioural impairments to children's development.

But as important is the rigour used to design and evaluate proven models. It takes considerable discipline and good science to develop models like *Multi-Systemic Treatment*, *High/Scope* or *Nurse Family Partnership* – to name just three of the programmes commonly referred to on the databases. That discipline will typically result in a manual that itemises how the programme should be delivered and a clear view of the target group to ensure that only those children known to benefit actually receive the service. The process will also generate a clear logic model, so that those involved know *why* the programme works, and an understanding of what will be achieved, so that expectations can be realistic. Many proven models also pay better attention to the ethics of service delivery.

It would be a mistake, however, to see proven models as a panacea. Care needs to be taken in their application and it is possible to see how their use might become counter-productive.

Perhaps the most pressing concerns are ecological validity and cultural competence. Regarding the former, a programme may work effectively in the context in which it is tested but remain largely irrelevant to daily practice with children in need. Some parenting programmes, for example, are initially tested in very carefully controlled conditions with unusually high investment of resources. The results will be important to the development of a new programme but they should not, of themselves, urge the wholesale application of the programme.

Surprisingly little attention is given to the related question of cultural competence. Few would imagine that a model shown to work in London would be immediately relevant to practitioners in the developing world. Yet there can often be an assumption that a model shown to work with, say, an African American community living in extreme poverty in the US is transferable to European societies. At the very least, the model should be tested again in its new context, and in some cases the programme may require considerable revision while keeping the underlying logic model intact.

More serious is the poor implementation of proven models. Given the pressures of modern children's services, there can be professional rewards for rapidly implementing part of a proven model, and, since adaptations are rarely rigorously evaluated, it is assumed that the benefits in terms of child outcomes will continue to be achieved. It would be surprising if they were. Applying a proven model to a different target group, or allowing children not known to benefit from the programme to receive it, can be similarly attractive to the busy manager but it is unlikely to achieve much for a child in need.

Another challenge is which proven model to implement. If childhood depression responds to assessment, cognitive behaviour therapy and pharmaceutical interventions, which of these options should be implemented? Some scientists

point towards effect size or the amount of reduction in impairment attributable to the programme. But low cost models with modest effect sizes and minimal side effects may often be preferable to expensive and intrusive services. In his work, Richard Harrington (2002) skillfully suggested how practitioners could skillfully combine effective techniques, each of which works with *some* children who have the designated condition, to achieve optimal results from limited resources.

Potentially the greatest danger from the call to implement more proven models would be the mistaken inference that *unproven* approaches are ineffective. Several local authorities in the UK are beginning to think about designing and testing a new service for a group of children within the care population. The existence of Multi-dimensional Treatment Foster Care, a proven model for another group of children in care now being evaluated in England, does not argue against the testing of new and by definition unproven methods. More innovation and evaluation is required, not less.

Two further points should be borne in mind as this trend continues. First, proven models entice us towards programmes that work. But improving outcomes for children also requires an understanding of what does *not* work, and applying that understanding to policy and practice. Very few specialists concentrate on the dissemination and application of the 'what doesn't work' evidence.

Second, despite the emergence of new models to reduce depression or anti-social behaviour, the underlying trends in both have been upwards, at least in England (Nuffield Foundation, 2004). Advocates of proven models might employ this evidence to argue for their greater use, and certainly the evaluation of innovation, coupled with the careful taking-to-scale of proven models, is likely bring considerable short-term and some long-term benefits for the children that receive them. But common sense might tell us that such targeted prevention, early intervention and treatment activity can only be one part of the broader task of improving outcomes for children.

Databases

Blueprints: <http://www.colorado.edu/cspv/blueprints/>

Promising Practices (RAND) : <http://www.promisingpractices.net/programs.asp>

SAMSHA: http://www.samhsa.gov/Campaigns_programs/campaigns_alpha.aspx

Penn Prevention: <http://www.prevention.psu.edu/projects/index.html>

Campbell Collaboration: <http://www.campbellcollaboration.org/>

Cochrane: [http://www3.interscience.wiley.com/cgi-](http://www3.interscience.wiley.com/cgi-bin/mrwhome/106568753/HOME)

[bin/mrwhome/106568753/HOME](http://www3.interscience.wiley.com/cgi-bin/mrwhome/106568753/HOME)

References

Harrington, R. (2002) 'Affective disorders', in Rutter, M. and Taylor, E. (Eds) (2002) *Child and Adolescent Psychiatry (4th Edition)*, Oxford, Blackwell.

Nuffield Foundation (2004) *Time Trends in Adolescent Well-being*, London:

Nuffield Foundation.