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[1] I have proposed a new terminology that moves away from referring to cultures as *'eastern/western'* or *'low-income/high-income'* as they do not lead to accurate ways of classifying cultures. I use the dimensions of power (high/low) and self concept (individualistic/collectivistic) to group cultures. In this terminology the so called *'western'* or *'high income countries'* of Europe and North America are characterised as high-power individualistic (HPI) cultures, while many *'non-western'* or *'low income countries'* would be relabelled as low-power collectivistic (LPCO) cultures. See Fernando (2012).

[2] The allegiance effect is also known as the *'Dodo bird verdict'* after a scene in the novel *'Alice in Wonderland'* where a competition is ended by the Dodo bird's verdict: *'Everybody has won, so all shall have prizes'* (See Luborsky et al., 2002).

Gaithri A. Fernando, PhD, is a professor at the Department of Psychology, California State University Los Angeles, U.S.A. She is consultant psychologist to Program for Torture Victims in Los Angeles.
email: gfernan2@calstatela.edu

Further thoughts on evaluating interventions for posttraumatic stress disorder in low and middle income countries

William Yule

While agreeing that there are very few studies on Narrative Exposure Therapy, the author responds to the critique of Mundt et al. (elsewhere in this issue), by arguing that psychological interventions should not only, nor primarily, be evaluated by outcomes immediately after the end of the intervention. This is because psychological interventions, such as Narrative Exposure Therapy, aim to start a

process of recovery that may continue long after treatment and eventually result in stronger change.

Keywords: Narrative Exposure Therapy, outcome measurement

Elsewhere in this issue, Mundt et al. (2014) raise a number of concerns about Narrative Exposure Therapy (NET) as an intervention to treat posttraumatic stress disorder (PTSD) following wars and disasters in low and middle income countries (LAMICs). While it is quite proper that such interventions are evaluated seriously, they also need to be evaluated properly. By this, I refer to the critique that is presented by Mundt at al. which uses a scattergun approach aimed at both appropriate and inappropriate criticisms.

Mundt et al. accept that PTSD may commonly occur as a result of (multiple) war experiences. They, and Neuner, Schauer & Elbert (2014) are also in agreement that there is a need to develop and apply evidence based intervention techniques to large numbers of war affected individuals. Neuner et al.'s strategy has been to first develop an intervention that is relatively brief and then, having validated it, roll it out so that many local therapists can be trained to implement it. So far, the two groups might be seen to be in agreement.

Even a brief reading of Schauer, Neuner and Elbert (2005) shows clearly that these authors take a very broad view of PTSD and see it as affecting a wide range of human experiences. After looking at the effects of war, torture and the holocaust, they discuss the cultural expressions of stress and seek to develop a novel approach to therapy that is informed by a sophisticated analysis of memory and neurobiology. They are not part of the hit-and-run group of naive, well meaning therapists who rush to a war zone and apply views of PTSD that are narrowly inspired by the western classification systems such as the DSM (*Diagnostic and Statistic Manual*) (American Psychiatric Association, 2013). Rather, they have built on testimony work and applied their models in refugee camps where survivors have faced multiple traumatic events.

Perhaps where the two groups of authors differ is that Neuner, Schauer & Elbert (2014) are developing a brief intervention, while Mundt et al. view trauma as *'deeper'*, more complex and not amenable to brief interventions. However, while Neuner et al. are well aware that their intervention does not tackle all aspects of refugees needs, they also know from experience (as opposed to prima facie musings) that traumatised refugees welcome some relief from their psychological suffering in order to allow them to tackle other troubling aspects of their life. To have developed a technique that can alleviate distress related to multiple traumas is therefore only to be applauded.

Mundt et al. developed a number of criteria that they apply to published studies of NET to evaluate their scientific worth. As Neuner et al. comment, there is a number of well worked out rules to evaluate interventions already in use. Why try to reinvent the wheel? To demand that psychological interventions should only, or mainly, be evaluated by standardised measures immediately after the end of an intervention betrays a misunderstanding of the nature of mental functioning. Indeed, the whole complex way of evaluating psychological therapies suffers from accepting a medical model, whereby only results of randomised controlled trials (RCTs) are considered. However, even effects of medication may well take time to affect a change in an organism. This may be even truer in the case of psychotherapies, which aim to start a process of recovery that is intended to continue after treatment, and results in stronger change over time. With some interventions, there may well be sleeper effects that do not manifest immediately. Therefore, any evaluation must look carefully at immediate, short and long term effects.

Moreover, Mundt et al. give a misleading description of the theoretical underpinnings of NET and seem to have missed the radical change in psychological models of PTSD,

whereby the disorder has been characterised as primarily a disorder of memory, and not of anxiety (Ehlers & Clark, 2000; Brewin & Holmes, 2003). Even the DSM-5, the new edition of the American standard psychiatric classification system, has taken that on board. In their characterisation of NET as a '*confrontational*' psychotherapeutic intervention – as if discussion and reliving of a traumatic event cannot be beneficial – Mundt et al. show themselves to be concerned that PTSD is a '*western concept*' that does not occur worldwide. Are they afraid that the application of NET may be considered cultural colonisation?

Having said that, it is appropriate to note that there are very few controlled studies of NET and those that Neuner et al. have published have very few participants. This is, sadly, characteristic of the whole field of psychotherapy. Objective measurement and humanitarian intervention rarely coexist. Neuner et al. respond by saying that their studies are efficacy studies that demonstrate that their method can and does have positive effects. They are not in the business of effectiveness studies, i.e. demonstrating that an intervention can be applied to large numbers outside of the controlled research setting and yet, that has to be the ultimate goal for NET and other interventions.

Finally, with regards to the multitude of other problems that refugees face, of course it is desirable that survivors have adequate food, clothing, hygiene and privacy. However, the question is: should mental health problems be ignored until these are attained? Of course, all countries should have well developed mental health delivery systems, but does that mean that psychological interventions should be postponed? Is it not better to try to help an extended family within earshot of others, rather than wait for sound proof rooms to be created? Of course, it is well recognised that after war and disaster there are many adverse mental health effects, but is that a reason to stop developing interventions for some? The concerns about '*complex PTSD*', '*possible harm of re-exposure*' and about '*early interventions*', are similar to just about every concern that has ever been raised about PTSD and its treatment, and is now targeted unfairly at NET. Why do Mundt et al. protest so loudly? Such criticism would be slightly more acceptable coming from mental health professionals with experience of mounting and evaluating studies in complex post war settings.

There does remain a need to devise realistic and acceptable criteria for the evaluation of all aspects of humanitarian help. This assumes that, some day, there will be adequate resources made available for all aspects of mental health. Meanwhile, single case experimental designs exist, as do small scale controlled studies that can demonstrate a causal link between an intervention and an outcome. To demonstrate that an intervention is better than any other or that it is cost effective requires larger scale studies. These should be large enough to take account of individual differences and not just be geared to demonstrating statistical significance on measures that may themselves be clinically insignificant. In my view, the need for interventions that can be delivered to groups of survivors is urgent. My colleagues and I have shown that group interventions can be implemented and evaluated in post disaster settings (Yule et al., 2013). Such work would be impossible without the pioneering developments of people like Schauer, Neuner and Elbert.

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William Yule, PhD is a child psychologist. He is Emeritus Professor of Applied Child Psychology at King's College London Institute of Psychiatry and Founding Director of the Child Traumatic Stress Clinic and chairperson of the Foundation for Children and War, a charity based in Norway (www.childrenandwar.org).
email: william.yule@kcl.ac.uk

Don't fault RCTs for not testing systems of care

Andrew Rasmussen

Among Mundt and colleagues' (2014) criticisms of NET is that testing a particular intervention implies that it is meant to be delivered irrespective of systems of care. But trauma-focused treatments should be judged based on the specific problems they target; larger sets of problems are the purview of health systems research.

Mundt and colleagues' (Mundt et al., 2014) critiques of the research on Narrative Exposure Therapy (NET) are various and wide ranging. These critiques include: modest effect sizes; concerns about privacy protection; disapproval of the practice of applying North American and European diagnostic categories in nonEuropean cultures; and doubts about the reliability of randomised control trials (RCTs) in low and middle income countries (LMICs), in general. Although some of these critiques may have merit, I am troubled by the undercurrent of enmity towards RCTs in LMICs as there are good examples of successful RCTs in LMICs. Additionally, RCTs are one of a powerful set of evaluation techniques, and it is unclear to me why any researcher, or monitoring and evaluation team, would want to limit the number of tools in their methodological toolbox. However, rather than attempt a cogent defense of RCTs here, I refer readers to the wealth of literature defending experimental designs in our field (e.g., Meffert & Ekblad, 2013) and the ethical arguments concerning withholding RCT results (e.g., Neuner, Schauer, & Elbert, 2014, in their response to Mundt et al., this issue). Suffice it to say that those who accept the argument that RCTs cannot be undertaken reliably in LMICs unnecessarily handicap their programmes' accountability.

My comments here primarily concern Mundt et al.'s (2014) criticism that NET, and by extension all trauma focused treatments with RCT support, are somehow