LETTER TO THE EDITOR

STRESS RESILIENCE TRAINING: THE NEED FOR CIRCUMSPECTION

Dear Editor,

Weltman et al have presented a fascinating report on the effectiveness of a stress resilience training program on police department personnel (Global Adv Health Med. 2014;3(2):72-79). Certainly at first reading there appears to be strong evidence of the effectiveness of the program. However, careful evaluation of the methodology and results suggests the need for some circumspection.

Firstly, this was not a randomized trial and there was no alternative intervention or control with which to compare the intervention. It is therefore impossible to exclude the fact that some of the perceived effectiveness of the intervention might have been due to a placebo effect. It is possible also that the assessment questionnaires and surveys that the participants underwent also had an effect on the outcomes. Reflecting on the questions and considering their answers could have given the participants greater insights into their levels of stress and how they dealt with it and might deal with it—even before they received the therapeutic intervention.

Secondly, the intervention, which the authors rightly described in considerable detail, was multifaceted. This is a reasonable approach to deal with a complex problem; however, it makes it difficult to unpick the intervention and say which facet was most effective in achieving the positive outcome. It could have been the training on stress and its effects or the coherence biofeedback or the HeartMath self-regulation techniques—it is difficult if not impossible to tell from the methodology used. This has important implications for how the results of the study might be implemented in practice (outside of a research setting). Each of the facets of the intervention will be associated with costs, and a multifaceted intervention will be more expensive than a uni-faceted one. The payer may simply not be able to afford effectiveness at any cost.

Lastly, the authors rightly ensured that the participants' voices were heard in the study report—by quoting some of their feedback. However, the authors might have better analyzed the feedback using a qualitative methodology and described the methodology employed and the theory that underpinned the methodology.

Perhaps some of these issues could be dealt with in a follow up study of this important subject and the related intervention.

Kieran Walsh, FRCPI
Tavistock Square, London

Author Response

We agree with Dr Walsh that these are important questions. This case study was performed with the cooperation of the San Diego Police Department and was necessarily relatively limited in scope. However, we felt the results of the case study needed to be published as the officers' feedback was surprisingly strong and positive, and we did achieve statistically significant results despite the small sample size and short exposure. We are in the process of conducting more formal studies with the US Navy and other organizations in which Dr Walsh's concerns will be carefully addressed. We greatly appreciate the careful reading of our case study article and the very helpful suggestions.

Gershon Weltman, PhD, Jonathan Lamon, Elan Freedy, and Donald Chartrand
Sherman Oaks, California

GAHMJ welcomes your feedback. Letters to the Editor may be sent to GAHMJsubmissions@gahmj.com.

Citation

Global Adv Health Med.
2014;3(4):5. DOI:
10.7453/gahmj.2014.039

**GAHMJ's mission is to engage,
inform, and inspire the global
community of practicing healthcare
professionals to more effectively
promote health and treat illness.**

Subscribe now at gahmj.com