

Commentary on “Five Essential Elements of Immediate and Mid-Term Mass Trauma Intervention: Empirical Evidence”  
by Hobfall, Watson et al.

## No Plans Survive First Contact with the Enemy: Flexibility and Improvisation in Disaster Mental Health

Norman Jones, Neil Greenberg, and Simon Wessely

This wide-ranging paper draws together a substantial amount of data about the nature of traumatic stress reactions and their management. The information has been drawn from a wide range of sources and has then been subjected to an expert panel review. The result is a comprehensive strategic guideline for the management of a population's response to major traumatic events. Given the nature of the world we live in, the paper explores an important and relevant topic (Bromet & Dew, 1995; Van Ommeren, Saxena, & Saraceno, 2005).

The authors, without question pre-eminent in their fields, identify five guiding principles which those in positions of responsibility need to keep in mind when putting together disaster management plans. When carried out alongside the control of lost or damaged infrastructure, the five guiding principles act as desirable outcomes which should help to maintain and restore psychological health. The principles are safety, calming, self- and collective efficacy, connectedness, and instilling hope. The paper details the evidence behind the utility of each principle and

goes on to clarify the public health measures that emergency planners can use to establish each principle at both an individual and group level. The paper is clear that it is evidence-informed rather than evidence-based which is appropriate, and we wholeheartedly endorse this approach. Because of the unpredictable nature of disasters, it is difficult, although not impossible (Boscarino et al., 2004; Rubin et al., 2005), to conduct rigorous research in the chaos of a post-disaster environment, and therefore using an evidence-informed philosophy has allowed the authors to draw from a wide range of available data. Furthermore, the paper avoids the potential criticism that is often levied at over-reliance on randomized controlled trials (Lau, Ioannidis, & Schmid 1998) which are especially difficult to undertake after disasters.

Considerable thought has gone into developing a theoretical framework underlying much of this paper, for example around the cognitive and neurobiological responses to trauma, which is admirable. We wonder, however, just how achievable some of the rec-

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*Norman Jones, MSc, and Neil Greenberg, MRCPsych, are affiliated with the Academic Centre for Defence Mental Health in London.*

*Simon Wessely, MD, is affiliated with the King's Centre for Military Health Research in London.*

Address correspondence to Dr Neil Greenberg, Academic Centre for Defence Mental Health, Weston Education Center, Cutcombe Road, London SE5 9RJ; e-mail: sososanta@aol.com

ommendations may be in practice. The paper discusses the public health measures recommended for promoting safety after a catastrophic experience which include, for example, "safety from bad news, rumors and other interpersonal factors that may increase threat perception." While such an aim is clearly desirable, is it feasible? How easy would it be to enable individuals and groups to discriminate between real and imaginary threat, both at a practical and cognitive level? How possible might it be to take people to a safe place in some post-disaster situations? After Hurricane Katrina, for instance, it took emergency services many days to reach the majority of those who had been affected, and despite billions of dollars being spent following 9/11 to improve emergency coordination, the response to Hurricane Katrina was not a success (Gheyntchi et al., 2007). While we support the concept that making a safe place available is likely to translate into psychologically beneficial rewards, it is not always going to be possible. Another example is the authors' desire to have appropriate and relevant information delivered through already existing information dissemination systems; this, however, assumes an intact communications network. Even in the aftermath of the London bombings, relatively discrete and small-scale traumatic events of 2005, the mobile phone networks failed for a number of hours. The research conducted into the consequences of the bombings showed that those who could not contact loved ones using their mobile phones were indeed more distressed (Rubin et al. 2005), echoing earlier findings from Israel (Bleich, Gelkopf, & Solomon, 2003) However, given both the need to allow emergency services to use the mobile phone networks and to mitigate the risk of further devices being triggered by mobile phone activation, it is hard to see how even the most psychologically minded emergency planners could have handled the situation differently. Such communication difficulties are likely to be even more pronounced in less well-developed areas of the world. Some control of the media is also required to influence the amount and type of information that is conveyed. Real life experi-

ence of the media, at least in the UK, suggests that while the media do not deliberately set out to mislead, they do not see it as part of their essential role to be a calming influence either. And there will be times when their overwhelming agenda of "telling the story," and doing so as fast as possible in a competitive, multi-media environment, will directly contradict some of the goals laid out in this paper. Rumor is likely to follow rumor in an information-poor environment, and even if some of the more established media organizations may perceive a wider public duty, at least for a short time during a crisis, the days of the quasi-monopoly provision of, for example, the BBC over here are long since passed. In the world of the media, unfortunately, chaos and high drama are often equated with success.

The paper assumes that "large-scale community outreach and psycho-education about post-disaster reactions should be included among public health interventions to promote calming," but as the authors will surely agree, the evidential basis for this is weak. One positive intervention with adolescents is cited (Goenjian et al., 2005); however, this was multi-dimensional and not simply psycho-education. On the other hand, one of the few trials that have studied psycho-education alone (Turpin et al., 2005) found a negative effect on victims of accidents. Furthermore, psycho-education is also a core part of debriefing, whose gradual eclipse was the stated reason for the production of this paper.

The majority of the copious references to this paper have been written in the last decade, only four pre-date 1980, and none pre-date 1970. Yet an interest in how people react to adversity, war, and trauma did not begin in 1980, as any consideration of the troubled twentieth century would confirm. A reader who consults the pre-1980 literature would encounter a rich historical literature that would challenge some of the assumptions made in this paper, for example, the value of psycho-education. Granted, that would also be lacking in much empirical evidence or support, but, as the authors repeatedly acknowledge, that remains the case for much of what is proposed in this paper. Our point is that more

attention needs to be paid to the cultural underpinnings and assumptions of our current approaches to understanding communities in crisis, and accepting that some of our current fundamental assumptions are just that—assumptions. It may well be that assumptions that hold true in one situation may not translate well into another (Furedi, 2003; Jones & Wessely, 2007; Turpin, Downs, & Mason 2005). Of course, it is true to say that the more historical assumptions were never empirically tested, but one might well point to the essential resilience of populations subjected to high levels of danger and tragedy over prolonged periods, such as the citizens of London, Berlin, and Leningrad. Likewise, there is no denying the scale of psychological problems in soldiers of the First and Second World War, but the current levels of psychiatric injury reported from service personnel in Iraq suggest that whatever else we have done, we have not solved this problem either.

Our assumptions about how both individuals and communities respond to threat and trauma have undergone a radical transformation in the course of the twentieth century, and hence so have our assumptions about management. It is hard, for example, to conceive of a major public health/mental health intervention such as Project Liberty (the large scale intervention for the inhabitants of New York City after September 11th) even being contemplated for a previous generation. This would not have been due not to a simple lack of resources, commitment, or awareness, but because it would have been considered fundamentally an unwise strategy (Jones et al. 2004). We cannot say whether or not the lack of such a program hindered or helped the citizens of London exposed to the Blitz, any more that we can say that the presence of such a program assisted the citizens of New York in 2001/2002. What we can say is that a previous generation thought about similar issues, came to different conclusions, and, as far as one can judge, the results were not catastrophic.

A further area of concern is that some of the suggested strategic interventions require ready access to trained therapists able to de-

liver evidence-based interventions, which in the main will be comprised of cognitive and behavioral techniques, after disaster has struck. Once again, we question if it is realistic to believe that sufficient, appropriately trained therapists will be available to deliver such a strategy when social structures have broken down. Certainly in the UK they are already in short supply (Layard, et al., 2006), and the demand for the provision of established evidence-based treatments for well recognized and non disaster-related mental disorders already vastly outstrips supply. It is also possible to question at least some of the assumptions underlying these interventions. In certain circumstances, some cognitive responses to trauma are adaptive, such as hypervigilance which may help to keep people safe when the risk of terrorist attack remains high. Likewise, the authors go out of their way to accept that many, perhaps even most, non-disabling post-traumatic stress reactions may be normal, certainly within the first month or so after an incident, but perhaps they pay less attention to the risks of disrupting the normal recovery pathway and with it the normal processing of events. As the UK's National Institute for Clinical Excellence states, "watchful waiting and subsequent targeting of those who are not progressing may be more fruitful" (National Institute for Health and Clinical Excellence, 2005). Despite endorsing the essential normalcy of many emotional reactions to trauma—and hence the importance of people mobilising their own social resources and networks as they deal with them—the paper also at times seems to be arguing for a broad-based public health and therapeutic response, which will seemingly involve substantial numbers of those affected by trauma, unlike the minority as envisaged in the UK NICE Guidelines. We feel that whatever natural resilience that populations are likely to have should be exploited rather than assumed to be lacking.

There is a tension between what is evidence-informed, intuitive thinking on an abstract level and what can be achieved in reality. This does not mean that the principles are incorrect—far from it; they are attractive and

based on the available scientific knowledge to date. It is their execution that will be difficult. The paper appears to adopt an industrialized, resource-heavy perspective that assumes a reasonable level of connectedness and preparedness pre-trauma, perhaps because the vast majority of its authors, and indeed the authors of this commentary, come from such a culture. But what of those parts of the world that lack even the most basic infrastructure. How would the strategy be delivered in such circumstances? Even when large-scale disasters do lead to the provision of additional resources from the international community, including those intended to have a direct mental health component (as opposed to the indirect mental health advantages that come from restoring homes, schools, water, and food), as happened after the Asian tsunami the available data suggests that this is not well used (Pupavac, 2001, 2002; Sumathipala, 2006).

The paper also suggests that high-level interventions are required, particularly in support of rural development and vocational skills training. While such plans are bold and ambitious, we believe that there are likely to be difficulties in getting local governments to make available sufficient fiscal resources in support of the population's psychological needs, especially at a time when they are appropriately considering the reconstruction of damaged buildings and infrastructure. However, we fully support that the recommendations should be considered by disaster plan-

ners at an executive level and be cascaded down stratified levels of executive responsibility through to local delivery. It may be that given the current fears of terrorist attack, governmental priorities can change rapidly in both their attitudes towards psychological need and the promise of financial support.

The uncertain nature of disaster often leads to the blurring of boundaries between the provision of practical support and psychological intervention. The authors have boldly attempted to use an evidence-informed approach to provide emergency planners with a set of guidelines that may help focus their post-disaster interventions. We believe that the paper probably tries to do too much and in some ways has tried to be all things to all people. While we fully support the paper's general strategies, we feel that it might be seen as too ambitious in its provision of detailed post-disaster public health interventions. We believe that emergency planning committees should include mental health practitioners, with relevant experience and training, who might help leaders to effectively discharge their responsibilities towards affected populations in a tailored fashion, using local assumptions. The paper sets out what is generally a consistent strategic and intellectual approach, but one which should be linked to a policy of allowing local responders to apply the principles when and where it is appropriate for the local context and culture.

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