

Correspondence

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Fear reduction by psychotherapies: a response

Dr Snaith (2000) misquotes us (Marks & Dar, 2000) on an important point. We do not conclude that “all elements . . . have therapeutic potential and that any assertion of superiority of one approach over another is unwarranted”. We specifically state that non-applied relaxation, avoidance (anti-exposure instructions), diary keeping, treatment set, giving a rationale, and regular homework assignments are not particularly therapeutic *per se*. Several approaches are less helpful than others.

We are grateful to Dr Snaith for reminding us of his results with anxiety control training (ACT). His paper (Snaith, 1974) noted that several ACT patients did imaginal or live exposure, which is covered by our discussion on exposure. He described his 1982 trial (Constantopoulos *et al*, 1982) of ACT briefly in a non-peer-reviewed chapter. Just 12 patients were randomised to either experience anxiety scenes or just cope with anxiety without exposure. His papers (Constantopoulos *et al*, 1982; Snaith, 1998) give too little detail to judge how much each treatment used imaginal exposure (implosion) or irrelevant fear exposure (stress immunisation). The reports give no mean ratings and standard deviations before and after treatment, preventing judgement of how much each group improved. Though both groups improved with no significant differences between them, the study lacked power – a very big difference would be needed to yield significance when comparing two cells containing just six patients each. Dr Snaith’s results with “just coping with anxiety” may echo those with irrelevant fear exposure and support our idea that stress immunisation (irrelevant fear exposure) may reduce anxiety. Snaith *et al*’s (1992) paper did not describe randomisation to ACT or a contrasting procedure.

Our call for psychotherapists to work towards a common psychotherapy language that defines each procedure in a standard accepted terminology is bolstered by examining Dr Snaith’s terms. What he calls “meditation” has relatively little in common with Kabat-Zinn’s (1996) mindfulness meditation, and his ACT, for example, includes components which are not specified regarding relevant *v.* irrelevant exposure. If psychotherapists agreed to call the same procedures by the same names, that would be a huge step forward. European and American associations in the field (the European Association for Behavioural and Cognitive Therapy (EABCT) and the Association for Advancement of Behavioural Therapy (AABT)) have appointed a joint task force to develop a common psychotherapy language.

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Psychological debriefing: historical military perspective

May I offer a historical military perspective on the paper by Mayou *et al* (2000). Proponents of psychological debriefing have misused the military experience from the Russo-Japanese War 1904/5 onwards to justify early psychological intervention using PIES – proximity (close to the scene – in safety), immediacy (as soon as possible), expectancy (that individuals will return to duty – not to prevent ensuing psychological illness) and simplicity (respite, rest, recollection, rehabilitation and return to duty). Proponents conveniently forget that PIES was only ever applied to those who were deemed to be suffering and was conducted by individuals who shared and understood their experience.

There may be many reasons why Mayou *et al* arrived at their conclusions but the same caveats apply as are appended to Bisson *et al* (1997), Kraus (1997) and Turnbull *et al* (1997), among others. Perhaps we (psychiatrists) are at fault in trying to categorise human responses to unpleasant events into medical conditions and are naive to think that one intervention could prevent post-traumatic stress reactions and illnesses that are multi-factorial and complex in genesis.

In trying to understand and manage post-traumatic stress reactions there are a number of useful metaphors. Garb *et al* (1987) find the grieving process particularly useful as both post-traumatic and grieving are responses to loss events. Although an unfashionable term, psychological defence mechanisms exist to protect the individual (at least initially); to interfere with such mechanisms carelessly courts disaster. Perhaps psychological debriefing does just this. In both post-traumatic reactions and grief there is a period of introspection during which individuals do not wish to talk. Such needs should be respected, and usually are in the case of grief. Why should traumatic events be different?

This period is followed by a time when assistance and advice is welcome, even sought, and in post-traumatic situations, as in grief, this should first be sought from the social support network. If this does not work, then professional help may be required, but we as professionals must question the seemingly ubiquitous societal belief that exposure to

traumatic events is always an entirely negative experience and that post-traumatic stress disorder is the only post-traumatic mental illness.

I do not share the authors' reassurance that the three-year follow-up rate was only 48% as it provides ammunition for those who will, I fear, continue to provide psychological debriefing. Perhaps it is cynical to question their motives but I am troubled by the almost pornographic nature of human experiences outwith the normal. There is a voyeurism and the potential vicariously to become part of a traumatic event, even of history, by intervening. Society's or is it the media's cry is 'something must be done', and despite the growing body of evidence that psychological debriefing does not work, or is harmful, I suspect such work will not be halted unless society changes from its 'psychologicalisation' of human distress. There is an old military adage that applies here: 'the only thing harder than trying to get a new idea into a military mind is trying to get the old one out'.

Perhaps Mayou *et al*'s paper reinforces the reality that there are no 'quick fixes' for human experiences. The provision of help should be directed towards those who are defined as affected by their experiences. Identifying these cases should be the challenge for psychiatry. Perhaps then the advice proffered by Salmon (1917) will be correctly applied, although such interventions are unlikely to be so simple.

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Turnbull, G., Busuttill, W. & Pittman, S. (1997) Psychological debriefing for victims of acute burn trauma (letter). *British Journal of Psychiatry*, **171**, 582.

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Psychosocial treatment programmes for personality disorders: service developments and research

Chiesa & Fonagy (2000) clearly demonstrate the beneficial effects of therapeutic community treatment for personality disorder, and more so in the treatment limb with less hospitalisation and more day care.

The logical extension of this is to offer these programmes with only day care. Several units around the country are now doing this, including new units in Aberdeen and Maidstone, as well as long-established units such as our own in Reading and the Red House in Salford.

The evidence from systematic reviews and meta-analyses for the effectiveness of therapeutic communities in treatment of personality disorders is strong (Lees & Manning, 1999) and, together with the Cassel study, demonstrates the need for new, creative ways of setting up effective treatment programmes.

A multi-centre research project funded by the National Lotteries Charities Board is now underway, which should help in this endeavour. It is using multi-level modelling and a path-analytic equation modelling technique to determine the impact of a number of features that therapeutic community programmes have. This research is more complex and sophisticated than a simple randomised controlled trial design, but for treatments that do not fit a drug model paradigm it will be much more helpful in designing effective programmes of therapy. The protocol is available at www.pettarchiv.org.uk/atc-protocol.htm.

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More disappointing treatment outcomes in late-life depression

Tuma (2000) reported disappointing outcomes in the treatment of late-life depression. Suicide rates are highest in the elderly

in many countries (Shah & De, 1998), while treatment with drugs and electroconvulsive therapy consistently results in full recovery rates of less than 30% (Murphy, 1983). Some studies show slightly more optimistic findings, such as Baldwin & Jolley (1986) and Brodaty *et al* (1993) who demonstrated prognosis in later life approaching that in younger adults at one year. Yet others suggest that longer follow-up reveals a worse outcome (Forsell *et al*, 1994). These studies use standard physical treatments but make no mention of adjunctive psychological treatments of any kind.

There are still too few studies demonstrating the effects of psychological interventions in older people (O'Rourke & Hadjistravropoulos, 1997). More recently published data have shown improved outcome using a combination of drug and psychological treatments, including interpersonal therapy and cognitive-behavioural therapy (Reynolds *et al*, 1999). In addition, important research by Ong *et al* (1987) demonstrated relapse prevention for individuals attending a support group.

In a recent postal survey, I enquired of members of the Royal College of Psychiatrists' Faculty for the Psychiatry of Old Age whether elderly patients in their care had specifically requested psychotherapy. The overall response rate was 65%, of which 49% had experience of patients asking for psychotherapy. One can only assume that those already in receipt of such treatments would not ask for it. Patients rarely demand drug treatment as they are often already taking medication. The National Health Service (NHS) Executive (1996) review of psychotherapy services endorses the need for older patients to have access to similar service opportunities as the young.

Since elderly consumers of our service are asking for psychotherapy, and because there is some evidence (Roth & Fonagy, 1996) that it is a useful adjuvant in the war against late-life depression, why are we still producing research which appears to ignore this approach?

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