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Correspondence

To the Editor,

Cognitive therapy of obsessive-compulsive disorder: treating treatment failures. *Behavioural Psychotherapy* 13, 243–255.

We note the comments of Gurnani and Wang (15, 101–103) on our paper and welcome their interest in the development of new strategies in the treatment of obsessive-compulsive disorder. They express the following reservations about our paper, which we will consider in turn:

1. Inappropriate use of cognitive strategies may result in inaccurate generalizations about its value.
2. The content of our treatment sessions should be described as imaginal exposure.
3. Mood changes alone might account for our results.
4. Cognitive therapists are merely relabelling well established treatments.

1. We agree that cognitive-behavioural models should not be applied to *individual* patients until a careful assessment of the cognitive phenomena has been performed. In this case, a small part of our assessment can be seen in Tables 1 and 2; from this we hypothesized that the distorted belief in the dangerousness of the feared stimuli may have been of particular importance in the patient's refusal to carry out exposure treatment. The cognitive treatment then used was a test of this hypothesis, although we note explicitly at several points that we are unable to rule out alternative explanations of the observed change in belief and behaviour. This analysis was particularly careful in view of doubts about the form of the patient's beliefs (see Salkovskis and Warwick, 1986; Thomas, 1986).

Although treatment is always based on a thorough behavioural analysis of each case, we concur with Gurnani and Wang's view of the importance of a more general theoretical formulation of "cognitive errors, deficits or absences that may be of . . . significance in the disorder to be treated". However we would regard the crucial elements relevant to treatment as being cognitive-behavioural factors implicated in the *maintenance* of the disorder (we are seldom in a position to understand aetiological factors). In this case the phenomenology was entirely consistent with the recent cognitive-behavioural formulation of obsessive-compulsive disorder (Salkovskis, 1985).

To prevent "inaccurate generalizations about the value of the treatment

approach," new strategies must not be accepted without rigorous empirical testing. However, clinically relevant single case experiments represent the most common initial stage of development for new techniques. An important aspect of our paper was the identification and examination of issues likely to be relevant to future studies, enabling the clear evaluation of treatment strategies in this population.

2. Imaginal exposure involves the repeated presentation of anxiety provoking stimuli until habituation occurs. Gurnani and Wang suggest that our intervention unintentionally contained elements of imaginal exposure, which accounted for the subsequent compliance with *in vivo* treatment. We clearly state (p. 254) that our intervention was a complex one and it is impossible to specify exactly what went on during the sessions—imaginal exposure may have been a component. Such *post hoc* theorizing is futile, as any cognitive approach to anxiety requires discussion of threat related material, thus making *any* cognitive strategies interpretable as imaginal exposure and *vice versa*. Note however that we have carried out a session of exposure *in vivo*, prior to the cognitive intervention (p. 247) during which within-session habituation did not occur and the subsequent refusal of further exposure sessions could be regarded as a failure of between-session habituation. These data suggest that it is unlikely that imaginal exposure entirely accounted for the observed effects of the treatment. For this to be the case imaginal exposure would have to be a more effective treatment than exposure *in vivo*—there is no evidence to support such a view (see James, 1986). Furthermore, if our results are interpreted as being entirely due to the effects of imaginal exposure then we would appear to have stumbled upon an unusual and particularly effective way of carrying out such a procedure. Gurnani and Wang suggest that their argument is supported by the fact that the patient continued to exhibit overvalued ideation throughout the treatment. They do not consider the marked drop demonstrated in degree of belief—98% to 30–70%.

3. The importance of depression in obsessional problems cannot be overstressed. The statement "the unsuccessful behavioural intervention took place in the setting of a severe depressive episode, whilst the subsequent more successful procedure was correlated with the remission of the same," is in direct contradiction to the information presented in our article. At the beginning of the psychotic episode the patient had a BDI score of 43 which fell to 31 on treatment with amitriptyline. During the next 14 weeks her mood remained stable (Figure 4), *until* the cognitive intervention was commenced. This allows us to argue strongly that the subsequent improvement in mood was due to this intervention. Further evidence for this view is the substantial and lasting change in instantaneous mood rating from 80 to 40 (using a 100 point scale, Table 1) obtained during the first cognitive session.

4. This is a preliminary report of the use of techniques utilized in cognitive therapy which, in combination with behavioural methods, produced improvement in a type of case which had previously been reported as failing to respond to a variety of behaviour therapy procedures (Foa *et al.*, 1983). As we state in the paper (p. 253), "the treatment which was successful contains a number of elements which may have contributed to its effectiveness and it is clearly impossible to decide which of these individually or in combination were responsible for change." We do not suggest "the inclusion of a formal cognitive component' in routine treatment of all obsessionals. However, cases of the type we reported are likely to be chronically distressed and we feel that all available strategies should be considered and evaluated.

Reports such as our own highlight the relationship between theory, clinical practice and single case experiments. The understanding of the psychopathology involved in any condition can be enhanced by experimental investigations of single cases (Salkovskis, 1984). Gurnani and Wang's point that a closer inspection of what cognitive therapists do "often reveals procedures which are *not entirely* different from those of well established treatments" (emphasis added) is, of course, true. However, such unhelpful generalizations may prevent us from considering the merits of new approaches—there is no room for such conceptual rigidity in either cognitive or behavioural fields, especially as they are so intimately connected (Salkovskis, 1986). Despite similar arguments in the early days of behaviour therapy, behaviour therapists never stopped talking to patients for fear that their procedures were *not entirely* different from psychoanalysis. The only principle we can usefully adhere to is that of sound empirical testing, indeed "It is what we do that should be evaluated, not what we call ourselves."

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