patients being asked to do an hour's daily self-exposure and to keep homework diaries of this to enhance compliance), rather than the systematic exposure which usually does well in non-severe cases. Perhaps in such cases cognitive therapy enhances systematic exposure or helps even without it, but the Oxford design precludes conclusions on both these issues.

How much do exposure and cognitive therapy both work through teaching of (a) problem definition, goal setting, and recording of goal attainment, (b) habituation, (c) changing of beliefs, and (d) fostering of a sense of control? Such factors need not be mutually exclusive and could be synergistic, but need teasing out in firmer designs than those employed so far. Better definition is needed of which procedures are (1) uniquely 'cognitive', (2) uniquely 'behavioural', (3) overlap, or (4) are common to any treatment. We also need to separate therapy *procedures* (what the therapist does) from the *theories* on which they are said to be based. We agree with the authors that the treatments should be tried out in severe cases. We would be glad to work with them to clarify the minimum therapy procedures needed.

BASOGLU, M., MARKS, I. M., SWINSON, R., *et al* (1994*a*) Pretreatment predictors of treatment outcome in panic disorder and agoraphobia treated with alprazolam and exposure. *Journal of Affective Disorders*, **30**, 123–132.

BAŞOGLU, M., MARKS, I. M., KILIÇ, C., *et al* (1994*b*) Relationship of panic, anticipatory anxiety, agoraphobia and global improvement in panic disorder with agoraphobia treated by alprazolam and exposure. *British Journal of Psychiatry*, **164**, 647–652.

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AUTHORS REPLY:

Our study (Clark *et al*, *BJP*, June 1994, **164**, 759–769) showed that cognitive therapy is an effective treatment for panic disorder. Marks *et al* suggest that it is effective not because of its specific cognitive procedures but because it encourages exposure to feared stimuli. We excluded the possibility that situational exposure could account for the effects of cognitive therapy by arranging that the comparison psychological treatment (applied relaxation) included the same amount of exposure homework assignments as cognitive therapy. Neither of these treatments involved the use of exposure diaries. However, Marks *et al* suggest that exposure to interoceptive cues during the behavioural experiments that are part of cognitive therapy may be important, and call for a component analysis study. There are two such analyses (Margraf & Schneider; Salkovskis *et al*) both of which were cited and discussed in our article. Together these studies indicate: first, that cognitive procedures are effective when used alone; and second, that combined interoceptive and situational exposure is also an effective anti-panic procedure. Further support for the efficacy of cognitive procedures alone comes from studies demonstrating their ability to block laboratory-induced panic (see Clark, 1993 for a review). It is less certain how effective is situational exposure alone. The London–Toronto (LT) study (Marks *et al*, 1993) did not find any difference between situational exposure alone and placebo/relaxation treatment as judged by panic measures. Marks *et al* (1993) argue that this failure to show a specific anti-panic effect for situational exposure was a consequence of the high response rate in their placebo/relaxation group. However, the end of treatment panic-free rate for applied relaxation in our study (50%) was almost identical to the end of treatment (8 week) panic-free rate for the placebo/relaxation group in Marks *et al*'s study (47%) and we were still able to demonstrate a significantly higher response for cognitive therapy (90% panic-free).

*Predicting long-term outcome*. Cognitive theory predicts that patients who continue to misinterpret bodily sensations at the end of treatment should have a worse outcome at follow-up than patients who no longer make such misinterpretations. Correlational analyses supported this prediction. Marks *et al* wonder whether other non-cognitive variables might have been equally predictive. We have now investigated the variables suggested by Marks *et al*. None are significant predictors.

*Severity of agoraphobic avoidance and response to cognitive therapy*. We excluded from our study the 18 cases (20% of otherwise suitable referrals) who met DSM–III–R criteria for severe agoraphobic avoidance. Marks *et al* suggest that cognitive therapy would have done less well with these cases and that situational exposure would have been relatively more effective. Clearly a direct comparison between cognitive therapy and situational exposure with such patients is needed to address this question. However, existing data are not encouraging for their suggestion. First, patients in our sample had a considerable range of agoraphobic avoidance but initial level of avoidance did not predict response to either cognitive therapy or applied relaxation. Second, within our sample of

https://doi.org/10.1192/S0007125000074365 Published online by Cambridge University Press

patients with panic disorder with no, mild, or moderate agoraphobic avoidance, cognitive therapy was superior to applied relaxation not only on measures of panic but also on measures of agoraphobic avoidance. In contrast, the LT study of exposure treatment found no specific effect of situational exposure on panic at *any* level of agoraphobic avoidance.

*Corrections*. Two statements in Marks *et al*'s letters require correction. In their first letter (Marks *et al*, 1994) they state that situational exposure "eliminated 96% of panics" in the LT study. This is misleading as the 96% figure seems to come from an assessment carried out 35 weeks after the end of exposure therapy (43 week assessment) and based on only 61% of the initial sample. At the end of exposure therapy (8 weeks) the group receiving exposure and placebo showed a more modest 63% reduction in panic frequency. When commenting on our exclusion of severe agoraphobics in the current letter, Marks *et al* state that "most LT cases (were) severe". This is also misleading as their use of the term "severe" is different and less stringent than ours. They appear to be referring to the DSM–III category of "extensive phobic avoidance" whereas we were referring to the DSM–III–R category of "severe agoraphobia". Many cases meeting criteria for the former would be categorised in DSM–III–R as "moderate agoraphobia" not as "severe agoraphobia" and would therefore have been included in our study.

CLARK, D. M. (1993) Cognitive mediation of panic attacks induced by biological challenge tests. *Advances in Behaviour Research and Therapy*, **15**, 75–84.

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**Home-based versus in/out-patient care for people with serious mental illness**

SIR: Marks *et al* (*BJP*, August 1994, **165**, 179–194) mention in their discussion that "outcome was rather better with home care than in/out-patient care", whereas in the abstract they report that "outcome was superior with home-based care". There appears to be some uncertainty about the interpretation of the results, and we would like to offer some suggestions that we feel would facilitate interpretation of this important and carefully conducted study.

*Analyses*. The authors have identified 19 endpoints in the trial (number of admissions, days in hospital, GAS, BPRS, PSEtotal, PSEdah, PSEbso, PSEnsr, PSEnsn, patient's satisfaction, relative's satisfaction, SAS global, SAS social, SAS extended family, SAS parents, SAS daily living skills, SAS economic, SAS work and SAS marital). As no primary, prespecified outcome has been identified, all 19 are of equal importance from the statistical point of view. For each outcome, separate significance testing is carried out at each of the three measurement points, so that the reader is presented with 55 tests in the main article, followed by another 255 in the appendix. *P*-values at the 10% significance level and over are reported "non-significant", and *P*-values $<0.1$ are apparently considered significant. This approach is problematic. First of all, presenting multiple *P*-values can only lead to a subjective conclusion, because it is impossible to decide how many of the 55 tests in the main article need to be "significant" in order for the trial to show superiority of home-based treatment over standard care. Fifteen (27%) of the tests were significant at the 5% level, but this still leaves 40 measurements where the confidence intervals include values that indicate that home-based care actually makes patients worse. Secondly, multiple conventional significance testing can seriously inflate the overall Type I error rate (false positive results), and the study protocol should specify in advance how the problem of multiple testing is going to be dealt with. One way to control *P*-values when all endpoints are analysed on equal terms is the Bonferroni correction. However, the Bonferroni correction becomes too conservative if the endpoints are correlated (as is clearly the case in this trial), and the modification proposed by Simes (1986) is more appropriate, as realistically the Type I error becomes $<\alpha$ (Pocock *et al*, 1987*a*). Applying the modified procedure by Simes to the data of Marks *et al* (only taking into account that there are 19 endpoints and ignoring the fact that further separate tests were carried out at each measurement point, and in five subgroups), reveals that of the 15 results significant at the 5% level, only 8 survive (15% of the total of 55). Thirdly, and most importantly, the plethora of significance testing is

https://doi.org/10.1192/S0007125000074365 Published online by Cambridge University Press