In its launch issue, the European Journal of Clinical Hypnosis – October 1993 – carried a paper by Elizabeth Taylor, Dr Mike Goodman and Tony Waring describing a twelve-week programme of psychological therapy conducted on ten patients with inflammatory bowel disease. The aricle described how all ten subjects received a sequence of three types of treatment, each delivered in four weekly sessions. The three treatments were:

'Non-specific hypnotic relaxation'.
'Tutorial therapy'.
'Gut-directed hypnotherapy'.

There was a four-week period before the start of therapy when baseline measures were taken and a similar four-week follow-up period on completion of the twelve weeks of therapy.

The measures taken before, during and after therapy were weekly General Health Questionnaire scores and daily records of bowel activity, ('morbidity') 'problem events' and 'emotional responses to those events'.

The article claimed that the results of the study offered strong support for the efficacy of psychological treatments for inflammatory bowel disease, and that while all the treatments were found to be beneficial, the gut-directed hypnotherapy was the most effective.

Not all EJCH readers agreed with the conclusion and among the responses to the article was a request from Dr Michael Heap, lecturer in Clinical Hypnosis at Sheffield University, England, to be allowed to comment on the paper which, he believes, failed to demonstrate its claims.

Three major areas of doubt over IBD study claims

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M. Heap & W. Dryden (Eds) (1991) ***Hypnotherapy: A Handbook.*** *Milton Keynes: Open University Press.*

H.B. Gibson & M. Heap (1991) ***Hypnosis in Therapy. Hove****: Lawrence Erlbaum Associates*

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There were three major problems with the Taylor, Goodman, Waring paper.

The first was that the number of patients treated was too small in proportion to the scale of the investigation, namely the comparative efficacy of three different psychological treatments.

This kind of research can only be satisfactorily undertaken using different groups for different treatments.

Studying a small number of patients with each as his or her own control leads to enormous problems in analysis and interpretation.

The second deficiency was the absence of a control group. An untreated but monitored group of patients would enable one to ascertain the extent to which the observed improvements were due to the prescribed interventions and not merely the reassurance of regular contact with a professional. Likewise it would enable us to chart the likely progress of the population selected for treatment, bearing in mind the variable nature of the disease and the fact that the patients were appropriately medicated.

I do not agree that the selection criteria render unlikely any observed improvements in the absence of treatment. For a clinical

trial lasting 20 weeks a selection criterion of a minimum relapse period of three months seems unduly small. I am suspicious of the additional criterion of *"one or more relapses during the previous twelve months"* since this implies the occurrences of remission in the same time period.

No Control Group

Incorporating an untreated group would allow for any propensity of the selected population to improve regardless of psychotherapeutic intervention; the experimental design would also be sensitive to the implied prophylactic benefits of the therapies for asymptomatic patients, of whom there were a high proportion in the experimental sample.

The absence of a control group could be mitigated to some extent by having a longer follow-up period; in any case this is essential with chronic illnesses in order to demonstrate that any therapeutic gain is more than just a temporary respite.

The third major problem with the study was that the gut directed hypnotherapy was always the last therapy each patient received (except, by default, in one case) and thus had the advantage of being undertaken after eight weekly sessions of the other two therapies. No comparisons were therefore possible between the effectiveness of this treatment and the other two. This evaluation was nevertheless attempted in two ways, both of which are incorrect.

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To evaluate the whole treatment programme we would, of course, compare baseline and follow-up measures. Indeed there was evidence of significant improvements from baseline to follow–up on three of the four measures – General Health Questionnaire, 'problems' and 'emotional responses', but not 'morbidity'. (It would, however, have been simpler and more sensible to compare mean scores for the four weeks rather than to take each week separately.)

The most logical method of evaluating a specific therapy is to look at the difference in scores at the beginning and end of the four weeks of treatment. Instead, the scores of all twenty weeks were compared for significant differences. This did not allow us to say anything about the differing effects of the therapies.

So, for example, the only significant differences between scores obtained during treatment were between the first week of tutorial therapy and the third week of gut–directed hypnotherapy ('reported problems' and 'emotional responses').

It was incorrect to claim that this demonstrated the greater effectiveness of gut-directed hypnotherapy; we can only speculate that the difference in scores may have been due to whatever interventions occurred in the interval between taking these scores, namely a mixture of two, and for some subjects, three different therapies. Moreover, as had already been stated, treatment effects and position of treatment in the order of administration were confounded in the experimental design.

Falacious Conclusion

The other mistake in the evaluation of the effects of each treatment was the non-statistical comparison between mean scores obtained during each of the three courses of therapy with those obtained before the onset of the treatment programme. This was reported in the case of 'morbidity' (although earlier in the paper it is stated that changes on this dimension were not statistically significant.) Thus it is concluded that gut-directed hypnotherapy was the most effective because the rating fell from an original baseline of 9.5 to 4.9 whereas for non-specific hypnotic relaxation the fall was from 9.5 to 6.3.

This was fallacious because it ignored the fact that in most instances one or (in the case of gut-directed hypnotherapy) two treatments intervened between the original baseline measures and the treatment under evaluation. The conclusion that gut-directed hypnotherapy was superior to the others was entirely unwarranted.

The only salient question about gut-directed hypnotherapy which the study allowed us to address was what additional effect this treatment had in patients who had already undergone eight sessions of non-specific hypnotic relaxation and tutorial therapy. The data given in the paper did not permit us to be confident about making this assessment but some idea

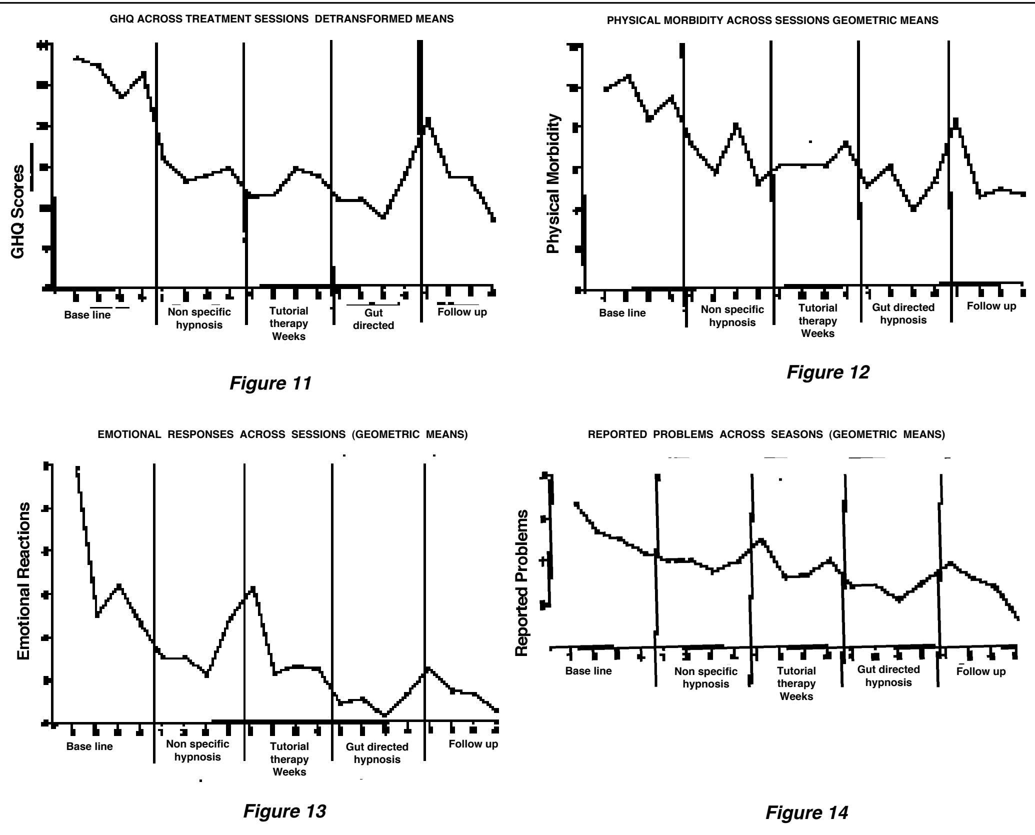

Figure 11

Figure 12

Figure 13

Figure 14

According to Tayllor, Goodman and Waring, figures 11 to 14 showed a clear reduction in symptoms and they concluded that psychological intervention was able to reduce the emotional problems and physical discomfort related to IBD.

could be gained by comparing the scores at the onset of gut directed therapy with those obtained during the follow-up period **(Figures 11-14 as shown above)**

There appears to be very little change. (In fact the statistical analysis does not report any significant differences of relevance here.) So without further evidence we should conclude that the gut-directed approach was surplus to requirements and the therapeutic gains observed on follow-up would probably have been largely the same without this intervention. (Of course, by that stage there may have been little further gain to be made.)

In the statistical analysis, the data of a patient who did not undertake the treatments in the pre-arranged order are omitted. I am not sure that this is necessary as the order of administration for the remaining nine patients is still not counter-balanced (even just for non-specific hypnotic relaxation and tutorial therapy). If the concern is for the carry-on effects of gut-directed hypnotherapy then there should also be the same concern for the other two therapies.

Studying a small number of patients with each as his or her own control leads to enormous problems in the analysis and interpretation

My second point is that the graphs in the paper may give the impression that the scores are plotted in chronological sequence (i.e. from week 1 to week 20 in serial order). This impression is conveyed by the linking of the data points. If the labelling of the horizontal axis is correct then this is not the case in most instances, so the reader should take care in interpreting the figures.

CONCLUSIONS

The Taylor, Goodman and Waring study and their analysis of the results allow us to draw the following conclusions. After a 12-week programme of psychological therapy (4 weeks each of non-specific hypnotic relaxation, tutorial therapy, and gut directed hypnotherapy) 9 patients with inflammatory bowel disease showed significant improvements on General Health Questionnaire scores, 'reported problems' and 'emotional reactions', but not 'physical morbidity' (bowel activity). No difference was observed between the effects of non-specific hypnotic relaxation and tutorial therapy. The gut-directed hypnotherapy was always administered after the other two treatments and appears to have had little additional impact on improvements already made. We cannot be sure how much, if any, of the observed improvement was due to the specific effects of treatment and how much this improvement is maintained beyond the four week follow-up period. It is important to undertake further research to clarify these matters..

I would like to commend the authors for undertaking this research and to urge them to persist in their investigations into the efficacy of psychological procedures for these kinds of problem.

● **See "*Readers Write*" for further discussion of the Taylor, Goodman, Waring paper**