given (1) in fantasy or in practice, (2) individually or in groups, (3) at varying speeds, (4) with anxiety heightened or lessened, (5) with or without 'psychodynamic' cues present, (6) with frightening cues which are relevant or irrelevant, (7) with differing durations of sessions and (8) of intersession intervals, (9) with differing intervals between fantasy and *in vivo* flooding, (10) with fantasy flooding sometimes imposed externally by the therapist and sometimes abreacted spontaneously by the patient, (11) with differing endpoints of a given session (is it best to end on a good note?), (12) by tape-recorder or by a live therapist, (13) with or without coping instructions, and the nature of these.

As work proceeds doubtless other minutiae will also appear potentially relevant. Generalizations about 'flooding' will only become accurate when the relevant conditions have been dissected out in detail. Some of these conditions are undergoing investigation in many centres, and from these useful generalizations should eventually become possible. Meanwhile, reports of exposure research will be interpreted more easily if they specify the experimental condition in more detail, including these 13 variables. Workers in the field need to develop an agreed vocabulary for describing research on exposure treatment.

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DEAR SIR,

We agree with many of the points made by Dr. Marks, but wish to clarify some differences:

(1) We defined, at the beginning of our paper (p. 446), those factors which we considered 'non-specific'. Encouragement to practise counter-phobic behaviour was included in these because it seems to be common to many different approaches to treatment. Our conclusion merely states how much effect might be attributable to the sum total of the components which we defined.

(2) We do not say there were no important differences between treatments during the *in vivo* phase; on the contrary, difference did exist in '. . . the hierarchy levels used and degree of anxiety tolerated' (p. 448). Thus, patients were vigorously encouraged to tolerate greater anxiety and more difficult situations during flooding than in desensitization, although we did not continue verbal flooding during practice sessions.

'(3) Dr. Marks states that *in vivo* exposure is 'much more therapeutic' than exposure in fantasy. There is no unequivocal evidence for this, since studies such as Stern and Marks (1973), like our own, use designs in which interaction between treatment phases is possible and even to be expected. For example, it may be that agoraphobic patients improve rapidly during *in vivo* treatment only after previous exposure in fantasy (p. 460). We are carrying out research to test this, by comparing *in vivo* practice given alone with combinations of fantasy and *in vivo* treatment.

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DEAR SIR,

The suggestion of M. G. Gelder *et al.* in their paper that revision of the current explanations of desensitization and flooding is needed prompts me to write this letter.

While this letter is neither a criticism nor an endorsement of behaviour therapy, I would refer to Locke (1971), who believes that behaviouristic procedures contradict every major premise of behaviourism, and to Wilkins (1971), who asserts that the effectiveness of the procedure is not due to the mutual antagonism between muscle relaxation and anxiety but rather to social variables involved in the patient-doctor relationship and to cognitive variables, including expectancy of therapeutic gains, information feedback of success, and so on.

If one accepts these views, considering the therapeutic results are favourable, one has to assume that perhaps the behaviour therapists are doing the right thing for the wrong reasons.

After sifting the accumulated wealth of material and both observing and carrying out behaviouristic therapy, I have come to the conclusion that perhaps desensitization and flooding are based on certain and

specific sets of neurophysiological mechanisms. These are:

(1) In desensitization the mechanisms are: (a) habituation, and (b) the operation of a self-adapting sorting screen for the memory bank. Sokolov (1963) exposed a subject to a beep of a specified intensity and duration at irregular intervals. The EEG and galvanic skin responses were monitored. At first the changes in the tracings were characteristic of the orienting reaction. As the experiment continued the indices of this orienting reaction diminished until the beep no longer had any effect. Habituation had taken place. When the intensity of the beep was reduced without changing the other variables the tracings indicated another orienting reaction. From this it appeared that habituation was not the result of fatigue in the neural elements but rather the action of a process against which incoming sensory signals are matched. Any change in the character of the beep had this orienting effect. Even sudden silence could become an activator. This process appears to be incorporated into a self-adaptive sorting screen which contains coded representations of prior signals brought about by person-environment interaction. These coded representations are matched against incoming signals. If there is constancy in this system the encoding is strengthened, leading to expectancies of environmental condition. This reduces or stops the orienting response. It needs to be stated that an exaggeration of the orienting response is accompanied by anxiety,

In therapeutic desensitization, habituation is favoured where the screening way-station matches incoming stimuli against the imaginal suggestions of the therapist.

By approximating a match between the imagery and the phobic content the necessity for an orienting reaction is diminished or altogether abolished. Consequently no alarm reaction takes place. Ideally the sorting screen permanently acquires a new set of encoded information which contains the elements of the phobic situation—or to put it in vernacular: familiarity breeds contempt.

(2) The flooding follows the principle of counter-irritation. Counterirritation should not be confused with the principle of reciprocal inhibition originally proposed by Sherrington which was extrapolated as a concept by Wolpe when he substituted imaginal representation of the feared conditioned stimulus for actual exposure—a generalization based on very tenuous grounds. When two stimuli vie for competitive acceptance by the nervous system each one tends to diminish the other (Jonas, 1962). This mechanism, by the way, may account for the anaesthetizing effects of acupuncture. Gripping the arms of the chair while the dentist drills one's tooth is a familiar example. In one experiment carried out in a different context I exposed an agoraphobic patient to a truly terrifying movie sequence of a head-on collision filmed from inside the crashing car. After three run-throughs of the same scenes the patient made the following significant remark: 'Going through a car makes my fear seem so insignificant . . . But . . . well . . . (shrugging his shoulders) in that car crash . . . you die only once'. Thus the flooding need not be specific as long as it possesses similar anxiety-provoking potential as the phobia itself, and therefore it appears to be a non-specific counter-irritant.

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SCOTTISH AND ENGLISH SUICIDE RATES

Dear Sir,

The Scottish suicide rate has been lower than the English for seventy years, but the difference may be an artefact, the result of differing ascertainment procedures, the Scottish Crown Office placing more borderline suicides in the category 'undetermined deaths'. I argued (Barraclough, *Journal* (1972), **120,** 267–73) from the 1968 suicide statistics that the incidence of suicide in the two countries was probably the same.

The purpose of this letter is to show that the 1971 mortality statistics for violent deaths (quoted by permission from the General Register Office), the most recent available, are consistent with there being a *greater* incidence of suicide in Scotland than in England. (See Table Overleaf)

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