and verify, the theory on which these treatments (desensitisation, flooding with response prevention, modelling, etc.) are based. All this does not relate to the theoretical standpoint of the investigator; he is simply testing the hypotheses of different psychotherapists, and assumes a neutral standpoint, as is appropriate for the research scientist.

When Smith, Glass & Miller (1980), in their book on *The Benefits of Psychotherapy*, discover, on the basis of a meta-analysis of all published studies, that there is no relationship whatever between success of therapy and duration of therapy, or between success of therapy and duration of the training of the therapist, we must surely begin to doubt the wisdom of advocating lengthy training of therapists, or lengthy treatment of patients. I have argued (Eysenck, 1980) that all psychotherapy and behaviour therapy treatments, as well as placebo treatments and spontaneous remission effects, rely on identical psychological principles, namely those underlying Pavlovian extinction of conditioned emotional responses, and that the superiority of behaviour therapy is dependent on its recognition of these principles and their explicit use in designing the treatment. The various psychotherapies, placebo treatments and spontaneous remission effects make use of these principles in a random and indiscriminate manner, and are hence less successful. Also, orthodox Freudian therapy contravenes these principles, and hence frequently harms, rather than cures, the patient (Strupp *et al*, 1977). It seems important to me that these facts should be known, and that they should be taught to budding psychiatrists. There is still an aura of successful achievement surrounding psychotherapy which is quite unjustified by all the available evidence (Rachman & Wilson, 1980). Similarly, the vast majority still spend a great deal of time being taught principles of psychotherapy which the evidence suggests are, in fact, untrue. These are serious matters, concerning the health and happiness of countless individuals, and it is important that they should not be swept under the carpet but discussed and debated on a factual basis.

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Suicide in Physicians

Sir: In their paper on "Suicide in young doctors" (*Journal*, October 1986, **149**, 475–478), Richings *et al* revisit the troubling problem of the apparent vulnerability of members of the medical profession to suicide. During a sabbatical year spent in England, I used similar published and unpublished data recorded by the Office of Population Censuses and Surveys (OPCS) for 1970–72. Deaths among physicians and their wives from suicide, liver cirrhosis, accidents, poisoning, violence, and all causes combined were compared with deaths among other occupational groups (Sakinofsky, 1980*a*, 1980*b*). The standardised mortality ratio (SMR) for suicide in male physicians aged 15–64 was 335 and in those aged 65–74 was 155. Of the 15 occupational groups studied, male physicians (aged 15–64) ranked third for suicide, after male pharmacists (SMR 464) and unskilled labourers (SMR 370). Members of the political bureaucracy ranked lowest (SMR 34) after the clergy (SMR 51) and university teachers (SMR 56).

I was not able to study suicide in married female doctors owing to the peculiarity that married female physicians were at that time frequently classified by the registrars of deaths according to the occupations of their husbands! However, like Richings *et al*, I did find an excessive SMR (257) among unmarried female physicians aged 15–64 (based, however, on only 3 deaths recorded between 1970–72). Here again, far higher SMRs were recorded for female pharmacists (763 based on 3 deaths) and unskilled labourers (623 based on 18 deaths). Caution should obviously be used in interpreting SMRs based on such small numbers as are indeed also present in the data of Richings *et al*.

A further group investigated in my study was that of doctors' wives, and again I found excess mortality from suicide (SMR 458 based on 31 deaths aged 15–64) as well as from accidents, poisoning, and violence (SMR 322 based on 62 deaths). These ratios considerably exceeded those from deaths of wives of husbands with other occupations, including pharmacists (SMR from suicide 141 and from accidents, poisoning and violence 98).

It would be an oversimplification to suggest that excessive suicidal mortality in doctors and their wives is simply due to their special knowledge about lethal means as well as the ready availability of such means. However, the even greater suicidal ratio for male pharmacists clearly suggests that these must be significant factors. On the other hand, if being a doctor or a doctor's wife carries with it a special kind of distress loading, clearly the physician is not unique in this either. Being an unskilled worker may be a worse predicament. By the same token, there appear to be some professions (e.g., the clergy, politicians) that are spared excessive risks of suicide. If we are to understand why doctors and their wives are at higher risk for suicide, we might benefit from undertaking in-depth comparisons of their demographies, lifestyles and value systems with those of other occupational groups.

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Mania Following Bereavement

SIR: In his letter (*Journal*, August 1986, **149**, 244) Bridges raises interesting points apart from, rather gallingly, misreading our intendedly ironic "Freudian" reference (*Journal*, April 1986, **148**, 468–70). He claims that in opposing grief and mania we create a false paradox in our patient's manic sequel to bereavement. If it is argued that grief and mania are not opposites then, by Bridges' own statements, grief and depression should have no relation. The disjunction of grief and depression is a little more problematic. The phenomenological closeness of these conditions is embedded in DSM-III, which requires the phenomena of "major depression" not to be caused by grief (p. 214). I do not believe DSM-III necessarily embodies 'truth', but it does conform to a body of respectable opinion, as do concepts of pathological grief, and to the extensive literature on bereavement and depression. Grief may be a normal experience, but it is not clearly differentiated from depression except by the presence of bereavement or loss. Bridges' placement of "happiness" as the polar opposite of grief is no less suspect than our opposition of grief and mania, or his separation of grief and depression. Only a psychiatrist who dealt with the extremes of illness would have the luxury of seeing such clear separations.

One of the worst sins of the analytic movement was to treat the hypothetical entities of "defences" as real or phenomenological entities. Bridges refers to "mani[a]-an illness" with a conviction that similarly treats the hypothetical entity of illness as a real or phenomenological entity. Moreover, psychoanalytic theory offered trite and circular 'explanations' for many mysteries of human behaviour and thereby closed them to investigation for decades. To speak of "specific vulnerability/non-specific stress' creates an illusion of explanation which threatens to do the same.

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The Prognosis of Depression in Old Age

SIR: Baldwin & Jolley (*Journal*, November 1986, **149**, 574–583) point out that "much thought needs to be given to research methodology in this field." I could not agree more; the authors' retrospective study of case notes can hardly be regarded as rigorous or comparable with a prospective follow-up study of the type I conducted. It is well recognised that case note reports of outcome frequently bear no relation at all to the mental states discovered in face-to-face interviews and no doubt the same vagaries of reporting afflict doctors' notes in Manchester as they do in East London. Baldwin & Jolley did, however, interview those alive for their long-term study. They reported a mortality rate (35%) remarkably similar to the 37% mortality rate of the East London cohort over a 4 year period (in press). If the dead are excluded, then 40% of their patients remaining alive were either 'continuously ill" or suffering from "depressive invalidism". No different from Post's findings and not much different from mine!

It seems to me that where our methodology is similar, the results are similar, though perhaps they view the same results with more optimism. Baldwin & Jolley's pint pots are always half-full, whereas mine are half-empty!

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Depression in School Phobia

SIR: We note with concern the comments of Weinberg *et al* (*Journal*, March 1986, **148**, 335). Some of the phraseology suggests that their views may be influenced by considerations beyond the substance of the Newcastle method. They enunciate standards which are easier to apply to the work of others and also over-emphasise shortcomings which we have