seems a pity to try and understand panic attacks without any recourse whatsoever to the insights available from 90 years of psychoanalysis.

Gelder reminds us that Freud coined the term 'anxiety neurosis' in a very early (1895) paper entitled "The justification for detaching from neurasthenia a particular syndrome: the anxiety neurosis." Gelder summarises some biochemical and neurophysiological hypotheses and a theory involving hyperventilation. Turning to "psychological mechanisms", he does say that they may be equally complicated but "for the present discussion it will be enough to consider one important psychological component: the cognitive changes." This is the only hint that there might be any more to it, any more in the way of human experience relevant to the problem. In particular, there is no glimmering of what Freud's paper was about. The fact is that he was on the verge of discovering the unconscious, the interpretation of dreams and inventing psychoanalysis. In the particular paper quoted, he thought he had discovered something rather different, a theory of "actual neuroses", syndromes due to adverse sexual practices. Almost the entire paper to which Gelder refers is devoted to an exploration of the idea that "anxiety neurosis" is caused by frustrating sexual habits and by coitus interruptus in particular. He thought that neurasthenia, his other "actual neurosis", was caused especially by excessive and compulsive masturbation. These seemingly naive ideas may have made more sense in 1895 but were soon to give way to the larger insights into the workings of the unconscious that changed the shape of the planet for every literate person since. The early stumblings of a genius are fascinating to follow and Freud's *obiter dicta* had a way of getting into everyone else's language afterwards.

After all this it is rather astonishing that Gelder, asking whether the psychological component of neuroticism in panic attacks can be identified more precisely than cognitive theory permits, recalls only that "Freud (1895) stressed the anxious patient's preoccupation with fears of dying, a stroke, or loss of sanity." He adds that Beck *et al* (1974) added fears of heart disease and fainting, which sounds like an observation of almost sublime triviality – the list of expressed fears must be almost endless. Surely we could grasp Freud's point in first describing "anxiety neurosis": far from stressing the presenting symptoms, so oppressive to the patient, a psychiatrist does well to look deeper. It is a weakness of cognitive therapy that it tends to make psychology thoughtless and mindless. Of course, cognitive theory can and should take account of fantasy life and unconscious thought but too often it does not.

I find it piquant that in these very early papers Freud's approach is closer to that of the behaviour therapist than latter-day behaviour therapists seem to notice. He was then very interested in behavioural modification as a two-way interaction with thought modification and he certainly wasn't above telling people what to do. He must, after all, have been one of the astutest clinical observers and most powerful intuitive therapists ever, even though he did warn us of the dangers of *furor therapeuticum*. Furthermore, being originally a celebrated neurologist, he remained all his life deeply preoccupied with the mind–body interrelation, always wondering about the physical basis of mental processes. Perhaps we may look forward to Gelder's re-appraisal of Freud, the behaviour therapist.

Finally, Gelder gives the reference to Freud's paper as it appears in the *Collected Papers* (reprinted 1940). We probably share a sentimental attachment to those old editions but younger readers would be better referred to the Standard Edition of Freud, published jointly by the Hogarth Press and the Institute of Psychoanalysis. Strachey's translation has been criticised but remains a definitive master-work. The editorial introductions to every item are indispensable for anybody liking to place the text in its context and time.

SANDY BOURNE

The Tavistock Clinic
Tavistock Centre
120 Belsize Lane
London NW3 5BA

Psychotherapy and Placebo

SIR: Michael DeMowbray (*Journal*, November 1986, **149**, 666) returns to our debate concerning psychotherapy and placebo treatment by suggesting that "the allotment of the various therapeutic factors to the categories 'specific' and 'non-specific' . . . is purely arbitrary and relative to the theoretical standpoint of the investigator concerned". This surely is a complete mis-statement of the position taken by psychotherapists in general. Whether they embrace the principles of psychoanalysis or gestalt, or whatever, they start with a theory concerning the origins and nature of neurosis, and derive methods of psychotherapy from these principles. It is common knowledge that these principles vary widely from one school to another, and the fact that all are equally successful (or unsuccessful) and do not do better than placebo treatment surely demonstrates once and for all that the theories involved are erroneous, and that the treatment effects are in that sense non-specific. Similarly, the fact that behaviour therapy is significantly more successful suggests that its effects are specifically derived from,

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.

H. J. EYSENCK

Institute of Psychiatry
Denmark Hill
London SE5 8AF

References

SMITH, N. L., GLASS, G. V. & MILLER, T. (1980) *The Benefits of Psychotherapy*. London: Johns Hopkins University Press.

EYSENCK, H. J. (1980) A unified theory of psychotherapy, behaviour therapy and spontaneous remission. *Zeitschrift für Psychologie*, **188**, 43–56.

STRUPP, H., HALLEN, S. & GOMEZ-SCHWARTZ, R. (1977) *Psychotherapy for Better or Worse*. New York: Jason Arrenson.

RACHMAN, S. J. & WILSON, G. D. (1980) *The Effects of Psychological Therapy*. London: Pergamon Press.

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