principle, the fact that a medication with a cardiovascular therapeutic action should prove effective in a cardiovascular pathology with secondary psychiatric symptoms should not surprise anybody; on the contrary, what surprises us is the fact that the authors did not even mention that possibility in the discussion of their case.

LUIS M. IRUELA
VICENTE IBAÑEZ-ROJO

Department of Psychiatry
Universidad Autonoma de Madrid
Clinic Puerta de Hierro
San Martin de Porres, 4
28035 Madrid, Spain

Psychogenic amnesia?

SIR: Domb & Beaman (*Journal*, March 1991, **158**, 423–425) reported the case of "Mr X", whom they consider to be a psychogenic amnesic.

I agree with their assertion that Mr X's loss of personal identity and the circumstances surrounding his admission to hospital suggest a psychogenic amnesia, possibly representing a flight from suicide. There is, however, another rather puzzling aspect to this case, namely, a persisting and selective failure to learn the difficult paired associates of the Wechsler Memory Scale (WMS), despite improvements in performance on other subtests of the WMS. In the experience of our neuropsychological group, this pattern is frequently seen in patients with organic amnesias of bilateral hippocampal origin (Walsh, 1985, 1987). It is also seen in the chronic phase of recovery after left posterior cerebral infarction (Ishikawa *et al*, 1988), and in patients with left temporal lobe epilepsy in the presence of hippocampal sclerosis (Saling *et al*, unpublished).

In view of the neuropsychological findings in this case, it would be interesting to know the nature of Mr X's two previous suicide attempts. Is it possible that some cases of functional retrograde amnesia represent massive psychological elaboration around a kernel of organic involvement?

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MICHAEL M. SALING

The University of Melbourne
Parkville 3052
Australia

Clinical interviews with children and adolescents

SIR: It is not customary for authors to write to editors commenting upon reviews of their books and indeed until now I have never done so. However I feel that some comment is needed about Dr Dora Black's review of *Clinical Interviews with Children and Adolescents*.

Dr Black says that "the author seems never to have got clear in his mind what his aims are". I believe that the aims of the book were stated clearly in the introduction. Re-reading this confirms my belief.

She goes on to say that I seem "to be considering therapeutic interviews as well as diagnostic ones" but that I miss them off the list of reasons for interviewing children. This statement is just wrong. Treatment interviews, defined as aiming to promote rather than impose change, are clearly listed on page 12. Dr Black seems to have missed the point I make, also on page 12, contrasting 'manipulative' interviews – which attempt to impose change – with 'treatment' interviews, which aim to promote it.

Earlier in the review Dr Black says that the idea of my book "is an original one". This is not the case. A book entitled *Interviewing Children and Adolescents* was published by John Rich in 1968 and is cited by me. Indeed, the classification of interviews upon which Dr Black comments is derived, with acknowledgement, from Rich's book.

I find it difficult to avoid the conclusion that Dr Black's review was written after only a cursory look through the book.

PHILIP BARKER

Department of Psychiatry
Alberta Children's Hospital
1820 Richmond Road S.W.
Calgary, Alberta
Canada T2T 5C7

AUTHOR'S REPLY: I certainly read Professor Barker's book from cover to cover. Our differences hinge on the fact that the whole book is devoted to what Rich called fact-finding interviews, and the author states that "psychotherapy techniques themselves fall outside the scope of this book" (p. 12) yet in the chapter on "Termination" there is advice about resolving attachment to the therapist. It is this that led me to state that the aims were not clear. Perhaps it would have been better to say that the aims were stated but a trainee would not emerge from reading the book with a clear idea of whether the interviews described were for assessment or therapy. As I said originally, the book is of value as the distillation of the experience of a sympathetic and skilled

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clinician. I am sorry I got it wrong when I said it was an original idea.

DORA BLACK

The Royal Free Hospital
Department of Child and Adolescent Psychiatry
Pond Street
London NW3 2QG

Portrayal of women in advertisements

SIR: The paper by Ashton (*Journal*, May 1991, **158** (suppl. 10), 30–35) on psychotropic drug prescribing for women, quotes an American study (Prather & Fidell, 1975) which showed that advertisements for psychoactive drugs show significantly more women. The same conclusion was reached by Penfold & Walker (1984) who also looked at North American journals.

In a retrospective study of our own, we have examined all advertisements for psychotropic drugs appearing in the *British Journal of Psychiatry* over the last 30 years and surprisingly find the situation to be different. We looked at the three categories of antidepressants, anxiolytics, and antipsychotics, and found the portrayal of gender within each group of advertisements to be in line with accepted prevalences of illness (e.g. Kendell & Zealley, 1988), i.e. there was no gender bias in *numbers* of women shown.

What we did find, however, was a marked difference in the *portrayal* of women. Whereas depressed or anxious men tended to be shown at work, or worrying about deadlines, women were more often portrayed in the home or with children. In 30 years, only one woman has been shown in a professional role – as a teacher in 1961! Similarly there has never been a female doctor portrayed – despite the fact that about one-third of members of the Royal College of Psychiatrists are women.

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PRATHER, J. E. & FIDELL, L. S. (1975) Sex differences in the content and style of medical advertisements. *Social Science and Medicine*, **9**, 23–26.

MATTHEW JELLEY
JOHN OWEN

Fromeside Clinic
Blackberry Hill
Stapleton
Bristol BS16 1ED

Independent evaluation of new drugs

SIR: Healy (*Journal*, June 1991, **158**, 737–742) is right to draw our attention to the weaknesses of the Hamilton Rating Scale in the assessment of response to antidepressant medication, and to question the marketing policies of drug companies to which clinicians are exposed without the benefit of 'independent trials'. While dilating upon the link between dopamine and serotonin he does not consider the relationship between noradrenaline and serotonin. The implication is that theories of depression involving these two neurotransmitters are mutually exclusive. Close anatomical relationships with reciprocal connections have been demonstrated between the brainstem raphe nuclei and the locus coeruleus suggesting closely related systems (Descarries & Leger, 1978; Fuxe *et al*, 1978).

Sulser (1987) has described the complex relationship between the changes in noradrenergic receptors in response to antidepressant medication and the role of serotonin in these changes. In drug-free animals, lesions of the serotonin system produce an increase in the density of β-adrenoceptors. In the absence of serotonin, downregulation of the β-adrenoceptor in response to antidepressants does not occur. This downregulation is a common feature of many treatments for depression (Leonard, 1988). Acute reductions in serotonin may reverse antidepressant-induced downregulation. The mechanism of interaction between the two may be modulated by 'second' or 'third' messenger activated protein kinases (Sulser, 1987).

Leonard (1988) suggests that the adaptive changes in the serotonin and noradrenergic receptors in response to chronic antidepressant administration have a similar time course to the improvement seen clinically in the depressed patient.

Clearly the story is a complex one and much remains to be clarified. The marketing strategies of the drug companies exploit these areas of doubt. While agreeing with Healy that there is a need for 'independent' studies and critical evaluation of these, this cannot be achieved by ignoring the wealth of evidence that serotonin in conjunction with noradrenaline has an important role in the molecular biology of depression.

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