

1982). In 141 patients with MVP Hartman *et al* found that 22 (16%) had panic disorder (PD). However, the prevalence of PD among the *relatives* of these patients (a population with minimal selection bias) was much lower: only 3% of relatives with or without MVP had evidence of PD. This is similar to the figure reported in the general population, and suggests that the high prevalence of PD in hospital populations is due to the selection of highly symptomatic individuals.

Two cardiologists (Leatham and Brigden, 1980) also criticized the ill-considered attempts to ascribe all sorts of properties to the prolapsing mitral valve. They were impressed by the absence of symptoms in those patients seen for the first time who were unaware of the existence of disease. In an attempt to explore the apparent association between clinical findings and reported complaints, Procacci *et al* (1976) identified all women on an air force base with an auscultatory click, murmur, or both and found that the symptoms of palpitations, dizziness, weakness and chest pains were as common in the group with as in those without the auscultatory abnormalities. These results, taken together with those of Hickey *et al* (1983), suggest that the underlying mechanism that causes MVP is not additionally responsible for psychological symptoms. Eight years after Wooley's editorial we are in a position to say that the association of MVP and anxiety disorders is no longer an enigma: it merely reflects the chance association of two exceedingly common conditions.

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#### DREAMS AFTER AMPUTATION

DEAR SIR,

I enjoyed reading the article by Frank *et al* (*Journal*, May, 1984, **144**, 493-497) on psychological response to

amputation as a function of age and time since amputation. I was particularly interested in their discussion of the importance of change in body image following amputation. It had come to my attention, in talking with persons who have had amputations, that a potentially useful measure of psychological response may be body image during dreams.

A 37 year old farm worker was sawing logs when one of the logs became caught in the saw. In an attempt to free it he became entangled in the log and had both arms amputated, one near the shoulder and the other just above the elbow. After several weeks of rehabilitation, physical therapy and occupational therapy he was fitted with two artificial arms and hook hands. He was then discharged home. In follow-up visits he continued to report difficulties in adjustment and acceptance of his amputations. Particularly, he observed difficulties in expressing physical affection to his children, his wife and in taking care of his toileting needs. As follow-up continued he continued to make excellent progress. He then reported that he knew the time when he had accepted his amputations because he began to dream of himself with hook hands.

This has been reported in a number of other patients I have seen who have suffered amputations. Many will no longer dream of themselves as running or walking but rather wheeling along in their wheelchairs or see themselves in their dreams with an artificial limb, these patients appear particularly well adjusted. To my knowledge, investigations in this specific area have not been completed. I think it is a particularly fruitful area to pursue from a research standpoint. From a clinical standpoint, it may also be useful in the ascertainment of individual acceptance of amputation and physical handicap. Finally, it would appear that, to a degree, this change in body image is a function of time since amputation.

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#### DEXAMETHASONE SUPPRESSION TEST AND RESPONSE TO ANTIDEPRESSANT DRUGS

DEAR SIR,

Beckmann *et al* (*Journal*, April 1984, **144**, 440-441) claim that in their study comparing response to amitriptyline and nomifensine, there was a preferential response of DST positive depressives to amitriptyline and of DST negative depressives to nomifensine.

In order to refute the null hypothesis that there is no difference in the distribution of treatment response to either drug among DST suppressors or non-suppress-

sors, statistical evaluation of the data is in order. Comparison of response to amitriptyline and nomifensine among non-suppressors does not reach statistical significance ( $P = 0.113$ , Fisher's exact test, two-tailed; use of the one-tailed test yields  $P = 0.057$  but the hypothesis does not warrant a one-tailed test). Comparison of response to these two drugs among suppressors does not reach statistical significance either ( $P = 0.65$ ). Lastly, comparison of response of non-suppressors with that of suppressors gives  $P = 0.208$  for those patients given nomifensine, and  $P = 0.35$  for those patients given amitriptyline. Again all tests are two-tailed. It is therefore hard to see what grounds Beckmann *et al* have for their speculation about biochemical subgroups of depression.

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#### INFORMED CONSENT

DEAR SIR,

My paper 'On Informed Consent' (*Journal*, October, 1983, **143**, 416-418) was intended to be a contribution to discussion rather than a full review of the subject. Perhaps because of its omissions C. J. F. Kemperman in his letter (*Journal*, March 1984, **144**, 331) appears to have misunderstood what I was trying to say. I did not suggest that patients should not be informed fully about what was being done to them. On the contrary, I thought that I had made clear that I always do my best to explain everything to them and that I expected other doctors to do the same.

My point about the lawyers' myth of 'informed consent' is twofold. First, it implies that the patients have made a rational and fully considered decision on the basis of the information given to them. As most patients do not know even the most elementary facts about biology, they cannot understand what is said to them. Even if they did, their emotional state is such that they are not really capable of making proper judgements. Second, 'informed consent' seems to imply that the patient has accepted some of the responsibility for the risks (either of treatment or research). It is my conviction that the doctor or investigator cannot be relieved of any of his responsibilities towards the patients and that the profession should make this quite clear. This responsibility is not altered in any way by an Ethical Committee accepting a research protocol.

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#### NEUROLEPTIC MALIGNANT SYNDROME

DEAR SIR,

Dr Hari Singh in his letter (*Journal*, July 1984, **145**, 98) draws attention to the hot weather contributing to the development of signs and symptoms of the neuroleptic malignant syndrome.

We have reported a case of the syndrome in other conditions. Our patient, a 26 year old man, had been on a long acting depot preparation (flupenthixol 40 mg. i.m. fortnightly) as maintenance treatment for schizophrenia. His symptoms of hyperpyrexia, marked rigidity and loss of consciousness were precipitated by working outside in cold weather while clearing the snow. His chest was clear.

This is in marked contrast to the case described by Dr Hari Singh. Our patient was admitted to the medical ward and treated with parenteral procyclidine (10 mg. i.m. three times daily) along with supportive measures. After recovering from the neuroleptic malignant syndrome he was recommended to remain on parenteral flupenthixol depot and on one year's follow up remains symptom free.

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#### MIANSERIN WITH WARFARIN

DEAR SIR,

I write concerning a letter from Dr. Warwick and Dr. Mindham (*Journal*, September 1983, **143**, 308) reporting a possible reaction between mianserin and warfarin.

I suspect there has been typographical error in reporting the prolongation of the prothrombin time to 25 seconds giving a ratio of 4.6. If the ratio is correct then one would expect the prothrombin time in seconds to be about 50 seconds and not 25 seconds.

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Professor Mindham writes that his letter gave 25 seconds in error, the correct figure being 55 seconds, ratio 4.6. *Editor*.

#### PHANTOM HEAD

DEAR SIR,

We were very interested to read the report of primary delusional bicephaly by Ames (*Journal*,