Tricyclic Antidepressants

SIR,—I am worried by one sentence in the article on tricyclic antidepressants (13 April, p. 102): "Jaundice, which is very uncommon, is of the cholestatic type similar to that found in chlorpromazine." This is misleading.

Jaundice is one of the more frequent side-effects reported from imipramine and imiprityline, the most widely used members of the group. Its incidence is probably related to

the amount of drug prescribed, and is only likely to be "very uncommon" with the newer compounds. I doubt, too, if the jaundice is always of the chlorpromazine type; this implies a cholestatic reaction which is usually benign. Some patients almost certainly have a far more serious hepatic reaction, and death can result from liver-cell failure.—I am, etc.,

Dudley Road Hospital,
Birmingham 18.

A. PATON.

Artificial Cardiac Pacemakers

SIR,—The summary of Dr. E. Sowton's admirable paper (6 April, p. 11) on pacemakers is misleading. As is clear from the text he has reviewed only *implantable* pacemakers. His report presumably includes the data which were sent to him describing the experience of the Sheffield Cardiothoracic Unit. With the implanted type of pacemaker we have had failures from wire fractures, pacemaker failure (causing a death), and sepsis.

For the past three years we have used the induction type of pacemaker, described by Abrams and his colleagues,¹ manufactured by Lucas Ltd. This instrument consists of an implanted coil and electrodes, the battery-operated stimulator and primary coil being carried externally. We have had no failure of the implanted unit. The stimulator and its lead are accessible at all times, and all patients carry a spare set in case of failure. They learn a simple drill in the ward after the internal unit is inserted, and are encouraged to regard themselves independent of the centre. All patients living at a distance either consult their own doctor if in difficulties or return faulty components to Lucas themselves. In a region in which patients may live more than 80 miles from the special centre this is important. Other advantages of the induction unit are the controllable rate which is used by our more physiologically minded patients, and the ease with which it can be stopped. Sinus rhythm has returned in two patients while being paced by the induction unit, and a possibly fatal competition between the natural and implanted pacing units has been avoided by removing the artificial stimulus.

In the present state of electronic miniature stimulators (Dr. Sowton's survey revealed 57% failed) it seems rational to put the electronics outside, where the informed patient can get at them at once if something goes wrong.—I am, etc.,

Sheffield.

DAVID VÉREL.

REFERENCE

- ¹ Abrams, L. D., Hudson, W. A., and Lightwood, R., *Lancet*, 1960, 1, 1372.

SIR,—The recent survey on artificial cardiac pacemakers (6 April, p. 11) highlights the fallibility of totally implanted pulse generators, and affords support for the use of the Lucas inductively coupled system.

We use this inexpensive system, with an endocardial electrode, and find it very suitable for permanent pacing in an area where patients live many miles from skilled help. The external pulse generator is simple and uses U2 cells, which are widely available and easily replaced. The patient has a spare generator and primary coil as an immediate remedy should faults develop in the external

components. Faulty components may be exchanged by post or by a technician on a service visit. Should "exit block" develop, the power delivered to the heart may be increased by more direct coupling of the primary and secondary coils, and, if necessary, by switching to the "high power" setting of the generator. The patient may select the pacing rate appropriate to his level of activity and check battery status as necessary. Should sinus rhythm return, the external generator and coil may be removed, leaving the secondary coil and electrode for future use if necessary. With a little persistence even the most non-mechanically minded patients learn to manage this system without difficulty or danger.

We find this system, which is not mentioned in your leading article (6 April, p. 2), satisfactory in providing reliable pacing at a modest cost. We hope the Birmingham team who pioneered this system will make their considerable experience more widely known and that in consequence inductively coupled pacing may receive the more extensive trial it deserves.—We are, etc.,

East Anglian Regional
Cardiac Unit,
Papworth Hospital,
Cambs.

J. H. N. BETT.
D. W. EVANS.

REFERENCE

- ¹ Abrams, L. D., Hudson, W. A., and Lightwood, R., *Lancet*, 1960, 1, 1372.

SIR,—May I correct the reference to corticosteroid therapy made by Drs. B. W. Lassers and D. G. Julian (20 April, p. 142)? The article they quote¹ reported the treatment of cases of heart block complicating acute myocardial infarction with oral steroid therapy. The shortening of the P-R interval by corticosteroid therapy, to which they refer, was reported by myself in a later article,² in which I also reported the advantages of a high-dosage, parenteral, hydrocortisone regimen in the treatment of "acute" heart block over the earlier series. In a group of 17 cases of complete heart block complicating acute myocardial infarction treated with parenteral steroids two died within three hours of onset, but the remaining 15 were restored to sinus rhythm within 24 hours. There were two late deaths in this group (pulmonary embolism at 10 days and fresh infarct at five weeks) but no recurrence of heart block, and 13 were discharged home. Continuing experience has not caused me to change this regimen.—I am, etc.,

Royal Alexandra Infirmary, J. L. C. DALL.
Paisley.

REFERENCES

- ¹ Dall, J. L. C., and Buchanan, J., *Lancet*, 1962, 2, 8.
² Dall, J. L. C., *Brit. Heart J.*, 1964, 26, 537.

Blood Pressure in Sleep

SIR,—I have been interested to read the article by Dr. Ian Oswald and others (30 March, p. 796), and in particular one comment that is made, that nitrazepam suppresses paradoxical sleep.¹

My own interest is in the effect of sleep on the blood pressure. By continuous recording of the blood pressure I have noted the gradual lowering and the periods of recovery of the blood pressure to near normal in natural sleep. In patients sedated with nitrazepam there has been little or no effect on the blood pressure—that is to say, there is just as high an incidence of blood pressure recovery periods as there is in unsedated sleep. I have not found this in patients sedated with barbiturates or with methaqualone—in these there is lowering of the blood pressure with few recovery periods. MacWilliam² described two different conditions of the state of sleep: "undisturbed or sound sleep" with a lowering of the blood pressure, heart, and respiratory rate, and "disturbed sleep," modified by reflex excitations, dreaming, nightmares, sometimes with extensive rises in blood pressure, increases in heart rate, and various reflex effects. I have assumed that his "disturbed sleep" was the same state later described by Jouvet³ as "paradoxical sleep."

If nitrazepam suppresses paradoxical sleep I would have expected few blood pressure recovery periods, but this is not the case. Would Dr. Oswald say I am wrong in assuming that MacWilliam's "disturbed sleep" is similar to Jouvet's "paradoxical sleep" and that the recovery periods of the blood pressure coincide with the rapid eye movements of paradoxical sleep?—I am, etc.,

A. W. RAFFAN.

Royal Infirmary,
Aberdeen.

REFERENCES

- ¹ Oswald, I., and Priest, R. G., *Brit. med. J.*, 1965, 2, 1093.
² MacWilliam, J. A., *Brit. med. J.*, 1923, 2, 1196.
³ Jouvet, M., Report on Current Research presented at Annual Meeting of Association for Psychophysiological Study of Sleep, 1965.

Post-infarction Electrocardiogram

SIR,—Solomon and Shapiro¹ have described seven patients who showed a temporary improvement or reversion to normal in their electrocardiograms after myocardial infarction. I wish to report a further case.

The patient was a 54-year-old male who had been healthy in the past. Three days before admission he had suffered a minor incident of chest pain which lasted two hours. On the day of admission an urgent request for a visit was received at 6 a.m. He had awoken with severe pain across the lower chest, which radiated down the inside of the right arm. He was found to be shocked and sweating. The diagnosis of myocardial infarction was subsequently supported by both clinical and laboratory evidence.

The first electrocardiogram, done three hours after the onset of pain, showed typical injury changes with elevation of the ST segments in standard leads II, III, and AVF. A second tracing taken six hours after the incident was, by the majority opinion of a number of cardiologists, within normal limits. After 12 hours the usual progression had been resumed with inversion of the

T wave in lead III. A tracing taken next day showed the mature picture of a posterior infarct.

There are a number of possible explanations for this phenomenon, but a failure to find any reference to it in the standard textbooks prompts the submission of this report.—I am, etc.,

Camberley, Surrey.

A. M. W. PORTER.

REFERENCE

- ¹ Solomon, R. B., and Shapiro, H. H., *Amer. Heart J.*, 1966, 71, 582.

Nursing Attachment Scheme

SIR,—In view of the possible increase in nursing attachment it may be relevant to consider the present schemes in more detail in order to determine their uses and possible abuses. The full value of any such scheme must surely be measured by the benefits it confers on those whom it serves in relation to its total cost and its efficient use of manpower, rather than by its power to shift a work load or its provision of job satisfaction for some of its participants. It is important to know if the improved communications described by Dr. J. F. Warin (6 April, p. 41) have resulted in improvement in local authority services, and, if so, whether improved communications could be introduced by less radical means or means less costly in terms of skilled woman power.

There is a shortage of general practitioners, and many of them are grossly overworked. There is also a shortage of health visitors, and many of these are also grossly overworked, but under the direct administration of the local health authority their work load can be planned so that priorities are met and deficiencies known and measured. If there is fault in the present system of health visiting this is not necessarily due to the health visitor, her work load, or the system itself, but to the lack of appreciation of priorities. Is the general practitioner in a better position to organize her priorities, or is there a danger that in an attachment scheme the health visitor becomes a buffer between those who contract to provide a medical service and those who are duty bound to provide ancillary nursing care for the community as a whole, with particular reference to disease prevention and health promotion?

Dr. Warin has pointed out that a successful attachment scheme revealed the need for an increase in the establishment of health visitors. This need has been felt by health visitors themselves for a very long time, but certainly not for the purpose of "regular visiting" of elderly patients or "follow-up visits in connexion with the common infectious diseases."

Of course this kind of work will lighten the work load of general practitioners, but is it the work for which health visitors are being trained? Does it lead to a more efficient health-visiting service, does it help to lighten the health visitors' case load, or is their work of so little importance that these questions are of no consequence?—I am, etc.,

MILDRED F. DAVEY,
Superintendent Health Visitor,

Health Department,
Town Hall,
Merthyr Tydfil.

Royal Malady

SIR,—When I read that Professor C. E. Dent was attempting to sum up the recent correspondence on the royal malady I expected an objective and fair summation of the various points of view, but in this I have been disappointed. It is for this reason that I write to you again.

It is quite unfair of Professor Dent to call upon physicians with experience in this field, as he did (3 February, p. 311), and then to throw out all the opinions with the exception of those of Dr. G. Dean (17 February, p. 443), in spite of the fact that I admire Dr. Dean's work in this field. It is of course unrealistic to state that "only one clinician has really adequate and reliable knowledge of variegate porphyria." This type of old-fashioned authoritarianism does an injustice to Professor Dent himself and takes no account of the clinicians in U.S.A. and in this country who have had first-hand experience of this disease, not to mention Professor L. Eales and Dr. E. B. Dowdle, of Cape Town, whose carefully reasoned letter (30 March, p. 841) I applaud, even though it comes out against the hypothesis. The variegate type of porphyria has become increasingly recognized in this country, and we have already sufficient knowledge of it to say that quite severe attacks can be provoked by factors other than drugs. There was no suggestion in my letter (24 February, p. 509) that acute porphyria and porphyria variegata are not two diseases, but they do share a common enzyme defect, and the clinical pattern of the acute attacks is the same in both conditions, as are many of the provoking factors.

By inference Professor Dent suggests that the proponents of the theory consider that this has been proved. My own position, stated in my previous letter (24 February, p. 509), and I sense that of many others, is that this is a hypothesis which has certainly not yet been proved, but which has been rendered more likely by the investigations of Professor C. Rimington and his colleagues (6 January, p. 7). Professor Dent states that he was "most distressed to note that one historian seemed to have swallowed the story hook, line, and sinker." May I reassure him that historians are a hard-headed lot, quite able to look after themselves in the jungle of historical theory.—I am, etc.,

Department of Medicine, A. GOLDBERG.
University of Glasgow.

Use of Amphetamines

SIR,—I must protest about the attitude of your "expert contributors" towards the amphetamines (20 April, p. 164). The statement "The amphetamines are of little use in any but the mildest cases of endogenous depression" could not have been phrased more dangerously. Are your experts not aware that mild endogenous depression is the commonest encountered by the general practitioners? As it is written, the statement made by the "expert contributors" suggests that amphetamines *should* be used for the treatment of mild depression.

Why on earth cannot these contributors say quite clearly that there is no place for the use of amphetamines in the treatment of depression? Do they in fact use amphet-

amines themselves? And if they do can they justify the risk of addiction?

Surely the time is long past when any of us can properly sit on the fence and watch a growing addiction problem.—I am, etc.,

Oldham, Lancs.

GARETH LLOYD.

Hiatus Hernia Box and Scoliosis

SIR,—Mr. J. Lister describes radial nerve palsy due to direct pressure from the edge of a plastic box used for the treatment of hiatus hernia (9 March, p. 643).

I am concerned about the possible effects of this hiatus hernia box on the vertebral column. Infantile idiopathic scoliosis is a lateral curvature developing in the first three years of life in a spine without demonstrable abnormality. It is commoner in boys than girls, commoner to the left than the right, progresses usually to 15° or 20° at the most, and then the great majority resolve spontaneously. Why most resolve is not known, but probably the normal development of power and tone of the spinal musculature plays a part.

We have recently reviewed 60 children with this diagnosis and found that 40 resolved within the first 18 months of life to become normal children. The 20 children whose curves had not resolved at 18 months are made up of 13 in whom a possible cause for non-resolution could be found (seven with mental retardation, two with congenital abnormalities, and four with double curves), and seven in whom none of these features were present. Of these seven three had oesophageal reflux in early infancy and had been nursed for long periods in a hiatus hernia box. Of these three two developed curves of 25° and did not resolve until aged 3½, and one deteriorated.

This one, a boy, is known to have had a normal spine when radiographs were taken at 2 months of age and revealed a hiatus hernia. He was nursed in a box until 9 months old because it was impossible to prevent vomiting any other way. His scoliosis then was 60°. At the age of 3, when his hernia was repaired by a transthoracic