fair to say that while the majority of plant biochemists are not prepared to accept his singularly personal view of photosynthesis they have hesitated, because of his undoubted eminence, to dismiss it entirely. While we ourselves are not qualified to comment, we suspect that his theory concerning the origin of cancer cells¹ might promote an entirely similar reaction. Nevertheless we feel compelled to draw attention to a chemically induced condition which appears to be remarkably consistent with his proposals. We refer to the increasing number of reports^{2,3} on the toxic effects of methyl viologen and related bipyridyls such as the weed killers Diquat and Paraquat. It is now well established that poisoning by these agents is characterized by a proliferation of the epithelium of the lung.^{4,5}

Methyl viologen can accept electrons from reduced coenzymes and carriers of appropriate redox potential and then undergo rapid reoxidation at the expense of molecular oxygen which is in turn reduced to hydrogen peroxide. In the absence of sufficient catalase, accumulation of peroxide will itself be damaging, but the point that we wish to emphasize is that near anaerobiosis is also easily established. Thus in some oxygen-producing photosynthetic systems the addition of methyl viologen is rapidly followed by a fall in oxygen to a concentration at which it can no longer be readily detected by polarographic measurement. Therefore, even in a well-aerated oxygen-consuming system, such as lung epithelium, methyl viologen (because of its rapid autoxidation and affinity for oxygen) might also be expected to promote near anaerobic conditions within the cells. Oxygen diffusing into the cells would then be consumed as long as the methyl viologen continued to undergo cyclic reduction and reoxidation. External administration of oxygen would facilitate this process. The effect of cycling would be to promote reoxidation of reduced coenzymes⁶ in such a way that the normal processes in which energy is conserved as ATP would be by-passed. As we understand it, this is also the crux of Warburg's theory of carcinogenesis.

It may be that this striking parallel (between Warburg's proposals and this chemically induced proliferation of a well-aerated tissue) is entirely fortuitous, but at least it would appear to merit further investigation.—We are, etc.,

D. M. STOKES.
D. A. WALKER.

Botany Department, Imperial College,
London S.W.7.

REFERENCES

- 1 Warburg, O., *Science*, 1956, **123**, 309.
- 2 *British Medical Journal*, 1969, **4**, 817.
- 3 Fennelly, J. J., Gallagher, J. T., and Carroll, R. J., *British Medical Journal*, 1968, **3**, 722.
- 4 Bullivant, C. M., *British Medical Journal*, 1966, **1**, 1272.
- 5 Matthew, H., Logan, A., Woodruff, M. F. A., and Heard, B., *British Medical Journal*, 1968, **3**, 759.
- 6 Gage, J. C., *Biochemical Journal*, 1968, **109**, 757.

Sleeping Pills

SIR,—Your leading article (8 August, p. 296) ends: "... it has become increasingly clear that good clinical practice requires every doctor to undertake a searching

review of his prescribing of hypnotic drugs."

In my view we have been soft-peddalling this aspect of prescribing long enough. I therefore propose to stick my neck out with the following pronouncements.

Dependence on sleeping pills is a condition created and maintained by doctors.

There is no condition known to medicine for which the sole treatment is a sleeping tablet every night for an indefinite period.

Any doctor prescribing sleeping tablets in this way is giving unnecessary and possibly dangerous treatment—alternatively he is giving inadequate treatment.—I am, etc.,

A. LEWIS.

London W.9.

Heparin and Haemolytic-uraemic Syndrome

SIR,—Heparin therapy in the haemolytic-uraemic syndrome is of theoretical value by preventing fibrin deposition in the kidneys and other organs. Its efficacy in this condition is by no means proved, however, and its use after renal biopsy and/or in the presence of thrombocytopenia and bleeding may occasionally lead to problems greater than those caused by the disease itself. The suggestion of Drs. M. W. Moncrieff and E. F. Glasgow (25 July, p. 188), based on only three cases, that heparin was beneficial must be interpreted with some caution. In the three cases discussed evidence is lacking that heparin was of benefit in terms of patient survival or histological improvement, and indeed it may have contributed to the death of the first patient. It is probable that heparin must be given in the earliest stages of the disease to be of benefit, and when administered after afferent arteriolar thrombosis has occurred may have little or no therapeutic effect. It is also of interest that two patients in the series of Clarkson *et al.*¹ and 14 out of 22 in the series of Gilchrist *et al.*² recovered without heparin therapy.

In a small series of six adult patients recently studied in Edinburgh evidence has been accumulated that in vivo platelet aggregation and impaired fibrinolysis may play a significant role in the natural history of this condition. Heparin is unlikely to interfere with these factors.

The evaluation of heparin therapy in the haemolytic-uraemic syndrome must therefore depend on further knowledge of the type of coagulation abnormality involved and better understanding of its aetiology. It seems possible that in some patients drugs directed at suppressing tissue damage or platelet aggregation may be more appropriate, whereas in others some form of thrombolytic therapy would be indicated.—We are, etc.,

A. R. CLARKSON.
V. FUSTER.
J. D. CASH.

Department of Medicine,
Royal Infirmary,
Edinburgh.

REFERENCES

- 1 Clarkson, A. R., Lawrence, J. R., Meadows, R., and Seymour, A. E., *Quarterly Journal of Medicine*, 1970, **39**, 227.
- 2 Gilchrist, G. S., Lieberman, E., Ekert, H., Fine, R. N., and Grushkin, C., *Lancet*, 1969, **1**, 1123.

Examination of the Prostate

SIR,—Your leading article on prostatic syncope (11 July, p. 61) led to my reconsideration of the orthodox positions of the patient during prostatic palpation. A search of the literature revealed many references to the dorsal, the kneeling, and the standing-leaning-forward positions, and to the *left* lateral position. To my surprise I was hard pressed to discover a mention of the *right* lateral position, which I prefer.

The positions of the patient in which prostatic palpation is most easily, satisfactorily, and comfortably done are kneeling or standing-leaning forward. If for any reason these positions cannot be used, then the position of preference is the *right* lateral position. I find it hard to believe that the *right* lateral position is not widely used, but if it is then it has passed without remark.

A brief consideration will show that in the kneeling and standing-leaning-forward positions the curve and the pulp end of the examining finger fall naturally to face the prostate. So too in the dorsal position, in which bimanual examination is easiest. However, in the *left* lateral position the opposite is the case for a right-handed examiner; inserted naturally here the extensor convexity of the finger and the finger nail face the prostate. In this circumstance it is necessary to rotate the examining finger, hand, and forearm anticlockwise through 180 degrees, so as to bring the flexor (and flexible) concavity of the finger and its pulp to face and explore the prostate and adjacent structures. This is an awkward manoeuvre which is for the examiner both uncomfortable and inefficient as it is not to easy to move the examining finger freely and to appreciate well what it is feeling. In this position there is a tendency for the examiner to move round the end of the couch and to go to some part of the way to standing on his head.

All these disadvantages are avoided if the patient, after emptying his bladder, lies on his right side with his knees tucked up towards his chest, his head to the right handed examiner's left. In this right lateral position palpation of the prostate is comfortable, easy, and effectual.—I am, etc.,

NEVIL H. SILVERTON.

Leeds 17.

Laboratory Diagnosis of Rubella

SIR,—In your excellent leading article (1 August, p. 237) on the subject of rubella diagnosis there is an assertion that centrifugation is the method of choice in identifying IgM specific for rubella virus. This may deter many clinical virologists from undertaking early diagnosis of this disease during pregnancy. Moreover, I disagree with it.

Experience during the past two years has taught us in this department that assay of specific antibody fractions by immunofluorescence is an accurate and convenient clinical method. Indeed, you have already published some data from Belfast on such antibody levels in uncomplicated and complicated rubella (18 July, p. 130) though full records of the diagnostic value