ultraviolet radiation cause melanoma in man. Further, there is no direct evidence that 5-methoxypsoralen, which is used in suntan preparations, is carcinogenic or mutagenic, although there are reasons to believe that it might be.² It would be desirable to investigate whether secular changes in sales of suntan preparations containing psoralens are associated with trends in melanoma incidence and whether prior use of such preparations appears as a risk factor in case-control studies.

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¹ Stern R S, Thibodeau L A, Kleinerman R A, *et al.* *N Engl J Med* 1979;300:809-813.

² Ashwood-Smith MJ. *Br Med J* 1979;ii:1144.

Supplies of Trilene

SIR,—Last July we announced our intention of discontinuing supplies of Trilene at the end of 1980. The production problems that precipitated that notice have been resolved. In response to the many requests from anaesthetists that we should continue to produce Trilene, we can now assure supplies for at least the next three or four years and, we hope, beyond that.

Supplies will be available through the usual distributors, but if your readers should have any difficulty a note or a telephone call to me will ensure that the matter is dealt with promptly.

A BARNES

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Poststreptococcal glomerulonephritis

SIR,—Your leading article on poststreptococcal glomerulonephritis (17 November, p 1243) reviews two recent papers with apparently conflicting views. The first of these, by Baldwin¹ from New York, challenges the belief that the condition follows a benign course in most patients, while the work of Lien *et al*² from South Australia provides support for the more traditional view.

We believe, however, that the data in these papers are compatible. Although 60% of Baldwin's patients show evidence of normal renal disease at two to 18 years compared with only 26% of Lien's patients, followed for one to 14 years, this may be due simply to different methods of study. Baldwin, for example, defined as abnormal any patient with trace proteinuria or reduced inulin clearance, whereas Lien *et al* required protein losses of more than 1 g daily or a raised serum creatinine concentration. It seems hardly surprising therefore that Baldwin observed a greater degree of abnormality.

Direct comparison of those patients with raised serum creatinine concentration is unfortunately complicated by the fact that so few of Baldwin's patients returned for follow-up. He does state nevertheless that 50% of 16 patients seen more than 10 years after their initial illness had a serum creatinine greater than 97 $\mu\text{mol/l}$ (1.1 mg/100 ml). It would be very difficult to draw any conclusions from this about the course of the disease in the other 152 patients in his series. Lien's group

on the other hand managed to trace all of their 57 patients and found only two (3.5%) with serum creatinine greater than 120 $\mu\text{mol/l}$ (1.4 mg/100 ml two or more years after the acute illness.

The histological differences reported, however, do appear to be real, in particular the occurrence of glomerulosclerosis in more than 50% of Baldwin's repeat biopsies and its relative absence from the Australian series. The explanation for this is not entirely clear. The importance of glomerulosclerosis nevertheless rests in its implied association with progressive loss of renal function and it is therefore interesting to note that the incidence of end-stage renal failure was identical—namely, 1.7% for each group.

In conclusion therefore we would like to suggest that, despite the differences in ethnic groups, age range, methods of study, and number of patients available for follow-up, Baldwin's and Lien's papers present essentially similar data for the natural history of post-streptococcal glomerulonephritis. Minor abnormalities of renal function are present in many patients, especially when these are looked for carefully. Progression to terminal renal failure appears to be rare, however, and few patients coming to dialysis are likely to have end-stage poststreptococcal glomerulonephritis as their underlying pathology.

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¹ Baldwin DS. *Am J Med* 1977;62:1-11.

² Lien JWK, Mathew TH, Meadows R. *Quart J Med* 1979;48:99-112.

Epileptic drugs and the police

SIR,—A patient of mine, a man of 20, who is an epileptic controlled on anticonvulsants for many years, and with a driving licence issued by the Department of the Environment in full knowledge of his condition, was recently arrested by the police and searched, when his medication was found. He was charged with being in possession of drugs and locked in a cell. His medication was taken away from him and it was only with the utmost difficulty that it was eventually restored to him. It is hardly necessary to point out the danger to which he was exposed—not only the hazard of a convulsion but also the probable loss of his driving licence.

The charge has been withdrawn and the situation is now resolved but it appears that many other epileptics may be in similar danger. I have now given my patient a statement detailing his condition, his medication, and the fact that his driving licence has been issued legally. I consider that it would be desirable for anybody in this young man's condition to have a similar statement and it might be preferable if an official form could be made available for this purpose.

It also seems desirable that police stations should have available a list of drugs used in the treatment of epilepsy (the current *MIMS* lists only 19). In the case I mention the patient's drugs were restored after communication with his mother. She forgot to mention that he is also taking folic acid and this seemed to raise the suspicions of the arresting officer.

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Sustained-release lithium tablets and renal function

SIR,—Drs L Wallin and C Alling (24 November, p 1332) claimed the superiority of sustained-release lithium tablets over conventional tablets on the ground that long-term treatment with the latter preparation lowered renal concentrating ability significantly more than did treatment with the former.

The concentrating ability is a measure of the lithium-induced alteration in renal water reabsorption. The authors compared patient groups that were matched with respect to age, sex, and total intake of lithium. However, the groups were not matched with respect to serum lithium concentration and duration of treatment. According to animal studies¹ and observations on patients (Vestergaard, Thomsen, data to be published), these two variables correlate significantly with the lithium-induced disturbance of water reabsorption, and their variation should consequently be controlled when renal function is investigated. Drs Wallin and Alling have not made it seem probable that their matching criteria sufficiently controlled the variation in all relevant treatment variables but the one under investigation. Sustained-release tablets may or may not be preferable to conventional tablets, but the evidence presented hardly decides this issue.

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¹ Thomsen K. *J Pharmacol Exp Ther* 1976;199:483-9.

Psychoanalysis, psychotherapy, and health services

SIR,—It is hard to know what to make of Professor M Shepherd's paper (15 December, p 1557). He quite correctly points out that the public is entitled to decide how far it will support health care for the community at large. In this context, cost-benefit analysis may well override considerations of efficacy. Can we, for example, afford renal dialysis and intensive care units? Similarly, his call for scientific objectivity and freedom from vested interest in psychiatric investigation makes good sense.

It is, however, difficult to align these attitudes with what I take to be the main thrust of his paper. How are we to understand his attack on psychoanalysis and psychotherapy? I cannot believe that a man of his reputation is unfamiliar with the extensive and significant literature on psychotherapy research available in this and other countries, and yet in support of his own critical view he uses references from *Time* magazine, an psychoanalyst named Appelbaum, and an out-of-context quote from D H Lawrence.

How can we understand his criticism of a lack of clarity and objectivity in others when it is impossible to discern whether he is espousing an abandoning of the dynamic therapies or whether he wishes us to abandon the whole of psychodynamic theory so that we can start afresh? Certainly he seems keen to support the embittered Janet's concept of psychoanalysis as "A mental liquidation" and *Time* magazine's accusation that psychoanalysis is alone responsible for the "narcissistic excesses of today's Me decade."

Professor Shepherd's concern for the

"most easily exploited" members of the community is again laudable, and yet he seems to be against registration of psychotherapy, quoting the one out of seven dissenting opinions to the Foster Report advocating statutory registration with the protection that this would inevitably bring. His implication that large numbers of the "most easily exploited" need to be offered "more than support, humanitarian assistance, and professional friendship" is difficult to follow in view of his clear opposition to those who are vigorously attempting to provide this.

Perhaps the answer to my attempt to make sense of Professor Shepherd's article lies in the fact that he has missed the very point he is himself making. If, as he states, the influence of psychoanalysis is on the wane and psychotherapy procedures based on this theoretical approach are unscientific, unproved, and harmful, why then is it that the medical profession, particularly those in the area of general practice, increasing numbers of psychiatrists and paramedicals, and the public at large persist in making ever-increasing use of such psychotherapeutic opportunities as are offered and mount increasing demand for trainee experience and facilities?

It may be that the answer to this question lies in the marked contrast between the therapeutic nihilism of the biological psychiatrists of this country when compared with the enthusiasm and optimism of those engaged in various aspects of the work of psychotherapy.

STANLEY GOLD

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SIR,—May I be allowed a few comments on Professor M Shepherd's paper (15 December, p 1557)?

I think that to judge psychotherapy from the premises of either Scientology or behaviour therapy is totally misplaced. Scientology, including some form of psychotherapy, has long been discarded as a confidence trick. Behaviour therapy treats symptoms only and not causes. It has proved to be valuable in phobias and psychosexual problems. These, however, represent only a small part within the field of psychotherapy. Moreover, psychotherapy is often used in conjunction with behaviour therapy in order to achieve lasting results.

With regard to the desirable scientific foundation of analytical theory, it is on the way to being realised. I would draw attention to a recent paper by Meyersburg and Post,¹ entitled "An holistic developmental view of neural and psychological processes: a neuro-biologic-psychoanalytic integration."¹ From my own long experience as a therapist I would like to say that most patients referred to me by GPs, with depression, anxiety states, or psychosomatic complaints, are patients who were not helped by traditional psychiatry; they come for psychotherapy as a last resort. Where medication, electric convulsive therapy, etc, make no impact, psychotherapy is attempting to go down to the roots of underlying conflicts. This often enabled patients to understand their problems, work them through within the therapeutic relationship, and either solve them or accept them. This makes them accept responsibility again for themselves and enables them to cope with their work and family situation. To bring this about needs a very special training from one of the analytical schools. This should be

carried out by recognised associations, such as are mentioned in Professor Shepherd's paper, and a unifying common denominator be found for the various schools. It would be a most valuable addition to the NHS.

Whether psychotherapy should remain within medicine or not is a different consideration. As a doctor I find it difficult to divorce my medical from my therapeutic experience. But there could be a grading of therapists from specialists down to auxiliaries, according to experience. Surely psychotherapy would be well represented within British psychological medicine and, rather than being ridiculed, be well received by traditional psychiatrists for the sake of their patients.

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¹ Meyersburg HA, Post RM. *Br J Psychiatry* 1979;35: 139-55.

Paracetamol concentrations, hepatotoxicity, and antidotes

SIR,—After the letter by Dr J A Vale and others (1 December, p 1435) commenting on the article of Dr L F Prescott and others (3 November, p 1097) we think that the treatment of paracetamol poisoning is very simple. All patients with suspected paracetamol overdose should receive an antidote immediately. After all, the patients are admitted to hospital anyway; the antidotes methionine and N-acetylcysteine have few, if any, side effects; and they must be given within 15 hours of ingestion to be effective. To base treatment on a patient's estimate of the size of the overdose or to wait for a serum paracetamol result, which may take several hours, seems irrational. Even when the paracetamol value becomes available, the decision whether it lies above a particular line usually depends on the patient's estimate of the time of ingestion. Finally, the best treatment line is not established. Dr Prescott and his colleagues suggest that the present treatment line could be lowered from the 200 line to the 150 line (dotted in the figure) but we wonder if the 100 line is not even more appropriate.

Dr Prescott's group has presented data in

untreated patients where none of nine patients with results below this line suffered liver damage compared with five of 22 with results between the 100 and 200 lines, six of 25 between the 200 and 300 lines, and 25 of 27 above the 300 line.² Patients with an induced hepatic mixed enzyme oxidising system could be at risk even with results below the 100 line.³ Overall there seems a good case for routinely giving an antidote immediately.

Are paracetamol levels useful guides to treatment? Because of the safety of the antidotes and the uncertainty as to whether a patient is at risk of liver damage, paracetamol measurements absolutely determine treatment only when paracetamol is not detected or values are extremely high. In most cases a full course of antidote would be given and paracetamol levels would not be used in clinical decision making. Emergency paracetamol determinations may already have become a thing of the past.

Which antidote should be used? A course of intravenous N-acetylcysteine costs our pharmacy A\$23.00 to prepare and lasts 24 hours, whereas a course of methionine would cost us less than A\$1.00 and last three days. Though the doubts of Dr Vale and his colleagues are justified, the results presented by both protagonists marginally seem to favour N-acetylcysteine, and the advantages of the intravenous over the oral route are considerable in that the antidote reaches the site of action immediately and is unaffected by vomiting.

We therefore give N-acetylcysteine immediately to all patients with suspected paracetamol poisoning and do not depend on serum paracetamol determination.

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¹ Prescott LF. *Health Bull* 1978;36:204-12.

² Wright N, Prescott LF. *Scott Med J* 1973;18:56-8.

Risk of liver damage from untreated paracetamol overdosage.