

that reported in such patients at the Whittington Hospital. Possibly this higher incidence results from our retrospective study in which "simultaneous blood and urine samples" were not collected. I doubt, however, that this can be the explanation as Dr Kennedy and his colleagues have not excluded the 59% of their patients in whom urine analysis is not reported.

Drs S J Iqbal and P J Ojwang (9 December, p 1640) express concern about the conclusion that biochemical analysis of the urine is of little value in such patients. I would go further and ask what conclusion one is entitled to make on data from 18 isolated estimations in 44 patients. Surely the value of this estimation lies in daily evaluation of electrolyte homeostasis.

To describe hyponatraemia as "dilutional" on the basis that postoperative patients have been infused with undisclosed volumes of 5% dextrose is dangerously misleading. Most of our patients received more than 100 mmol sodium/24 h; 5% dextrose was only occasionally used. Is surgical practice at the Whittington Hospital so very different? Surely the widely recognised views subsequently expressed by Dr C T G Flear and his colleagues (9 December, p 1640) are in no way challenged by the information presented.

Finally, the observation that hyponatraemia cleared rapidly after intravenous dextrose infusion had been stopped and that "water restriction or hypertonic saline was not needed" leaves the surgical houseman confused. Should he then infuse saline to his fasting patients or merely deprive water by omission? Saline, and particularly hyperosmolar saline, should be used with caution as many such patients easily become overloaded with sodium. Fortunately, hyponatraemia tends to recover with improvement in general condition, but two of our postoperative and jaundiced patients required prolonged treatment with dextrose, potassium, and insulin. These patients survived, whereas the mortality in our hyponatraemia group was worse than the 27% in the Whittington patients. It is hard to imagine what evidence can justify a statement that "although 12 deaths occurred among the 44 patients hyponatraemia did not play a part in any."

Dr Kennedy and his colleagues planned to comment on inappropriate antidiuretic hormone secretion. They conclude that it is not a common cause of hyponatraemia. It had not occurred to us that it was. I hope house officers and students will be advised to treat hyponatraemia by established methods. They should not be discouraged from the selective postoperative infusion of 5% dextrose as a valuable source of the water upon which life depends.

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SIR,—We entirely agree with Dr C T G Flear and his colleagues (9 December, p 1640) that internal shifts in sodium and water contribute to the hyponatraemia found in a number of conditions which cannot be explained solely by changes in external balances. However, we do not agree that the "sick cell" concept need be invoked to explain the hyponatraemia of the large number of our patients on diuretics and intravenous dextrose.

While Dr Flear and his colleagues are in agreement with us that urine analysis is of little value in the diagnosis of the cause of

severe hyponatraemia, Drs S J Iqbal and P Ojwang (p 1640) take another view. However, they provide precious little evidence that the management of such patients is improved by urine analysis, and in one of the references they give it may possibly have been a disadvantage.¹ If Drs Iqbal and Ojwang reread our paper carefully they will see that we did, in fact, compare plasma urine ratios. We can confirm that plasma:urine osmolality ratios proved no more helpful in diagnosis than other direct and derived biochemical measurements. In addition, we ourselves stressed that diuretic-induced hyponatraemia is more often dilutional than depletional.

Finally, so far as "emergency" is concerned, we were referring to acutely ill patients who are found to be severely hyponatraemic.

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¹ Ruby, R J, and Burton, J R, *Lancet*, 1977, 1, 1212.

Seat belts and the safe car

SIR,—I read with great interest the article of your special correspondent on road accidents about seat belts and the safe car (16 December, p 1695). I am delighted that your journal is being used to disseminate so clear an account of the issues at stake, and so careful a weighing of the evidence.

However, it is not enough for doctors to convince doctors. Doctors must now convince the public. It was because the surgeons of the Royal Australasian College of Surgeons convinced the Australian public that the politicians in that country followed public opinion and legislated. In Northern Ireland, the involvement of virtually all the surgeons in an education campaign has completely changed public attitudes here. Unfortunately, for other reasons, our legislation has never been brought forward.

What about compulsory wearing of seat belts in England, Scotland, and Wales? The British Medical Association is in favour—so are the London and Edinburgh Royal Colleges of Surgeons and many other medical and surgical associations. But as yet the public is totally unaware of this. The Government has announced its intention of bringing in legislation. Will this move go by default through sheer inertia? There is the inertia of MPs who do not know or care about the facts; the inertia of the public, who are confused and have other things on their minds; and the inertia of the medical profession, who are in possession of the facts and support the measure but cannot or will not communicate their concern to the public.

It is the Sunday papers, the weeklies, the national and provincial papers, the radio, and the television that we need to communicate through. The *British Medical Journal* is not enough.

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SIR,—Your special correspondent writing on this subject (16 December, p 1695) makes no mention of laminated glass in cars. This is unfortunately not compulsory in this country

and without it no car is safe for its occupants. The latest figures (for 1977, quoted in the *Daily Telegraph*, 29 December) show that, of the 6614 people killed on the roads, the majority were not occupants of cars. There were 2313 pedestrians, 301 cyclists, and 1031 motorcyclists killed, a total of 3645 non-occupants. The number of car occupants killed was 2441 and one suspects some of these to have been suicides. Of course, seat belts properly used would reduce deaths and injuries to occupants but it is not just occupants one should consider. A car out of control or in irresponsible hands was presumably the cause of most of the pedestrian deaths and no seat belts would have saved them.

As to injuries to the occupants, many of the serious ones we have to deal with are serious just because of fragmentation of glass. It is not just the unrestrained occupant who breaks the windscreen. I have recently had a patient with perforation of the eyeball from a fragment of broken windscreen even though he was properly belted in. I have also had a case of ruptured eyeball from mugging when the car stopped at traffic lights as the victim was belted in and could not evade his attacker. There are some terrible people loose on the roads these days.

It seems to me that more good would accrue from the non-controversial and easily enforceable measure of making laminated windcreens compulsory on all new cars, as in most countries, than in trying to enforce the controversial body restrainers of one type or another. Not all doctors are in favour of legal enforcement of seat belts or other penalties after the damage is done and I am one of them. After all, it is usually the innocent passenger who bears the brunt of the injuries rather than the supposedly responsible driver and one does not wish to add to his or her distress.

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Dialysis and transplantation and the quality of life

SIR,—I welcome your timely leading article (25 November, p 1449) on dialysis and transplantation and would particularly support your call for medical and nursing staff increases to match the increased funds for kidney machines. It is right, too, that the deplorable level of renal replacement therapy in Great Britain should be publicised to the medical profession.

Having said this, however, I am disturbed by the implications, particularly in the last paragraph, that dialysis and transplantation might not result in the restoration of a reasonable quality of life. I think it is important to stress that these treatments, in fact, result in a remarkable rehabilitation rate. Two-thirds of patients on home dialysis are in full-time employment and a further 7% work part time. Four out of five successfully transplanted recipients achieve the life style that they hope for, and the overall three-year survival figure for renal replacement therapy is 70%¹ (these statistics are from all Europe and, therefore, include older patients and those with multi-system disease, whose treatment the leader writer appears to question). There can be very few treatments for potentially fatal conditions which achieve the figures quoted

above, and to refer to dialysis and transplantation treatment as "extraordinary" is surely an unjustifiable judgment.

At a time when the referral rate to nephrologists clearly indicates that many patients are dying from end-stage renal failure without the benefit of proper assessment, the general medical profession should be clearly aware of the real benefits of renal replacement therapy.

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¹ UK Transplant Annual Report 1977-1978. Bristol, UK Transplant Service, 1978.

Drug treatment of psychiatric patients in general practice

SIR,—A recent survey by Dr Peter Tyrer (7 October, p 1008) of patients seen at his psychiatric outpatient clinic purported to show that many had been prescribed psychotropic drugs incorrectly by their general practitioners. In a subsequent letter (18 November, p 1433) Dr Tyrer confirms that general practitioners received "feedback" about their prescribing habits in the four years covered by his survey and this resulted in less incorrect prescribing during the second half.

As Dr Tyrer and his colleagues responded to psychiatric referrals from the same general practitioners for many years before he started his survey it would be interesting to learn what alteration they made in the contents of their letters to effect a significant change in general practitioner prescribing habits during the survey period. Perhaps he has demonstrated that the standard of general practitioner prescribing in any area alters in accordance with the quality of the advice given by the local specialists.

IAN H MCKEE

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SIR,—Dr Peter Tyrer (7 October, p 1008) states that "many general practitioners were unaware that their prescriptions [of antidepressants] were insufficient to produce true antidepressant effects" in his series. The leading article (16 September, p 783) on this topic suggests that plasma concentration might be more important than actual dosage, but even this is debatable. High plasma levels may be ineffectual.

It is a common occurrence to achieve a satisfactory clinical response with small doses of antidepressants. Those doctors who use small doses initially, with increments as required, usually confirm this finding. Such patients are not usually referred to a psychiatrist. On the other hand, if antidepressants are started routinely in large doses, unacceptable side effects may result in the patient's refusing to continue or seeking to place his confidence elsewhere.

JOHN MATHEW

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"Lecture Notes on Medical Statistics"

SIR,—I have just read the review (9 December, p 1631) of *Lecture Notes on Medical Statistics* by Aviva Petrie, and the letters by Aviva Petrie and Mr Ian Clarke (23 December, p 1783) and am appalled that your reviewer, although

claiming to be a consultant statistician, appears to have a very prejudiced view of the bread-and-butter methods of applied medical statistics.

The first half of his review appears to be a general criticism of the subject of medical statistics, but the criticisms he makes are unqualified and without rational foundation. For example, he states that small sample sizes are the bane of medical research. How can this statement be defended when a large part of the development of statistical theory in this century has provided solutions to many of the problems that small samples present? For example, the two sample *t* test is a valid test provided that the assumptions of equality of variance and normality are reasonable (the *t* test is, of course, robust to minor departures from normality). Non-parametric tests are valid when used with small samples and make no assumption about the form of the parent distribution.

Does your reviewer really believe that if a patient who survives is scored 1 and a patient who dies is scored 0, the mean of the scores implies that on average the patients are half-dead? Does he not realise that the average of these scores is the proportion of patients who survive? His criticisms of the book are equally absurd. I would be interested to know his specific problems with the example of the *t* test on pp 82-83. The sample sizes are indeed 12 and 13 patients; so what? Are these not the sort of sample sizes which require a *t* test?

On item 4 of Aviva Petrie's letter and the reply concerning whether or not to include Fisher's exact test in an elementary text of medical statistics, I agree with Aviva Petrie but accept that Ian Clarke is entitled to his opinion. The test is certainly very clumsy to perform without a table of logarithms of factorials. However, Clarke's reference to Swinscow¹ is unfortunate. The example used in Swinscow's otherwise good, if brief, book is bad because the decision to perform the test is made *because the results look extreme*.

Ian Clarke's reply to Aviva Petrie's letter gives some excuses and apologies for his wild criticisms but his final paragraph expresses an opinion, stated as a fact, that the book is an inappropriate text for its target audience—a statement with which many experienced medical statisticians would disagree.

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¹ Swinscow, T D V, *Statistics at Square One*. London, British Medical Association, 1976.

Reusing dialysers

SIR,—We are not sure why Dr J M Vandembroucke and his colleagues (18 November, p 1434) reuse the dialyser 10 times and not 15 or 20 times. Limiting factors such as financial gain, rupture rates, dialyser clearance, increasing morbidity, patient acceptance, and nurse and technician times need active consideration before reaching an arbitrary set figure. They do not mention whether they reuse the blood lines and heparin administration sets. In 1975 we described reuse of the Gambro-Lundia dialyser inclusive of blood lines using an automatic rinsing device,¹ which has been further automated, now requiring 15 minutes of rinsing time, and is commercially available. In our experience, considering the labour and time involved, the financial gain achieved in reusing the dialyser more than six times is

negligible. If the blood lines are not reused and the dialyser is reused up to 10 times the cost per dialysis turns out to be more when compared with a dialyser reused up to six times inclusive of blood lines (see table).

Cost comparison of dialyser reuse (Dialyser: Gambro Lundia 1.36 m²: £18.50. Blood lines: Gambro; £3.55)

Reuse	Cost of reuse including blood lines (£)	Cost of reuse exclusive of blood lines (£)
6th	3.67	6.63
7th	3.15	6.19
8th	2.75	5.86
9th	2.45	5.60
10th	2.20	5.40

Finally, we confirm their results concerning pyrogenic reactions and septicaemia but emphasise that the technique of dialyser reuse must be practised only by skilled staff, preferably using an automated dialyser rinsing device, in order to achieve satisfactory results.

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J HUSSLER

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¹ Ahmad, R, and Goldsmith, H J, *Dialysis and Transplantation*, 1975, 4, 29.

Tranquillisers and plasma prolactin

SIR,—The report by Dr S K Majumdar and others of a prolactin-lowering effect of chlor-methiazole (4 November, p 1266) also refers to the wide range of drugs which cause hyperprolactinaemia, including "tranquillisers such as phenothiazines and at least some of the benzodiazepines." While neuroleptic tranquiliser-induced hyperprolactinaemia is well recognised,¹ the effect of benzodiazepines on plasma prolactin is less clear and confusion can arise if the general term "tranquilliser" is used in this context. Patients presenting for investigation of amenorrhoea and hyperprolactinaemia are not infrequently found to be taking or to have recently taken a tranquilliser, most commonly diazepam. Horrobin² maintained that diazepam elevated prolactin secretion in animals but Noel *et al*³ found no increase in plasma prolactin over a 3-h period following intramuscular diazepam in human volunteers. We therefore decided that it was important to establish whether diazepam has any effect on plasma prolactin levels.

We studied basal plasma prolactin levels and the release of prolactin after administration of the dopamine antagonist metoclopramide in 10 women patients before and during treatment with 15 mg diazepam per day.⁴ The patients were selected from a group of hospital in-patients with anxiety symptoms. All were drug-free for at least one week before being studied. After an overnight fast an intravenous cannula was inserted and the patient allowed to settle for one hour to overcome the effects of stress on plasma prolactin levels. Three blood samples were then taken through the cannula at 10-min intervals and the mean of these three values taken as the basal plasma prolactin level. Metoclopramide 2.5 mg was injected intravenously via the cannula and blood samples for plasma prolactin estimation taken 10, 20, 30, 60, 90, and 120 min later. The patients were then started on 15 mg diazepam by mouth per day and the estimations of basal and stimulated prolactin levels