had had upper abdominal operations, we found that demand reduced quite quickly over the first 24 hours in an apparently exponential manner.¹ Thus overdose can be expected on occasions with Dr Church's technique despite precautions, and indeed in two of his patients the antidote naloxone had to be given.

There is, however, a more fundamental objection to such a system. Only the patient can know when pain relief is sufficient and balance side effects against further requirements. Self-regulation may be because of excessive sedation or nausea, or because relief is then limited by the drug's analgesic potential. Rather than use an onlooker to try to balance such factors at short intervals, it seems more logical to allow the patient himself to initiate each incremental dose, imposing restraints which ensure that the effect of each dose is tested by the patient before another can be obtained. In our experience with such a system, no patients made demands anywhere near the maximum possible—an indication of feedback control. Furthermore, none showed signs of overdose such as respiratory depression or excessive drowsiness. This is in accord with our results from a large group of mothers in labour. It would seem therefore that patients do not voluntarily overdose themselves; it is only when a drug is administered on a preselected basis that this is likely to occur.

Although the average dose of pethidine used by our patients (8.1 mg/kg/24 h) is close to that of Dr Church's group (7.2 mg/kg/24 h), the variation in individual dosage was very substantial, ranging from 2.9 to 23.0 mg/kg/24 h. This wide individual variation was also reported by Professor M Holmdahl, speaking at the Royal Society of Medicine (6 April 1979), who also measured blood levels of pethidine in post-operative patients using the Cardiff apparatus. He found wide variations in the blood levels which afforded pain relief as well as differences similar to ours in the levels of demand.

As in so many fields of medical practice, maximum effectiveness must be balanced by considerations of safety. At present intramuscular administration of drugs provides inadequate pain relief in some patients, but this must inevitably be so if the level of supervision is inadequate. Continuous intravenous infusions obviously improve pain relief but need considerable supervision to make them safe. Close supervision involves mainly consulting the patient frequently—which is a pretty close approximation to self-demand without the built-in feedback safety. The apparent cheapness of the infusion has to be set against the expense of greater supervision. The apparent expense of a sophisticated apparatus must be set against less supervision and could soon be substantially reduced by mass production and developments in micro-electronic technology.

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¹ Chakravarty, K, *et al*, *British Medical Journal*, in press.

Clinical medical officers

SIR,—The problem of clinical medical officers, the large majority of whom are employed in child health, is of great concern to the Joint Paediatric Committee of the three Royal Colleges of Physicians in the United Kingdom and the British Paediatric Association. We feel

strongly that their natural affiliation lies not with community medicine but with clinical paediatrics.

There are, of course, many important matters to be looked at in relation to their training, qualifications, and career structure and in relation to the status of existing clinical medical officers. We intend to consider these in detail in the near future. The decision of the British Paediatric Association to establish a grade of associate membership in this context is welcomed.

The urgent necessity for the present is to establish clinical medical officers firmly as clinical doctors, albeit with important links with community medicine.

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Recent trends in opiate dependence

SIR,—Your timely leading article (7 April, p 911) has rightly raised "... doubts about the efficacy of the prescribing policy." This policy, adopted in the late 1960s, stopped the over-prescribing by a few doctors, probably largely responsible for the heroin-cocaine problem at the time,¹⁻³ but in recent years the numbers of "known" addicts have steadily increased.

The number of notifications of new ("not previously known") narcotic addicts has risen from 711 in 1970 to 1112 in 1977. A comparison of these recent figures with, for example, the total number of narcotic addicts known in the years 1960-5 (437 in 1960, 927 in 1965), or of the heroin addicts among them (94 in 1960, 521 in 1965) hardly supports claims that addiction has been "contained." Moreover, the finding that year by year a sizable and rising number of new addicts becomes "known" (or surfaces) probably reflects a gradually increasing illicit market in preceding years. Many addicts tell one that in recent years "much more stuff is available on the black market, chased by more people than previously." The Home Office explanation of improved notification procedures therefore probably does not account fully for the rising notification figures. Anyway, for various reasons many addicts are "lost" to the notification system.⁴ Similarly, one may doubt that the increased number of new cases is "largely offset by the number of cases removed from notification": of those addicts removed from the index, after admission to prison or because they are "no longer seeking treatment," quite a few are likely to resume drug taking on the illicit market without necessarily reappearing on the index.

Recent Home Office publications concentrate on the numbers of addicts known to receive drugs, as at 31 December of each year: 1426 in 1970, 2023 in 1977. But probably more significant, and more comparable with the statistics of the 1960s, are the numbers of addicts notified during the whole of the year (even if for some of the reasons mentioned above some were subsequently removed from the index): 2881 in 1969, a slight fall to 2661 and 2769 in 1970 and 1971, followed by a steady rise to 3611 in 1977. The 1978 figure seems likely to show a further, considerable increase.

Contrary to a widespread misunderstanding in the United States, no official British body has ever recommended a heroin maintenance programme. The 1926 Rolleston Report—the basis for all later policies—never suggested the possibility of maintenance prescribing without strict safeguards (although these were conveniently overlooked by the overprescribers of the 1960s).^{2,3} In practice, a proportion of addicts under treatment by the treatment centres have been maintained on injectable methadone (as distinct from the American oral methadone maintenance method). Treatment centre doctors originally probably chose injectable methadone rather unwillingly in the hope thereby to get addicts away from the illicit market. A black market in injectable methadone developed shortly after the establishment of the treatment centres. Oral methadone is obviously less risky but, on the other hand, recent trends of centres towards substituting oral for injectable methadone may be not unconnected with increased pressures on GPs (and private practitioners) to prescribe alternative (addictive) drugs such as dipipanone (Diconal) or dextromoramide (Palfium). GPs should be warned against a likely influx of patients asking for alternative drugs whenever centres tighten their prescribing policies; and there may be risks if private practitioners prescribe addictive drugs for purposes other than tiding addicts over the withdrawal period.

Among other evidence of an unsatisfactory state of affairs are cases of sniffing of heroin and cocaine (14 April, p 971) among affluent and "smart set" youngsters coming to one's attention. Moreover, as treatment centres were never set up to deal with the problem of non-narcotic yet addictive drugs (such as the "mainlining" of barbiturates,⁵ amphetamines, etc) or with the prevalent polydrug misuser, no adequate provision exists for tackling these serious and common problems. The time seems ripe for a review of management not only of the opiate problem but of the whole problem of misuse of, and dependence on, all types of addictive drugs.

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¹ Interdepartmental Committee on Drug Addiction, Second Report. London, HMSO, 1965.

² Spear, H B, *John Marshall Journal* (Chicago), 1975, 9, 67.

³ Glatt, M M, *et al*, *The Drug Scene in Great Britain*. London, Edward Arnold, 1967.

⁴ Smart, R D, and Ogborne, A, *British Journal of Addiction*, 1974, 69, 225.

⁵ Glatt, M M, *Lancet*, 1969, 2, 429.

Short dialysis regimens

SIR,—We read with interest the papers of Dr J A P Trafford and others (24 February, p 518) and Dr L Sellars and others (p 520) evaluating shortened haemodialysis regimens, particularly since their conclusions concerning its effects on hypertension differed. We wonder, however, how much the results of the Newcastle group reflected changes in dialysis equipment, frequency of dialysis, patient population, patient management, and the difficulties of retrospective analysis rather than a change in the hours of treatment given.

When studying a cohort of 53 patients in whom treatment hours were decreased from 18-21 to 12-15 h/wk, but without changing the dialyser, dialysate sodium, or frequency of dialysis, we also detected significant increases in systolic ($P < 0.01$) and diastolic ($P < 0.05$) blood pressure¹; but the degree of change was trivial (mean predialysis blood pressure $\pm$ SEM rose from $139 \pm 3/78 \pm 2$ to $146 \pm 3/81 \pm 2$ mm Hg). Moreover, the proportion of patients taking hypotensive agents did not increase, but fell slightly from 30% to 20%. We

were also unable to demonstrate adverse effects on mortality, bone histology, anaemia, peripheral neuropathy, or rehabilitation score, though plasma creatinine, urea potassium, and the need for phosphate binding agents increased.

Both the Newcastle and Brighton authors discuss patients assessed during shortened treatment hours but do not mention patients in whom this was not possible. We would like to emphasise that, of 66 patients who entered this latter phase of our study, the hours of treatment were effectively reduced in 55. Reasons for the failure to maintain short-hour dialysis treatments were commonly related to inadequate ultrafiltration; hypertension occurred in only two patients (3% of total).

With the proviso that we may not have studied our patients for long enough, the limitation to short treatments seems to relate more to difficulties in ultrafiltration in a minority than to the medical or surgical complications of hypertension, or indeed of other consequences of uraemia. As dialysis hours continue to be pruned, it seems clear that an inflexible approach in recommending treatment regimens should change and that the tailoring of treatment to best balance the priorities of the patient's well-being, the acceptability of treatment, and economy will have to be established.

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¹ Paton, S, *et al*, *Proceedings of the European Dialysis and Transplant Nurses' Association*, 1978, 6, 14.

*. We sent copies of this letter to the authors of the two papers, whose replies are printed below.

SIR,—As in Oxford, we have had no significant problem with blood pressure during the period in which we have progressively shortened dialysis. Our experience also has been of a reduced number of patients needing hypotensive therapy.

The mean dialysis time per week for all patients in 1978 was 9.16 hours, the range being 6–15. We thus agree entirely with the Oxford workers' comments that an inflexible approach to treatment regimens should change and that the tailoring of treatment to strike the best balance between the patient's well-being and the acceptability of treatment as well as cost will have to be established.

It has been shown many times that, even at the price of certain side effects, patients prefer to have their number of hours on any machines reduced. The resistance to very short dialysis comes therefore from doctors and not from patients and is another reason why no regimen can be standard and that both the patient's concern and the clinical opinion have to be taken into account.

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SIR,—Sue Paton and her colleagues wonder whether the increase in exchangeable sodium and need for antihypertensive drugs in our patients studied in 1978 could have been due to factors other than a reduction in weekly hours of dialysis. If the 1973 "high sodium dialysate" and 1978 groups are compared it will be seen that in both cases dialysis was given thrice weekly against dialysate of sodium concentration 136 mmol/l. In 1973 all patients

were using the Meltec multipoint cuprophane 12 m dialyser and in 1978 the Meltec multipoint was used by 23 patients and the Rhone Poulenc six by 3 patients, so that there was little change in dialysis equipment. In both 1973 and 1978 the patients studied were taken from a pool of 30–40 patients on long-term hospital haemodialysis. Selection was random except that patients who lived a long distance from the dialysis centre were not approached. The mean age of patients in 1978 was 42 ± 11.75 years, with 17 males and 9 females. In 1973 mean age was 41 ± 11.8 years with 14 males and 4 females. The patient populations are therefore comparable. The only non-dialytic factor likely to effect sodium loading is the amount of salt in the diet and, as stated in our paper, this aspect of patient management did not change between 1969 and 1978—that is, patients were advised to avoid salty foods and not to add salt at the table.

Paton *et al* in their study of 53 patients found a small but significant rise in blood pressure and a fall in the proportion of patients taking antihypertensives when dialysis was shortened. Exchangeable sodium was not measured. We are not told whether the patients were selected or how many had had bilateral nephrectomy performed. Dialysate sodium was unchanged, but one wonders whether this was low, and if so whether the patients had troublesome cramps or symptoms of disequilibrium. It would also be of interest to know whether sodium intake was severely restricted in their patients. These factors would minimise sodium overload when dialysis was shortened and might explain why hypertension was not a serious problem in their patients.

It seems to us that in the Newcastle patients the only reason for the higher exchangeable sodium levels and the increased need for antihypertensive agents in the 1978 group compared with the 1973 "high-sodium dialysate" group was the shortening of dialysis time. In some patients this accumulation of sodium will be accompanied by severe hypertension and, of course, we agree that a flexible approach is necessary when deciding on the duration of dialysis in individual cases.

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Health problems of anaesthetists and their families

SIR,—The critics of my article "Health problems of anaesthetists and their families" (24 March, p 779) appear to have missed the key points. These were that there were more health problems in the families of anaesthetists than occur in the general population and that conventional statistical assumptions are biased against the findings of a health hazard in investigations dealing with safety.

Contrary to Sir Richard Doll's belief (21 April, p 1078), I did state in my article what types of cancer there were among the adult anaesthetists. I regret that I did not discuss in detail his study of cancer in male anaesthetists, but since my article did not dwell on cancer in male anaesthetists I did not consider it appropriate. I did not conclude that there was an increased risk of breast cancer solely on the basis of two cases occurring in 78 female anaesthetists, but when they are combined with those reported by Corbett¹ to give 17 cases in 601 predominantly young women then that conclusion

emerges. I agree that the use of the annual incidence of cancer of the breast in all ages is not an ideal base for comparison—but this choice, based on simplicity and including many older normal subjects (in whom the risk is increased), would have probably produced a bias to make my conclusions an underestimate.

Dr M P Vessey and Professor C M Conway (pp 1078–9) appear concerned about the low birth weight of the children born to non-exposed parents. This concern is misplaced as this particular group contains an excess of first-born children (67%) and hence they have a lower mean birth weight than the normal population. This was why I compared the birth weights of children of the exposed parents (43.7% were first born) with the established norms of the population. The only reason for including data about the non-exposed children was to determine if my method of sampling yielded results comparable with established values—for example, for abortion rate and congenital abnormality rate. It did.

Professor Conway's criticisms (p 1079) do him less than justice. He criticises my statistical calculations—for example, my observed incidence of female children occurring twice as commonly as male children in the offspring of female anaesthetists. Serious students of the pollution problem are aware that this was originally suggested in one of the first reports about pollution—from Russia, where the majority of anaesthetists were female. Since there has not been a report about a significant excess of male children there was an *a priori* reason to expect an excess of females. The exact test of significance gives the probability of my observation occurring by chance as 0.042 single tail. The result was only included since it confirmed the original observation but its clinical significance is trivial and, contrary to Professor Conway's assumptions, has no bearing on my final conclusions. He seriously misquotes and attempts to ridicule my report in that I did not claim solely on the basis of two cases of cancer occurring in 277 children that this risk is increased. I referred to the findings by Corbett² and specifically used the words "combined incidence." It is customary when rare events occur to combine results with those from other comparable series. On this aspect I considered Corbett's study to be the only one published that is comparable with mine.

The cavalier treatment of my results by Dr Mary Vowles and others (p 1079) is disconcerting. They wish to disregard a number of my cases in table 3 of the article. Of these case 2, fully described in the text, cannot be considered a normal child. Case 3 had a congenital tumour and died—their study included congenital tumours. Cases 6 and 18 had lesions that standard textbooks describe as congenital or familial in origin. West's syndrome, present in Case 10, is of unknown aetiology but birth trauma has been implicated in only 33% of the reported cases,³ and no external aetiological factor has been found in more than half. Cases 12 and 13 occurred in the same family. The remaining four cases they wish to disregard because they were not treated vigorously, which assumes a common standard of treatment across the country. This is highly unlikely, but even if they are disregarded this leaves a congenital abnormality rate that is still much higher than they found occurring in the normal population.

Dr J F Nunn and others (p 1079) accuse me of totally ignoring recent experimental studies. On this aspect I cited only four references, dated 1967, 1968, 1976, and 1978. I would draw their attention to two recent publications in which it is stated that exposure of experimental animals to trace concentrations of halothane⁴ and nitrous oxide⁵ had adverse effects on the fetuses. The effects observed were not dissimilar to my observations—small size, musculoskeletal system deformities, and fetal loss⁵—that is, the experimental evidence is not quite as they would imply.

I accept Dr Nunn's strictures on my statement "Several anaesthetics are carcinogenic," citing only one reference—the study by Corbett on isoflurane.⁶ I apologise for omitting references to two other studies—one by the National Cancer Institute, in which trichlorethylene was found to be carcinogenic,⁷ and the study by Winslow and Gerstner⁸ on chloroform, both of which are available in this