mothers from a painful experience is understandable though mistaken, for it impedes their mourning; and failed mourning gives rise to psychological troubles.³

The management of stillbirth causes considerable anxiety. For this reason it can help if those managing such painful conditions as stillbirth have discussions with a psychotherapist with experience in this technique.

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¹ Bourne, S, *Journal of the Royal College of General Practitioners*, 1968, **16**, 103.

² Lewis, E, *Lancet*, 1976, **2**, 619.

³ Lewis, E, and Page, A, *British Journal of Medical Psychology*. In press.

SIR,—We had already proposed to do just what Dr Ann Cartwright has suggested in her letter (4 February, p 298), not as an alternative but in addition to the leaflet for parents. Our third objective is to establish a national counselling service composed of lay and professional counsellors. This should answer Mr Peter Diggory's point raised in his letter in the same issue (p 298).

We hope the influence of this leaflet for parents will sensitise the hospital staff to try and work out what best to do in each particular instance, be it to see and hold or not to see and hold a grossly abnormal baby and whether to let the hospital or the parents carry out the funeral

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Fluids for parenteral nutrition

SIR,—Some modern pharmacies have the technical skill and equipment to dispense a daily parenteral nutrition solution which is tailored to a patient's requirements. The cost of this is often used as a deterrent for developing such a system, but this view is negated by the inherent advantages. These facilities have been available in American hospitals for many years and safety guidelines are well developed. On the other hand most British centres have relied on multiple-bottle systems, which are clumsy and are associated with more practical problems.

The pharmacy premix service requires a sterile area with appropriate pressurised filtered air. This is provided by a lamina flow hood, which is not a very expensive piece of equipment. The required nutrients can then be added to a bottle using aseptic pharmaceutical techniques. Repeated checks are required to be sure that the system is maintained at this high standard. The final daily fluid volume (from 1 to 5 l) contains the appropriate amounts of amino-acids, dextrose, and electrolytes (sodium, potassium, phosphate, magnesium, and chloride ions). Trace elements and a multivitamin preparation with vitamin C, folic acid, and vitamin K are also added. It has been shown that only when the appropriate amount of electrolytes are included can the maximum anabolic effect be obtained¹; yet to add these and the necessary vitamins to a multibottle system is time-consuming for the nurses and adds to the sepsis risk, even when the electrolyte solution is used for a vehicle as is generally recommended.

The benefits of this premix system are: (1) sepsis is less likely to develop with a simple one-bottle and one-giving-set regimen; (2) electrolyte concentrations can be adjusted from day to day and spread over the whole day, which is most important for potassium; (3) as many additives as necessary can be included without extra risk of infection—if more than two additions are to be made to any intravenous solution this should be done in a pharmacy²; (4) nurses are saved the daunting task of regulating multiple drips in parallel; and (5) an infusion pump set can be used to ensure a constant infusion rate and therefore better regulation of blood glucose.

The cost of preparing the single-bottle solution in our pharmacy (over the first 85 patient days of total parenteral nutrition) has been tabulated at £8.60 per patient per day if the solutions are prepared three times a week. The cost of each working session is broken down as follows:

For two days' supply	£
Labour	11.00
Sterile containers	2.80
Disposable needles, etc ..	1.00
Millipore filters	1.50
Running expenses	0.90
Total	17.20

This cost would be correspondingly less if more patients were treated each day and might be reduced by some refinements of technique with increasing experience.

In conclusion, we feel this added cost to the pharmacy is clearly justified by the reduced hazard of sepsis, by improved parenteral solutions for the patients, by decreasing the nursing load, and by giving more flexibility to the clinician. We feel that provision should be made for the introduction of this service to all major hospitals in the UK.

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¹ Rudman, D, *et al*, *Journal of Clinical Investigation*, 1975, **55**, 94.

² Department of Health and Social Security, *Addition of Drugs to Intravenous Fluids*, Health Circular HC(76)9. London, DHSS, 1976.

Noxythiolin-resistant organisms

SIR,—Further to my study on noxythiolin-resistant organisms (29 October, p 1121) I have observed a peculiar phenomenon. After prolonged storage on nutrient agar slope some of the original noxythiolin-resistant strains have now reverted and are sensitive to this antiseptic. But they have maintained their resistance on repeated subcultures in nutrient broth or, better still, with freeze drying. The same phenomenon has also happened with gentamicin-resistant *Pseudomonas aeruginosa*.¹ However, there has not been any change in the antibiogram with multiresistant *Staphylococcus aureus*² after prolonged storage for more than a year under similar conditions.

It is rather interesting to note that this spontaneous loss of resistance occurs only on preservation on solid medium in the absence of re-exposure. During my initial investigation the *Escherichia coli* (NCTC 10418) which was used as a control was being used in the laboratory for more than two years and was sensitive to noxythiolin. Now we have obtained a fresh culture from the NCTC, which,

although sensitive to all the commonly used antibiotics against Gram-negative rods, is nevertheless, to my surprise, resistant to noxythiolin. We have therefore inoculated this newly acquired strain on a nutrient agar slope so that the sensitivity can be checked from time to time to see whether the same phenomenon is repeated.

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¹ Chattopadhyay, B, *Lancet*, 1975, **2**, 934.

² Chattopadhyay, B, *Journal of Antimicrobial Chemotherapy*, 1977, **3**, 371.

Psychological evaluation in cases of self-poisoning

SIR,—I would like to comment on the above article by Dr R Gardner and his colleagues (17 December, p 1567). I find its conclusions difficult to evaluate and I think this may be because of some lack of clarity in presentation of the methodology and data.

I am prepared to accept the finding that a group of physicians who are sufficiently interested in the topic of parasuicide to enter a study of it will organise follow-up disposals for their patients very similar to those arranged by a group of psychiatrists, especially since "preregistration house physicians and medical registrars were instructed in the principles of a psychiatric assessment" by the principal author. If all doctors were as interested and as well instructed there would indeed be less need for mandatory psychiatric intervention in the assessment of parasuicidal cases, as the authors conclude, and disposals on discharge would be rationally determined whether the psychiatrist was involved or not.

If the article stopped at this one conclusion one might agree that Addenbrooke's was fortunate in its physicians and simply comment that parasuicidal patients in other parts of the country should make for there rather than for their local hospitals, where attitudes more like these described by Patel¹ are liable to be encountered. The fact is that in many psychiatrists' opinions parasuicidal patients are given very short shrift by doctors and nurses when they arrive in casualty departments and medical wards and it will take a long time to change this adverse attitude. Till then I think it would be safer to call in the psychiatrist routinely to make an assessment of each patient's needs. However, one point is hinted at in this article: physicians often complain that psychiatrists are unduly slow in responding to requests to see the patients and one notes that (possibly for entirely different reasons) more patients in the psychiatric group discharged themselves before they could be seen than was the case in the medical group. Psychiatrists must be prepared to view parasuicide as an emergency and make arrangements to see cases with no delay.

Much of the rest of the article is taken up with the demonstration that the outcome for patients over a 12-month period was much the same whether they were assessed by physicians or by psychiatrists. Since the proportions of patients within the two groups routed to different forms of treatment or supervision was similar this is scarcely surprising. It merely highlights the point that a good physician, with enough interest and with some knowledge of modern psychiatry, should be

able to carry out a straightforward psychiatric assessment and thereafter make sensible use of the available facilities.

But was the assessment by either the physicians or the psychiatrists at a satisfactory level? It all depends on the definition of the word "relapse," which is used more than once but never defined. If this just means "psychiatric breakdown" in this context, then its usage is inherently vague but otherwise unremarkable. If it means a second suicide attempt it is alarming, since this would mean that 28.75% of the physicians' and 35% of the psychiatrists' patients had repeated within one year of the first attempt. This cannot be, since the authors say that there were "312 consecutive admissions (276 patients) to the trial," which would imply that 36 patients had repeated in a year, which is approximately 13%, and a gratifyingly low proportion. Unfortunately a little later they mention that 24 patients were admitted more than once to the trial. Does this imply that some were admitted three or more times? It is irritating to go over data several times and to be unable to extract their significance because the authors have given us insufficient information.

Having worked in a poisons treatment centre in the past I remain impressed with the concept and agree with Kennedy² and with the Department of Health that these are both useful and desirable. In my view it is in such centres that we can develop clinical, research, and didactic activities that will lead to a truly inter-disciplinary approach to the frightening epidemic of parasuicide. Ad-hoc (or more likely, ad-hominem) arrangements as described by Dr Gardner and his colleagues are worthy efforts but are imperilled by every rotation of junior staff and every new physician appointment.

Could the authors clarify their data and, in addition, tell us whether, in 1978, almost three years after their study, the system they describe is still working as well as they would like?

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¹ Patel, A R, *British Medical Journal*, 1975, 2, 426.
² Kennedy, P, *British Medical Journal*, 1972, 4, 255.

*We sent a copy of this letter to Dr Gardner and his colleagues, whose reply is printed below.—ED, *BMJ*.

SIR,—Dr Munro has made a plea for the retention of the Department of Health's recommendation¹ (and its introduction to Toronto?) even though he is unable to cite any empirical evidence in its support 10 years after the recommendation was made. Yet he agrees with our findings and, in his letters, repeats several of our views precisely.

His main objection is to the general application of our findings to other hospitals because of the "adverse attitudes" prevailing there. If another hospital were to repeat our work and come to the same conclusion would that hospital also be atypical? Perhaps such a prospective clinical trial would have to be carried out in every hospital. We anticipated this objection and tried to minimise it by arranging for all the physicians and psychiatrists who customarily treated self-poisoned patients admitted to this hospital to participate in the trial.

If instead of asking us to "clarify our data" he had read our paper carefully he would have

found the answers to several of his questions. The definition of the word relapse is on p 1568 and on the same page we explain about the reallocation of patients in the trial. It would also have been obvious to him that a few patients must have been admitted to the trial on more than two occasions. He clearly misunderstands the concept of the trial. The figures are higher because the follow-up was not confined only to those patients who were readmitted to Addenbrooke's and who attended the accident department but also included all patients who relapsed elsewhere. Taking this into account we think that our assessments at Addenbrooke's were at a "satisfactory level." The relapse rates were calculated using standard statistical techniques for follow-up studies—that is, the figures were corrected for patients who had been lost to follow-up for some reason. Had we taken the crude figures from Tables IV and I we would have obtained slightly different relapse rates of 22% and 25% for first admissions to the trial. More "gratifying" figures might have been obtained had we not taken the trouble of following up our patients about the country.

To answer Dr Munro's final question, the system described in our paper is not only working three years after the study but has been fully adopted since November 1977 so that the medical teams now make the initial psychiatric assessment and ask for advice from psychiatrists and social workers as necessary. We continue to place emphasis on the instruction of junior doctors and nurses, on the education of medical students, and on improving the treatment and after-care services.

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¹ Central and Scottish Health Services Councils, *Hospital Treatment of Acute Poisoning*. London, HMSO, 1968.

Delayed respiratory depression after use of fentanyl

SIR,—The paper by Drs A P Adams and D A Pybus (4 February, p 278) raises important implications for the postoperative care of patients with the increasing use of high doses of analgesics, particularly fentanyl, during anaesthesia. However, in the discussion no mention is made of the fact that all their patients were less than average in weight and that all had received narcotic premedication, including hyoscine in two of the cases.

It is my observation that even small doses of morphine used as premedication can result in potentiated respiratory depression post-operatively, and for this reason patients undergoing major maxillofacial surgery are always given a non-narcotic premedication, usually oral diazepam and droperidol, the doses varying with the size of patient and degree of sedation required, and if necessary a sleeping state can be produced with no evidence of respiratory depression. I use large (0.5-mg) doses of fentanyl and postoperative respiratory depression has never occurred while these patients are observed for the following 24 h in the intensive care unit.

With many alternative drugs available there seems little justification for using narcotic analgesics as premedication if there is any risk of further respiratory depression occurring

during anaesthesia and presenting a post-operative hazard to the patient.

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SIR,—The paper by Drs A P Adams and D A Pybus (4 February, p 278) raises considerable doubts about the safe use of fentanyl in anaesthesia and reports three serious cases of delayed respiratory depression.

I believe that this respiratory depression is already clinically well recognised and is associated with the premedicant drugs mentioned—morphine and papaveretum—whose effects are potentiated by fentanyl. I regard papaveretum as the best narcotic premedicant for many patients but have reluctantly abandoned it before neuroleptanaesthesia precisely because of the delayed respiratory depression. Naloxone is very effective in dealing with this, but, *pari passu*, abolishes the analgesia which is one of the main objects of the technique.

I teach now that premedication before medium-dose fentanyl neuroleptanaesthesia (0.5–1 mg) should consist either of fentanyl itself in the form of Thalamonal (fentanyl-droperidol mixture) or, failing this, a pethidine mixture. Personally I would not proceed to a "neurolept" sequence if morphine or papaveretum had been given in the last 12 h.

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The water story

SIR,—The "water story" leading article (4 February, p 264) is certainly an interesting one. There is, nevertheless, a problem in comparing British and American studies of possible links between the softness of water and patterns of cardiovascular disease.¹

The situation in American soft-water areas is simply not comparable with our own, at least where human calcium intake is concerned. Firstly, the definition of "soft" water differs, but, more important, Americans in soft-water areas have probably grown up consuming far more calcium than people nurtured on our chalkiest streams. The reason is that until the early '50s milk consumption was substantially higher than in the UK and until the '40s it was very much higher. This covers most of the lifetime of the majority of subjects in the cardiovascular studies. Since cows' milk contains about 1.25 g/l of calcium even a difference of less than half a litre in daily milk consumption would have provided Americans with as much extra calcium as Britons drinking over a litre of the very hardest unboiled water (430 ppm calcium). Thus where British and American studies agree on an effect of soft water it seems unlikely that the common factor is a low basal intake of calcium.

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¹ Michell, A, *Perspectives in Biology and Medicine*, 1976, 20, 40.

SIR,—In your leading article on this subject (4 February, p 264) you state that there is a lack of theoretical explanation for the inverse