Bowel sounds were heard in 42 cases from group A compared with 13 from group B ($\chi^2$ test, $P<0.001$). When it became likely that an effect was present (after 50 cases) the scope of the investigation was widened to include observations of the bowel sounds 30 and 60 minutes later by trained recovery room staff who did not know which drugs had been injected. Of 31 patients from group A, 24 had audible bowel sounds at the end of the operation and 25 half an hour and 29 an hour later; for 19 patients from group B the figures were 5, 19, and 17 resepctively. The first pair of figures show a significant difference ($P<0.001$), but the remaining pairs do not. Moreover, in 11 cases, all from group B, the bowel sounds 30-60 minutes after operation were loud enough to be heard without a stethoscope.

Hyoscine butylbromide evidently has too short an action to be useful in this context and the rebound contractions that follow its use can be more powerful than those caused by neostigmine and atropine alone. The use of hyoscine butylbromide should therefore be avoided during the immediate recovery period following anastomosis of bowel.

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[1] Doughty, A G, and Wylie, W D, *British Journal of Anaesthesia*, 1952, **24**, 66.
[2] Bell, C M A, and Lewis, C B, *British Medical Journal*, 1968, **3**, 587.

## Bran content of wholemeal bread

SIR,—In a recent reply to a query (24 January, p 203) it was erroneously stated that 900 g of wholemeal bread was equivalent to 150-200 g of bran. The correct value is of the order of 90 g bran (at 14% moisture content), assuming 40% moisture content in the bread and making due allowance for the yeast, salt, and fat used in bread-making. The calculation is based on a bran content of wheat of 15%.

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**Our expert agrees that the corrected figure is right but does not think it alters the point he was trying to make—ED, *BMJ*.

## High-pressure medicine

SIR,—The statement in your leading article (6 December, p 541) that most surgeons operating at 20 atmospheres would prefer to rely on local anaesthetics with heavy doses of morphine if necessary is a facile and inadequate solution. This not only ignores the dangers of vomiting and respiratory depression associated with morphine but also ignores the ability of anaesthetists to cope with unfamiliar situations—an ability demonstrated throughout the development of modern anaesthesia at one atmosphere.

As Dr I C F Wisely (7 February, p 340) says, the problem is not simple. Even an invited paper in an anaesthetic journal on "The treatment of the diving casualty"[1] failed to give any guidance, though on reading the article it became obvious that the author's remit was treatment of the patient once he had reached a casualty department. Ketamine alone is unsatisfactory in adults at normal pressure because of its hallucinogenic effects,[2] but combined with diazepam it has been used very effectively in emergency situations.[3] Althesin (alphaxolone/alphadolone) alone does not guarantee sufficient anaesthesia.[4] Nevertheless, a combination of Althesin or propanidid with neuroleptanalgesia may be satisfactory. There are therefore at least two potentially satisfactory techniques that would permit endotracheal intubation to protect the airway or the use of muscle relaxants if these were indicated.

As well as the academic projects mentioned by Dr Wisely, basic scientific research is continuing into the mode of action of inhalational anaesthetics utilising high atmospheric pressure as a research tool with potential spin-off information for deep-diving gas mixtures. However, there appears to be no research into the suitability of different intravenous anaesthetic agents for use at 20 atmospheres.

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[1] Elliott, D H, and Hanson, R de G, *Anaesthesia*, 1976, **31**, 81.
[2] Hunter, A R, in *Recent Advances in Anaesthesia and Analgesia*, ed C L Hewer, 11nd edn, chap 1. Edinburgh, Churchill Livingstone, 1972.
[3] Carmichael M, personal communication.
[4] Savage, T M, *et al*, *Anaesthesia*, 1975, **30**, 757.

SIR,—Dr I C F Wisely (7 February, p 340) finds that Government expenditure on bone necrosis research is a luxury we cannot afford. I deny this.

I have, at the second Dartford tunnel, 320 men working to establish a new road link between the north and south sides of the river Thames. Scattered about the British Isles are numerous similar tunnels and underneath all our major cities are telephone cable tunnels, sewers, water conduits, and similar structures on which our society depends. Many of these tunnels could not have been constructed without the use of compressed air. The number of men who have been involved in this work up and down the country is somewhat close to the 25 000 sport divers who dive off our summer coasts. Unlike these divers they are at considerable extra risk from dysbaric osteonecrosis, and it was because of these men that the Medical Research Council Decompression Sickness Panel and Registry was first created.

I am probably the biggest single spender of the MRC's money for joint radiographs of these men and of divers. What depresses me most is the very small amount of money that can be made available for this study. I am personally proud to be associated with the work, which has established the diagnostic criteria for this disease throughout the world and continues to evolve higher standards of diagnosis and reporting.

The very fact that long-bone *x*-rays are now standard in our medical fitness examination for divers is clear indication that it has passed beyond the academic and is now of utmost clinical and medicolegal importance. The medical profession of late has been too involved with sordid commercial interests rather than professional activity. I am happy to inform Dr Wisely that my professional activities know no national boundaries; but, holding a British passport, I am jealous to preserve our commercial interests. In his penultimate paragraph Dr Wisely asks for a single clear assessment of the various problems and asks for positive action to be taken. It is unwise for anybody to ask for single clear assessments. There is no single answer to any of the problems that presently beset the world of underwater and underground medicine.

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## Geriatric patients in acute medical wards

SIR,—I have been interested to see the very different interpretations of our paper on this subject (6 December, p 568). Several correspondents have questioned the economics of the study. In reply I would point out that the figure of £178·60 is the 1974 average weekly cost of an acute medical bed in this hospital. It represents the "opportunity cost"—that is, the availability of the acute medical bed. However few of the ward's resources patients use, this opportunity cost remains the same. In contrast to Dr P W Hutton (3 January, p 41) I believe that patient costs do in fact depend on the type of ward in which a patient is treated.

The fixed costs of being able to offer acute medical care are far higher than the variable costs of actually providing it. The facilities associated with acute medicine are expensive—for example, purchase, maintenance, and depreciation of machinery, provision of highly specialised medical, technical, and general hospital staff, who are paid for being available rather than on an item-of-service basis. By comparison the additional costs of actually treating a patient are small—for example, reagents, drugs, syringes, and *x*-ray films.

As Professor B Isaacs (p 40) says, had the study patients been transferred and those waiting taken their places the cost of the 160 bed-weeks might not have been saved in overall terms. However, the money and beds in the acute medical ward could have been used by those in need, while the study patients would have benefited from the specialised resources of a geriatric ward.

Dr Monnica C Stewart's (p 41) mind boggles to read that a patient died for whom medical treatment had been completed. Perhaps she is forgetting that the average age of these patients was over 79 years and that patients, no less than "ordinary people," die some time.

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## Oxytocin and neonatal jaundice

SIR,—Professor E A Friedman and Mr M R Sachtleben's suggestion (24 January, p 198) that operative delivery and not oxytocin induction is responsible for an increased incidence of neonatal jaundice prompts comment.

An association between oxytocin use and neonatal jaundice was first described by Mast and his co-workers in two studies[1 2] which are seldom mentioned by those discussing this field. An association with instrumental delivery was sought but not found. Most of the subsequent work has emanated from Britain, where there has apparently been an epidemic of