had produced remissions in 8 out of 12 patients. There was some support amongst the audience that the enema did work if given in sufficient dosage.

Ole Iversen of Naestved reviewed the local experience with lateral subcutaneous internal sphincterotomy for chronic fissure-in-ano. His results were satisfactory but he pointed out that there was an occasional problem with haematoma formation and prolapsing piles after the operation. These complications could be avoided by using an anal pack as described by Peter Lord.

The final session was a stoma care panel discussion chaired by Mr Brendan Devlin and supported by Coloplast, after which the Section of Proctology returned to Copenhagen by coach to depart the following day for an excellent 48-hour tour around some of the well known sites and cities of Denmark.

A P WYATT
P F SCHOFIELD
Editorial Representatives
Section of Proctology

Letters to the Editor

Epidural blood-patching to treat severe post lumbar puncture headache

From Dr C M Sliom
Director of Obstetrical Anesthesia
Jewish Hospital of St Louis, Missouri, USA

Dear Sir, I have read Dr Perkin's reply to Dr Collier's letter (December 1982 *Journal*, p 987), but I fear that he may not have taken the point of Dr Collier's recommendations. Certainly, no one would recommend that blood-patch 'become routine in the management of patients having lumbar puncture for diagnostic purposes', nor should it be necessary.

Firstly, the incidence and severity of spinal headache can be much reduced by the use of fine-gauge spinal needles and by avoiding multiple punctures of the dura – precautions which the majority of anaesthetists routinely employ, but which our medical colleagues have been slow to adopt.

Secondly, under these circumstances those headaches which do occur will usually be mild and self-limiting, requiring no special treatment other than ensuring adequate hydration. However, when the headache is troublesome and intractable, I fully agree with Dr Collier that epidural blood-patch is the treatment of choice. It is easily performed by anyone familiar with the technique of epidural anaesthesia and is rapidly effective and safe. We have provided almost instantaneous relief to a patient with a severe spinal headache who had been allowed to suffer unnecessarily for more than two weeks following a diagnostic lumbar puncture, because her physicians were not at first aware of the availability of this simple treatment.

I notice that Dr Collier mentions an incidence of headache in 60–70% of his post partum patients following 'dural tap'. I presume he must mean following inadvertent dural puncture during the course of an epidural block using a wide-gauge needle. In our experience the incidence of headache following planned spinal anaesthesia using a fine-gauge needle is much lower than this, although of course still higher than in non-obstetrical patients.

Sincerely

C M SLIOM

5 January 1983

Data on lung cancer in radiation workers

From Dr Robert A Rinsky, Epidemiologist and Dr Philip J Landrigan, Director
Division of Surveillance, Hazard Evaluations and Field Studies, National Institute for Occupational Safety and Health, Cincinnati, Ohio, USA

Sir, In their letter published last October (p 828), Bross & Driscoll refer to a retrospective cohort mortality study of nuclear shipyard workers undertaken by the US National Institute for Occupational Safety and Health (NIOSH) (Rinsky *et al.* 1980), and describe that study as containing a 'number of serious analytic mistakes'. Their contention is that the NIOSH study obscured (deliberately, they imply) a dose-effect relationship between low-level exposure to ionizing radiation and death from lung cancer through the technique of improperly combining productive with unproductive person-years.

That charge is serious, and unchallenged it casts considerable doubt on our scientific

integrity. To reply to it, it will be necessary to recount briefly the history of the mortality study conducted by NIOSH at the Portsmouth (New Hampshire) Naval Shipyard.

The study was begun following publication of a report (Najarian & Colton 1978) which described a five-fold excess of leukaemia mortality in workers involved in the maintenance and overhaul of nuclear submarines at PNS. To evaluate those findings further, NIOSH conducted a retrospective mortality study of the more than 27 000 current and former civilian workers employed at PNS since 1952; over 9000 of these men had been radiation workers. We succeeded in tracing and in ascertaining the vital status of 98% of these workers. Then for each of 83 specific causes of death, we calculated expected numbers of deaths adjusted for age, sex, race and calender time period and stratified by five-year latency periods, by five-year duration-of-employment periods, and (for radiation workers) by cumulative radiation exposure.

When we compared the number of deaths observed in each category with the number expected, we found no evidence for the previously reported five-fold excess in leukaemia mortality, in spite of a statistical power of greater than 99% to detect such an increase. Further, we found no statistically significant excesses in mortality due to any of the other causes of death which we examined. We have since been able to determine that misclassification of occupational history data accounted for the previously reported excess of deaths due to leukaemia (Greenberg *et al.* 1982).

Subsequent to the release of our results, Bross obtained from us copies of all computer printouts which we had produced during our analysis. Included in this material were tables showing numbers of observed and expected deaths by five-year latency and duration categories, and by radiation dose categories for each of the causes of death which we evaluated; in all, the tables contained over 4000 cells. Apparently in the absence of any *a priori* hypotheses, Bross & Driscoll then proceeded to examine these tables seeking possible associations between radiation exposure and mortality from any cause. From the countless thousands of permutations available to them, Bross & Driscoll perceived a positive relationship between radiation exposure and lung cancer in those workers who had accumulated more than 15 years' latency (i.e. elapsed time from first radiation exposure at PNS) and who had cumulative radiation exposures of one rem or more (Bross & Driscoll 1981).

This observation of possible excess lung cancer mortality in certain workers exposed to low-level ionizing radiation cannot be ignored. Indeed, our group at NIOSH has already embarked on a case-control analysis of deaths from respiratory cancer in the PNS cohort; in that analysis we shall evaluate possible associations between respiratory cancer and occupational exposures to radiation as well as to other pulmonary carcinogens, such as asbestos, known to have been present in the shipyard.

At the same time, however, it is inappropriate of Bross & Driscoll to have presented their observations as scientific conclusions or, worse, as received truth. In extracting their observations from the PNS data, Bross & Driscoll appear to have ignored the fundamental statistical fact that at a 95% confidence level, 5% of findings will differ from expectation on the basis of chance alone and will thus appear to be statistically significant; Bross & Driscoll provide no information as to the number of possible associations which they may have examined in their perusal of PNS data. Bross & Driscoll's observations should, quite properly, be considered as suggestions for further research. Over-interpreted or taken out of context, however, their findings illustrate the pitfalls of evaluating complex data sets without benefit of an *a priori* hypothesis (Feinstein & Horwitz 1982).

ROBERT A RINSKY
PHILIP J LANDRIGAN
22 December 1982

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Amoebiasis

From Mr P G Sargeaunt
Department of Medical Protozoology
London School of Hygiene and Tropical Medicine

Sir, I refer to the letter from Drs Robertson, McMillan and Young published last December (p 987). The possible consequences of *Entamoeba histolytica* being present in the lumen of the intestine have been acknowledged since this organism was recognized in 1873. The fact that *E. histolytica* colonizes is irrelevant to this argument since all intestinal amoebae do this. Presumably it is not suggested that, for example, *Endolimax*

nana presenting under the same circumstances should be regarded as a potential pathogen? My groupings are not based on isoenzyme findings alone: they are based on clinical observations, eventually matched in blind studies to isoenzyme patterns (*see* Sargeaunt & Williams 1979, Sargeaunt *et al.* 1978, 1980, 1982*a,b,c,d*). Regarding statistics, my thesis is founded on the examination to date of 1423 subjects harbouring *E. histolytica*. Organisms expressing zymodeme I have never been demonstrated in tissue, but have only ever been found in the gut lumen.

To assess the ability of this particular organism (zymodeme I) to invade requires a prolonged study, in all age groups and both sexes. Assuming that *E. histolytica* was the only species of amoeba present in the subjects studied by Dr Robertson and his colleagues, then the presence of *E. histolytica* zymodeme I cysts (apparently not even haematophagus trophozoites) in the subjects, coupled with mucosal reaction, does not constitute a challenge to our hypothesis on zymodeme-related pathogenicity. We do not, however, deny that so far unidentified host factors may play a role in pathogenesis in exceptional cases.

P G SARGEAUNT
18 January 1983

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Treatment of cramp

From Dr Stanley Rivlin
London NW1

Sir, The wisdom of prescribing drugs for the treatment of nocturnal calf cramp in pregnancy (December 1982 *Journal,* p 988) should be questioned.

In practically every case, pregnancy cramp arises from the presence of dilated veins in the limbs which shoot a massive amount of blood into the calf muscles when the patient flexes or extends the foot at the ankle joint when 'stretching' in bed and thus, inadvertently, using the calf muscle pump to suck this vast amount of blood from the subcutaneous tissues into the muscles.

The treatment is simple. Instruct the patient to sleep with the foot of the bed raised 9 inches. This empties the legs of excess venous blood within 10 minutes of going to bed, and the cramp ceases from that moment and does not reappear. One of the minor virtues of this method is that, in addition to being inexpensive, it is also 100% successful.

The only contraindication is the presence of severe oesophageal reflux (heartburn) and then it is up to the patient to decide which is preferable – cramp or heartburn; although, of course, the heartburn can always be mitigated by the use of an antacid.

Yours faithfully
STANLEY RIVLIN
10 January 1983

Calcium metabolism during rifampicin and isoniazid therapy for tuberculosis

From Dr W Perry
Faculty of Medicine
King Faisal University, Dammam, Saudi Arabia

Dear Sir, I should like to reply to the interesting comments of Drs Brodie and Hillyard published last November (p 919). Our paper (July 1982 *Journal,* p 533) was mainly concerned to observe a clear clinical effect on calcium metabolism in patients receiving rifampicin and isoniazid. Only nutritionally-deficient Indians had evidence of osteomalacia during therapy, whereas Europeans showed no evidence, suggesting that any effect over an 18-month period was likely to be small

Of course, a small effect may be of some importance in the Indian group, but during metabolic calcium balance the osteomalacic patients had increased intestinal calcium absorption during mid-summer. This experiment provides good evidence of normal vitamin D function during combined therapy. What it does not do is exclude a direct effect of the drugs on bone itself, for which there is little evidence at present. Brodie and his colleagues (1982) agree that the combined effect of both drugs was less than they predicted from the two drugs alone, a point which we were at some pains to emphasize, and that only the 25-hydroxycholecalciferol parameter was reduced at six months. However, their patients had also received ethambutol and thus a different pharmacokinetic situation was being observed compared to our patients on rifampicin and isoniazid alone.

The data they used from the British Thoracic Association (1981) study did show a small but

significant hypocalcaemic effect at six months (Brodie *et al.* 1981), but if a large proportion of them were Indian we would expect a further deterioration in their calcium levels with time from pure D deficiency. The case report of rifampicin osteomalacia (Shah *et al.* 1981) was very inadequately documented (Perry 1983) and cannot usefully be cited as evidence in favour of their hypothesis.

If rifampicin and isoniazid do have an effect it must be a small one, for in our Indian D-deficient subjects it was adequately overcome by 900 units of vitamin D_2 per day.

Yours sincerely

W PERRY

28 December 1982

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Globus hystericus and the masticatory apparatus

From Dr G M Ardran
Clinical Reader in Radiology
Radcliffe Infirmary, Oxford

Dear Sir, Dr Myrhaug's letter (February *Journal,* p 162) is of interest, in making the suggestion that abnormalities of the masticatory apparatus may be the cause of a feeling of a lump in the throat usually described as globus hystericus though recognized by many as infrequently due to hysteria.

I would absolutely agree that many patients suffering from lack of dentures or ill-fitting dentures will have problems with chewing and resultant indigestion. However, I do not believe that the majority of patients who complain of a lump in the throat are suffering from significant problems with their masticatory apparatus. It must also be remembered that a lump in the throat can be purely emotional and intermittent; and many individuals are cured by treatment of their gastric reflux. If 'lump in the throat' were due to abnormalities of the masticatory apparatus, one would expect the symptom to be relatively constant.

I appreciate that it is always difficult to sort out causes and effects when something is fairly common and may have more than one cause.

Yours sincerely

G M ARDRAN

10 January 1983

Unreliable 'memories' under hypnosis

From Dr David Waxman
Chairman, British Society of
Medical and Dental Hypnosis

Dear Sir, I refer to the valuable and timely comments of Professor Max Hamilton (January *Journal,* p 82) concerning the recent paper by Dr Wagstaff (October 1982, p 793).

The use of hypnosis for the recall of earlier memories is far from infallible, and Professor Hamilton's important point should be extended even further. Orne (1979) shows that subjects may not only confabulate under hypnosis, but may also wilfully lie or even simulate the hypnotic state for their own purposes.

In clinical use, regression under hypnosis may on rare occasions produce an abreaction resulting in a dramatic therapeutic effect, but as Freud discovered one hundred years ago, what the subject says happened is not necessarily accurate historical fact.

This possibility should be known to all those who use hypnosis, either as a therapeutic agent or for forensic purposes.

Yours faithfully

D WAXMAN

2 February 1983

Reference

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Swimming and grommets

From Mr N J Kay
Department of Otolaryngology
Leeds General Infirmary

Dear Sir, The matter of counselling patients with grommets was rationalized from elegant hydrodynamic theory and observation by Marks and Mills (January *Journal,* p 23). However, the mode of anaesthesia was an important variable omitted from the observations of the 12 anaesthetized patients. If nitrous oxide was used, the increased middle ear pressure thereby incurred would surely impede or even reverse the movement of water within the grommet lumen (Singh & Kirk 1979). It would be important to know the number of ears examined in the anaesthetized group and their distribution on the histogram.

Yours faithfully

NICHOLAS J KAY

Reference

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Amphotericin B versus miconazole in treatment of candidal infection

From Dr Peter J Wyld
Haematology Department
Christchurch Hospital, New Zealand

Dear Sir, I am grateful for the opportunity to respond to the questions raised by Dr Wise (March *Journal*, p 233).

Miconazole has, of course, been used – albeit with anecdotal success – in the management of systemic candidiasis, and it was with these early reports in mind that we had some enthusiasm for its use in certain circumstances (Medoff & Kobayashi 1980). The text of our paper (Slater *et al.*, November 1982 *Journal*, p 875) does not make it clear, due to our own editing, that both patients were treated with miconazole initially for extensive oral candidiasis. When it was evident that they had invasive disease, we felt that their generally poor clinical state precluded a change of therapy. It was and is our general policy to use amphotericin B in the management of presumed or proven systemic candidiasis, and this drug remains the first-line therapy.

I am not convinced that the use of serological titres helps differentiate systemic from localized candidiasis. Even newer techniques have similar difficulty in clearly separating these patients (Segal *et al.* 1979, Eng *et al.* 1981).

Yours faithfully

PETER J WYLD

24 January 1983

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Mammary tuberculosis

From Dr B G Rigden
Centre for Medical Research
University of Sussex

Dear Sir, In connection with the recent editorial on mammary tuberculosis (October 1982 *Journal*, p 764), may I draw your attention to the informative and interesting paper by K P Goldman published in *Tubercle* (1978, **59,** 41–45). In this paper 5 cases histories are described and the whole condition thoroughly discussed.

Yours faithfully

B G RIGDEN

The authors of the *Journal* editorial have commented as follows:

Dear Sir, We are aware of the paper by Goldman referred to above, in which the work of Raw is also cited (*British Medical Journal* 1924, i, 657–658), but in the interests of brevity discussion of Goldman's work and that of a few others was omitted from our editorial.

HAROLD F HAMIT

THOMAS H RAGSDALE

Book reviews

Human Conception *In Vitro*

R G Edwards & J M Purdy (ed)
pp 435 £20.00 London: Academic Press 1982

This book records the proceedings of the first Bourn Hall symposium on *In Vitro* Fertilization (IVF). Participants were invited from eleven prominent groups working in the field in Europe, North America and Australia. The papers presented encompass the full breadth of the subject, from ethics to fine procedural detail.

There are five parts to the book and each contains authoritative manuscripts combined with a report of results and problems encountered in day to day clinical practice. A report of the discussion on the papers concludes each part. A particularly topical and controversial issue is the discussion on what to do with the 'spare' embryo.

This book is not for those looking for a broad overview of the subject. However, for those interested in establishing an IVF service, it must be considered the standard text. The world's most successful proponents of the art of IVF keep no secrets but rather share their experiences and give enough scientific detail to allow others to copy their methods. At £20 it represents good value for a detailed reference book on a subject of great current interest.

MALCOLM H M TUCKER
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