South East Thames regional drug information pharmacists at present collating patient information leaflets produced by both the pharmaceutical industry and other sources I want to inform your readers that we hope to publish an even more comprehensive list in the future. In the meantime, I want to remind your readers that throughout the country the drug information pharmacists, who are mainly hospital based, are pleased to help doctors and members of other health care professions—based both in general practice and in hospital.

ELAINE HONEY

Royal Sussex County Hospital,
Brighton BN2 5BF

¹ Honey E. Medicine and the media. *Br Med J* 1980; **281**:674-5.

² Honey E. Additional instructions for medicines to aid patient compliance. Liverpool: Mersey Regional Health Authority, 1981.

*** This correspondence is now closed.—ED, *BMJ*.

Buprenorphine and pethidine in acute pancreatitis

SIR,—Mr S L Blamey and others randomised patients with acute pancreatitis to receive either 0.3 mg buprenorphine or 100 mg pethidine intramuscularly, and they assessed pain before and about 24 hours after analgesia with visual analogue and ordered category scales (19 May, p 1494). From their results they conclude that pethidine and buprenorphine provide comparable pain relief in acute pancreatitis. But 13 of 17 buprenorphine and 14 of 15 pethidine treated patients demanded additional pethidine after a mean of 9.2 and 9.5 hours respectively, and inevitably assessment of pain after 24 hours would have been influenced by supplementary injections of pethidine.

The authors also suggest that the duration of pain relief, as determined by the patient's demand for further analgesia, was comparable in the two groups. Unfortunately pain does not appear to have been assessed formally before additional pethidine was given, and the supply of further analgesia may have depended on factors other than duration of action of the trial drug. Although both drugs were initially given from identical ampoules it is not clear whether the study was conducted single or double blind. Interestingly in another study with postoperative patients intravenous buprenorphine (5 µg/kg) had a considerably longer duration of action than intravenous pethidine (1 mg/kg), but attempts to conduct the study blind were limited because buprenorphine caused marked miosis.¹

If Mr Blamey and others had assessed pain in their patients more often and before additional analgesia was required different results might have been obtained. The data provided do not substantiate the authors' conclusions, and further information about the use of pethidine and buprenorphine in acute pancreatitis is required.

R A HENDERSON

Royal United Hospital,
Bath BA1 3NG

¹ Kamel MM, Geddes IC. A comparison of buprenorphine and pethidine for immediate post-operative pain relief by the intravenous route. *Br J Anaesth* 1978;**50**:599-603.

SIR,—Mr S L Blamey and others state that respiratory depression did not occur in their patients. How did they reach this conclusion? What did they measure—respiratory rate, minute ventilation, arterial pressure of carbon dioxide, or depression of the carbon dioxide response curve? The price of opioid analgesia is usually some ventilatory depression. What is the evidence that this was not the case in these patients?

J F SEARLE

Royal Devon and Exeter
Hospital (Worford),
Exeter EX2 5DW

*** Professor Carter replies below.—ED, *BMJ*.

SIR,—Assessment of pain was undertaken on only one occasion at a maximum of 24 hours after giving the trial drugs. In practice, this was the morning after emergency admission. Using standard techniques patients were asked to indicate the pain experienced before initial drug treatment and the least pain experienced after the trial drugs. The visual analogue scale is highly reproducible in this respect with 95% confidence limits $\pm 2\%$ at 24 hours.¹

We agree buprenorphine causes miosis, rendering studies of intravenous administration single blind. The present study remains double blind because the trial drugs were given intramuscularly by nursing staff from identical coded ampoules. Pain was assessed retrospectively by a separate medical observer after the action of the trial drug had ceased. We therefore consider our conclusions to be valid.

The prognostic factor scoring system for acute pancreatitis referenced in the methods requires estimation of arterial blood gases since hypoxaemia is a feature of the disease and is of prognostic importance. The routine policy in this hospital is to obtain arterial blood gases soon after admission (one to two hours) and again at about 12 hours. In addition respiratory rate is recorded four hourly. No difference between the treatment groups was observed. More sophisticated testing for respiratory function was not employed.

D C CARTER

Department of Surgery,
Royal Infirmary,
Glasgow G4 0SF

¹ Revell SI, Robinson JO, Rosen M, Hogg MI. The reliability of a linear analogue for evaluating pain. *Anaesthesia* 1976;**31**:1191-8.

Acute obstructive hydrocephalus after subarachnoid haemorrhage

SIR,—It is a common sequence of events for a patient to deteriorate after subarachnoid haemorrhage and then improve. We are surprised that Mr Y M Kohi and others (5 May, p 1342) should interpret the deterioration in their patients as being due to acute obstructive hydrocephalus on the basis of the clinical details presented and the computed tomogram. Blood within the ventricular system is commonly seen on the scan after subarachnoid haemorrhage and may persist for several days without clinical evidence of disturbance of cerebrospinal fluid flow.

Our experience is that patients with acute

obstructive hydrocephalus deteriorate dramatically and the condition is infrequently associated with an intraventricular clot. Hydrocephalus after subarachnoid haemorrhage is almost invariably of a communicating nature and only rarely obstructive (with extrinsic compression of the cerebrospinal fluid pathways). In any case the deterioration may be reversed by ventricular drainage (although compressive haematomata require evacuation) while drainage by lumbar puncture alone often proves adequate in communicating hydrocephalus.

Do Mr Kohi and his colleagues suggest that external ventricular drainage helped their two patients by relieving pressure and so improving cerebral perfusion or by allowing removal of blood and its breakdown products? We think that, although they describe a legitimate surgical procedure, the improvements may have been purely coincidental.

COLIN L SHIEFF
ANDREW M THOMAS

Queen Elizabeth Hospital,
Birmingham B15 2TH

*** Mr Kohi and others reply below.—ED, *BMJ*.

SIR,—We also find that after subarachnoid haemorrhage acute obstructive hydrocephalus associated with intraventricular clot is uncommon, but its incidence is unknown. Hydrocephalus in this situation is most often of the communicating type (non-obstructive). Blood may occur anywhere within the cerebrospinal fluid pathways, but our report is based on computed tomographic evidence of fourth ventricular outflow obstruction by clot associated with dilatation of the lateral ventricles.

Both patients improved quickly after ventricular drainage suggesting that relief of raised intracranial pressure was a contributing factor in their improvement. We do not think that it is reasonable to speculate further on possible pathophysiological mechanisms based on only two clinical observations and without data on perfusion pressures.

The practice of cerebrospinal fluid drainage sometimes used for patients with communicating hydrocephalus after subarachnoid haemorrhage could usefully be extended to include this group of patients.

YADON KOHI
ROBIN A JOHNSTON
U P DEVKOTA

Institute of Neurological
Sciences,
Southern General Hospital,
Glasgow G51 4TF

British cosmetic regulations inadequate

SIR,—Dr C M Ridley and others (19 May, p 1537) describe a case of patchy depigmentation of the skin following the use of Venus de Milo cream, which contains hydroquinone. They suggest that the cosmetic regulations ought not to allow manufacturers to name the constituent without stating its function. We wish to report the use of a skin lightening cream containing monomethyl ether of hydroquinone, which is described on the label as merely containing hydroquinone, a less potent depigmenting agent.¹ The product caused severe patchy depigmentation and

considerable psychological trauma to our patient.

A 19 year old West Indian girl applied Drula Bleaching Wax for two or three months in order to lighten the colour of her skin. She subsequently developed severe irregular hypopigmentation of her hands, arms, neck, and legs. Despite discontinuing the use of the cream hypopigmentation continued for some weeks.

The monomethyl ether of hydroquinone, present in the product used by our patient, is a popular depigmenting agent in Europe but is used infrequently in the UK. It is closely related chemically to the monobenzyl ether of hydroquinone, which is prohibited from use in cosmetic products in the UK because of its known severe and irregular depigmenting qualities. In contrast hydroquinone is generally considered safe from these adverse reactions. We wish to draw attention to the disfiguring cosmetic results in our patient following the application of monomethyl ether of hydroquinone.

A large number of skin lightening preparations are readily available and other products may contain monomethyl ether of hydroquinone. At present all cosmetic products containing hydroquinone must state that they contain hydroquinone and the concentration must be less than 2%. We support Dr Ridley and others' argument and suggest that the cosmetic regulations dealing with depigmenting agents should be reviewed. We further propose measures to ensure correct labelling, with the active constituent of cosmetic products of this nature clearly stated.

J BOYLE
C T C KENNEDY

Department of Dermatology,
Bristol Royal Infirmary,
Bristol BS2 8HW

¹ Brun R. Contribution à l'étude de la dépigmentation expérimentale. *Dermatologica* 1959;118:202.

Acupuncture

SIR,—Dr George T Lewith's review of clinical trials of acupuncture (19 May, p 1475) divided these into those comparing acupuncture with conventional treatment, those comparing it with random insertion of needles, and those comparing it with a physical placebo. He concluded that acupuncture has an analgesic effect in about 60% of patients.

Dr Lewith referred to our study among those comparing acupuncture with the random insertion of needles. In fact our placebo technique was rather more complicated.¹ It used a homogenous patient group and was designed so that a 60% response rate to acupuncture and a 35% response rate to placebo would be recognised as a significant difference. Dr Lewith did not indicate that placebo controlled studies of acupuncture have repeatedly shown that there is no significant difference in the extent of pain relief obtained by acupuncture compared with placebo.² Our study confirmed the results of others, and we concluded that although acupuncture can increase the endorphin level in the cerebrospinal fluid and that its analgesic effect can be blocked by naloxone, the placebo component is clinically more important than its physiological effects.³⁻⁵

The "sceptics" to whom Dr Lewith refers have been supported by the studies published to date. It is now up to the acupuncturists to conduct valid trials confirming the efficacy of acupuncture if it is to become more than another fringe treatment based on belief rather than clinically demonstrable effectiveness. Studies have so far failed to confirm the various

extravagant claims made for acupuncture by enthusiastic practitioners.^{9,10}

G MENDELSON

Melbourne

- ¹ Mendelson G, Selwood TS, Kranz H, Loh TS, Kidson MA, Scott DS. Acupuncture treatment of chronic back pain: a double-blind placebo-controlled trial. *Am J Med* 1983;74:49-55.
- ² Mendelson G. Acupuncture analgesia: I. Review of clinical studies. *Aust NZ J Med* 1977;7:642-8.
- ³ Lee PK, Andersen TW, Modell JH, Saga SA. Treatment of chronic pain with acupuncture. *JAMA* 1975;232:1133-5.
- ⁴ Gaw AC, Chang LW, Shaw LC. Efficacy of acupuncture on osteoarthritic pain. *N Engl J Med* 1975;293:375-8.
- ⁵ Moore ME, Berk SN. Acupuncture for chronic shoulder pain. *Ann Intern Med* 1976;84:381-4.
- ⁶ Edelist G, Gross AE, Langer F. Treatment of low back pain with acupuncture. *Can Anaesth Soc J* 1976;23:303-6.
- ⁷ Sjolund B, Terenius L, Eriksson M. Increased cerebrospinal fluid levels of endorphins after electroacupuncture. *Acta Physiol Scand* 1977;100:382-4.
- ⁸ Mayer DJ. Pain inhibition by electrical brain stimulation: comparison to morphine. *Neurosci Res Program Bull* 1975;13:94-100.
- ⁹ Gold J, Wilcox R. Acupuncture: why we don't use it more often. *Med J Aust* 1983;2:4-5.
- ¹⁰ Mendelson G. Acupuncture. *Med J Aust* 1984;140:181.

SIR,—Dr G T Lewith (19 May, p 1475) cites 20 controlled clinical trials of acupuncture versus placebo or conventional treatment. From these he concludes that acupuncture has an analgesic effect in about 60% of patients with chronic pain, and that it may be more effective than conventional treatment in some specified conditions.

Of these 20 studies 11 showed no real difference between the effects of acupuncture, placebo, or conventional treatment. Six had either serious methodological flaws, very small numbers, or both, that cast doubt on the validity of their conclusions. Three appeared to show a significant difference. Of these three studies one was not randomised, the treatment groups were not comparable, and the statistical analysis was faulty.¹ When reanalysed using conventional statistical methods there was no real difference between acupuncture and steroid injections. The second of these three studies analyses the results using a non-parametric method and a one tailed test of significance which fails to achieve statistical significance if the more usually applied two tailed test is used.² The last of the three studies appears in the *American Journal of Acupuncture*, which has proved difficult to get hold of at short notice.³

Could Dr Lewith tell us on which of these trials he bases his conclusions?

JOHN ROBSON

South Poplar Health Centre,
London E14

- ¹ Brattberg G. Acupuncture therapy for tennis elbow. *Pain* 1983;16:285-8.
- ² Hansen PE, Hansen JH. Treatment of facial pain by acupuncture. *Acta Neurochir* 1981;59:279.
- ³ Junnila SYT. Acupuncture therapy for chronic pain: a randomised comparison between acupuncture and pseudo-acupuncture with minimal peripheral stimulus. *American Journal of Acupuncture* 1982;10:259-62.

*.*The author replies below.—Ed, *BMJ*.

SIR,—My statement that "the results give an overall impression that acupuncture has an analgesic effect in about 60% of patients with chronic pain is based on an overview of all the data available. My method of reaching this conclusion is described in detail elsewhere,¹ but in simple terms it involves averaging the

effects claimed for acupuncture in all the 20 studies.¹ Statistically this information is of limited value but it provides some sort of rational answer to the question "How effective is acupuncture?"

I stated in my leading article that many of the studies are poorly designed. The main point I was attempting to make was not to "prove" acupuncture is effective, but to cite the inadequate evidence available and suggest how acupuncture could be further investigated in an objective and critical manner. To be scientific we must begin with some assumptions which are necessarily based on inadequate evidence, and which may be proved ultimately to be false.

GEORGE LEWITH
Director

Centre for the Study of Alternative Therapies,
Southampton SO1 2DG

¹ Lewith GT. How effective is acupuncture in the management of pain? *J R Coll Gen Pract* 1984;34:275-8.

Psychosocial stress in pregnancy and its relation to low birth weight

SIR,—We commend Dr R W Newton and Miss Linda P Hunt for the careful methodology of their prospective study into psychosocial stress and birthweight (21 April, p 1191) but unfortunately the same cannot be said for their interpretation of the results. They describe an inverse relation between the life events of the pregnant woman during pregnancy and the child's birthweight. They admit that life events did not add measurably to the effects that the mother's smoking has on birthweight, but they still maintain that there is an effect of stress on birthweight. Even if the effect of smoking were to be ignored they have misinterpreted their life events data.

The association between life events and disease is that "stressful life events, by evoking psychophysiological reactions, play an important causative role in the natural history of disease."¹ Life events are extremely complex, and any broad theory which does not state which psychophysiological reactions are to be considered stressful or the mechanisms by which disease is caused is likely to be of little scientific use. With few exceptions life events are as much a consequence of behaviour as a determinant, and in addition to any psychophysiological changes related to "stress" many other changes precede and follow an important life event.²⁻⁴ It is extremely difficult, therefore, to implicate life events as causal rather than confounding factors in health outcomes with any confidence and even more difficult to show that any effect is due to stress.

Dr Newton and Miss Hunt's study is not discredited by these criticisms. The prospective design does mean that a case for an effect of life events on birthweight could be made. The finding that mothers of preterm and low birthweight babies reported considerably more life events in the last trimester of pregnancy than controls is particularly interesting as fetal weight increases most during this period. It is surprising, however, that for the whole sample the incidence of life events differs dramatically between trimesters. If the incidence of life events were unrelated to the mother's behaviour no difference in incidence between trimesters would be expected. Yet Dr Newton and Miss Hunt report a reduced