successfully for resources within district or area.

If prenatal diagnosis has to be done, let us at least do it properly.

RODNEY HARRIS

Department of Medical Genetics,
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¹ Ferguson-Smith, M A, *Lancet*, 1976, **2**, 252.

² Evans, J A, Hunter, A G W, and Hamerton, J L, *Journal of Medical Genetics*, 1978, **15**, 43.

³ Hagard, S, and Carter, F A, *British Medical Journal*, 1976, **1**, 753.

Hazards of epilepsy

SIR,—I was interested to read your leading article on this subject (3 June, p 1436), especially that section in which you state that "from the infrequency of reports of death during sleep apparently due to suffocation during a fit the hazard may be presumed rare—especially in comparison with the frequency of nocturnal fits in children. Even so, substitution of a firm pillow might be a wise safeguard."

This statement is certainly contrary to the conclusions drawn by my colleague Dr H J A Longmore and myself in 1970, when three patients with grand mal epilepsy who had died by suffocation in their pillow were reported.¹ This article stimulated the reply from Dr C Worster-Drought² on which was based the comment referred to above. Dr Worster-Drought had, during his 45 years as a neurologist in Harley Street, seen no such cases and considered them uncommon.

My own view is that such tragic deaths are not so uncommon and that most of them occur in the home so that their frequency is not generally appreciated—certainly not by consultant neurologists. To emphasise the need for patients with grand mal epilepsy to use smother-proof pillows a further letter was written in 1973 describing two further cases which had occurred in Dumfriesshire within a 10-year period.³ Should this incidence be typical of the rest of the country this would mean that 20 cases would occur in Scotland each year—no negligible number. An article from Denmark⁴ helps to bear out my contention. The number of deaths in epilepsy was found to be increased in the fourth and fifth decades by 413 and 558%, respectively.

From my own findings and from the fact that six other cases have come to my notice since the publication of our original article (the latest, reported in the *Bury Times* on 6 January this year, described an epileptic who had suffocated in his pillow during a fit, this time in hospital with the diagnosis verified at post-mortem examination) I would suggest, contrary to the statement in your leading article, that many deaths in younger epileptics do occur from suffocation during a fit.

JOHN WILSON

Lockerbie, Dumfriesshire

¹ Longmore, H J A, and Wilson, J B, *Lancet*, 1970, **2**, 782.

² Worster-Drought, C, *Lancet*, 1970, **2**, 876.

³ Wilson, J B, *British Medical Journal*, 1973, **2**, 173.

⁴ Henriksen, P B, et al, *Acta Neurologica Scandinavica*, 1967, **43**, suppl 31, p 164.

Spontaneous abortion and fetal abnormality in subsequent pregnancy

SIR,—In his criticism of our paper (22 April, p 1016) Dr W H James (27 May, p 1417) misrepresents one point and ignores another.

Firstly, he states that *in so far as the malformations in our index cases were anencephaly or spina bifida (ASB) (our italics) we interpreted the data as favouring the "rest" hypothesis*. This is quite untrue, the whole point of our paper being to report that in the 256 index cases, where the last pregnancy had ended in a spontaneous abortion there was a statistically significant increase in congenital malformations involving several different systems as compared with controls. Our summary concluded: "This may possibly be explained by the trophoblastic 'rest' hypothesis and suggests that spontaneous abortions are more relevant to congenital abnormalities than has been thought." As stated in our paper, it seems to us highly improbable that each previous abortion was affected by the same abnormality but to a more lethal degree.

Secondly, he ignores the findings in our earlier paper¹ in which we reported that mothers of ASB babies were significantly more likely to have had either a spontaneous abortion or a stillbirth immediately before than immediately after the abnormal baby ("middle child" effect). Agreed that it is unknown whether the spontaneous abortions were themselves ASB conceptuses, but it is certain that the stillbirths were not, because, in the series we studied, where they were so affected this was recorded and we then used them as index cases. This was also referred to in our recent paper, but was not mentioned by Dr James either in his letter to you or in his earlier paper.²

A way of testing the "rest" hypothesis has been suggested to us by Professor K D Bagshawe. He thought it might be profitable to investigate for congenital abnormalities the subsequent pregnancies of women who have had a hydatidiform mole, because in this condition it is known that "rests" sometimes occur, and their presence is indicated by high human chorionic gonadotrophin levels. One of us (JC) has started to look at Professor Bagshawe's data and the survey seems to us a much more practical proposition than that put forward by Dr James in his letter.

A J GARDINER

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CYRIL A CLARKE
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O M MCKENDRICK

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Liverpool Corporation,
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¹ Clarke, C A, et al, *British Medical Journal*, 1975, **4**, 743.

² James, W H, *British Medical Journal*, 1978, **1**, 72.

Adjuvant chemotherapy in T3 bladder cancer

SIR,—Two-thirds of patients diagnosed as having T3 bladder cancer die within three years, whether they are treated by surgery,¹ radiotherapy,^{2,3} or the combination.³ More than half of those dying within this period do so as the result of metastases which were not evident at the time of diagnosis. The possibility of decreasing this mortality by attacking the micrometastases which must be present at the time of diagnosis in many patients is attractive.

Adjuvant chemotherapy has been strongly advocated by Prout⁴ and DeKernion,⁵ and a toxicity study recently carried out by the

Yorkshire Urology Cancer Research Group on 18 patients has shown that the morbidity of a combination of radical radiotherapy with doxorubicin (Adriamycin) and 5-fluorouracil is acceptable.⁶ The two-year survival figures for these patients are now available and will be presented to the plenary session of the European Organisation for Research into the Treatment of Cancer on 27 June. The numbers involved are small, but the survival figures are much better than were expected and justify a randomised prospective study to find out whether adjuvant therapy is advantageous.

Such a trial is now in progress and 46 patients have been entered so far. It is clearly desirable to complete this study as soon as possible, and it would be appreciated if anyone interested would contact Mr Brian Richards, Department of Urology, York District Hospital, Wigginton Road, York YO3 7HE, for further information.

BRIAN RICHARDS
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ROBIN GLASHAN
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¹ Wajzman, C, et al, *Journal of Urology*, 1975, **113**, 806.

² Rider, W D, and Evans, D H, *British Journal of Urology*, 1976, **48**, 595.

³ Wallace, D M, and Bloom, H J G, *British Journal of Urology*, 1976, **48**, 587.

⁴ Prout, G R, *Cancer Research*, 1977, **37**, 2764.

⁵ De Kernion, J B, *Cancer Research*, 1977, **37**, 2771.

⁶ Glashan, R W, Houghton, A L, and Robinson, M R G, *British Journal of Urology*, 1977, **49**, 669.

Thyroid extract

SIR,—We write to suggest that thyroid extract (Thyroid, BP) be removed from the *British Pharmacopoeia* and that its manufacture be abolished.

We continue to see patients who have been diagnosed as having myxoedema and who are being treated with apparently adequate doses of thyroid extract but who are clinically and biochemically hypothyroid. They have subsequently responded to thyroxine.

Although it is never possible to be certain that drugs prescribed are being taken, there is good evidence that the potency of thyroid extract is variable and its shelf-life dated.¹ As both active constituents, thyroxine and triiodothyronine, have been available for many years we see no reason for the retention of thyroid extract, which we consider to be dangerous.

W VAN'T HOFF
R HOFFENBERG
D R LONDON
R HALL
G F JOPLIN

G M BESSER
J S STAFFURTH
DAVID C ANDERSON
J JENKINS
R L HIMSWORTH
PETER SONKSEN

¹ Martindale: *The Extra Pharmacopoeia*, ed A Wathe, 27th edn, p 1509. London, Pharmaceutical Press 1977.

Living with artificial valves

SIR,—I write to concur with the policy outlined in your leading article (10 June, p 1505) stressing the advantages of an imperfect natural mitral valve over those of an artificial one of whatever type.

I have been preaching the policy of conservation of the mitral valve wherever possible for the last 13 years,¹ starting at the time when the enthusiasm for valve replacement was at

its height. As your leader implies, the long-term follow-up has considerably diminished this enthusiasm and a conservative policy is now rightly more frequently adopted.

I believe that stenotic mitral valves should almost never be replaced, that patients with some degree of mitral incompetence may have little disability for many years after commissurotomy, and that even heavily calcified mitral valves can be successfully treated by simple valvotomy.²

Lastly, although the relevant merits of open and closed valvotomy have not yet been firmly established, the risk of unnecessary valve replacement with all its attendant evils must be increased when an open operation is done.

J R BELCHER

Middlesex Hospital,
London W1

¹ Belcher, J R, *Thorax*, 1973, 28, 608.

² Smith, G H, and Belcher, J R, *British Heart Journal*, 1970, 32, 198.

Uniform style for biomedical journals

SIR,—I wonder whether Mr Roger Hole (3 June, p 1483) was objecting to "unpublished work" or "personal communications" rather than "in press" papers. The first two should certainly be barred from reference lists. Professor W B Jennett (24 June, p 1699) is, of course, right about "in press" papers being perfectly acceptable. If the journal's name can be given (and preferably the title of the paper) any interested reader can eventually find a paper of this kind. But referees should perhaps examine reference lists of manuscripts they are reviewing and then ask to see copies of papers listed as being "accepted" or "in press"—as suggested by Professor S Saxov at a recent conference of editors (Editerra Fourth General Assembly, joint meeting with ELSE).

MAEVE O'CONNOR

*.*Since "in press" implies that the paper has definitely been accepted for publication in a specific journal the name of that journal should always be given in such references. But since, as pointed out by Professor Jennett, the interval between acceptance and publication is often long, the date and volume number cannot be given without risk of inaccuracy.—ED, *BMJ*.

SIR,—In response to your leading article (20 May, p 1302) I have the following comment and proposal to make. One great advantage of the Harvard system is that creditability of a quoted observation can be readily assessed. Thus I find myself frequently having to look at the list of references to see just who proved what and when when reading the journals using the number system. The Harvard system saves a great deal of time and also refreshes my memory of important observations made by others. As such, it more closely reflects a scientific discussion between colleagues or at "grand rounds," where one quotes Harvey, Osler, Salk, etc, but never 13, 41, 57, and sq forth. To be continually reminded by the Harvard system by whom a certain observation was made is a positive learning experience which I believe we cannot afford to lose for sake of uniformity. The argument that the head of a unit's name might appear too far down the list and may be deleted in subediting

may be dealt with by placing the head of a unit's name in a constant position (for instance, second or third position) or alternatively by limiting the number of authors for any given article.

G TEVAARWERK

London, Canada

*.*The placing of the name of the head of a unit at the end of a list of authors is often deliberate, indicating that he exercised only a supervisory role while the previously named authors bore the chief responsibility for the work reported and should be given the credit for it. Neither the Harvard nor the numerical system, however, rules out the possibility of mentioning by name the head of the unit ("Professor Smith's team (Jones, Brown, Robinson, *et al*, 1977) . . .") or the department ("at St Swithun's¹⁰ . . .").—ED, *BMJ*.

Standardisation and safety

SIR,—Among the benefits of standardisation, we are told, not the least is that of safety. How safe then is a new anaesthetic apparatus recently supplied to this department by a well-known manufacturer? The gauges are calibrated as follows:

Oxygen, high pressure	..	kPa × 100
Oxygen, low pressure	..	lb/in ² and kg/cm ²
nitrous oxide, low pressure	..	lb/in ² and kg/cm ²
nitrous oxide, high pressure	..	"full at varying temperatures"
vacuum		kPa and mm Hg
blood pressure	..	mm Hg
Manley ventilator, pressure	..	cm H ₂ O
Manley ventilator, tidal volume		ccs
Rotameters		cc/min
Cylinders, tested to		2000 lb/sq in (psi)
nitrous oxide		900 litres, of which 100 gallons weighs 30 oz
oxygen		1360 litres
carbon dioxide		450 litres, of which 100 l weighs 0.182 kg (6½ oz)

No sane comment seems possible, but do other specialties enjoy such variety?

K BRYN THOMAS

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Royal Berkshire Hospital,
Reading

Drug treatment card

SIR,—Experience with the use of this card in the way described in my article on the Aldeburgh record system (10 September 1977, p 683) has suggested that the division of drugs into those for maintenance therapy and those for episodic treatment is impracticable. It is better to enter all drugs in chronological order, ignoring the division between those for short- and long-term use. Each drug should carry a reference number, which can then be used on the repeat prescription card or the continuation notes if appropriate.

Any drug given for long-term use should be recorded on the flow sheet described in the article. This will make it clear whether the patient is currently on any long-term therapy and if so in what dose. In due course the design of the drug treatment card will be amended; meanwhile the present card can be used as suggested above.

Those seeking information about the record system or "starter pack" of background data cards and questionnaires should now contact

the Central Information Service for General Medical Practice, 14 Princes Gate, London SW7 1PU (*not* the Cardew Stanning Foundation as stated in the article). Those wishing to purchase cards can still do so from: F W Pawsey and Son, Dales Road, Ipswich, Suffolk.

IAN TAIT

Aldeburgh, Suffolk

Oesophagitis as a side effect of emepronium

SIR,—I read Mr S L Stanton's article on drugs acting on the bladder and urethra (17 June, p 1607) with interest but was disappointed that oesophagitis was not mentioned as a possible side effect of emepronium bromide. This adverse reaction to the drug is now well recognised and has recently been highlighted by the Committee on Safety of Medicines.¹

Two patients have recently been admitted to this hospital with severe oesophagitis causing complete dysphagia. Both had been taking emepronium 200 mg thrice daily at the time of onset of their symptoms, both had endoscopically proved mid and distal oesophagitis, and one had an ulcer at 30 cm. Whether the oesophagitis is due to direct mucosal injury by the drug or a result of increased gastro-oesophageal reflux by an anticholinergic mechanism is not certain.

Three other patients treated with emepronium in this department have developed dysphagia of such severity that treatment has had to be stopped. I would therefore suggest that this drug be used with caution.

R H HIGSON

Department of Urology,
Radcliffe Infirmary,
Oxford

¹ Committee on Safety of Medicines, *Current Problems*, No 3, 1978.

Genital infections and cervical carcinoma

SIR,—In your leading article (1 April, p 807) you discuss the significance of herpes simplex virus infections in cervical carcinoma and its prevention. Although herpesvirus has been considered carcinogenic and shown to be a common genital infection, the possible significance of other accompanying infections should be borne in mind.

In the past five years herpesvirus cultures from cervical mucus have been performed from patients admitted to our venereological clinic for vaginal infections. Thirty-seven patients with positive cultures were followed up with vaginal smears. Cytological findings were negative in 35 of the women and atypical in one. Biopsy in this atypical case produced no evidence of cervical carcinoma stage 0. These figures do not exceed what can be found in large mass examinations.¹ However, the observation time of 1-5 years is short and cytological and clinical manifestations might require a longer time to develop.

We also followed up another series of 162 patients who had been infected with neisseriae in 1954-5 and were still residing in the receiving area of the hospital. These patients were all traced in the gynaecological cytology register. Patients with positive smears had been referred for gynaecological examination