

than two-thirds of recurrences are detected before they are clinically apparent and that standard tests are of limited value.

A clinical trial has been mounted to evaluate the efficacy of imaging with his labelled monoclonal antibody in comparison with CT scanning. There was 49% overlap but the antibody was more effective in extra-hepatic disease. The trial was mainly applied to patients with rising levels of CEA. Toxicity was minimal but the HAMA response (human/mouse

reaction) was sometimes a problem. Dr Franks thought his method would be useful for initial staging, in follow-up for recurrent disease and before second-look surgery. He could not say how the cost of his method would compare with that of CT scanning however!

K Hellmann
Editorial Representative
Section of Oncology

Letters to the Editor

Preference is given to letters commenting on contributions recently published in the JRSM. They should not exceed 300 words and should be typed double-spaced.

Helicopters

Your editorial on the use of helicopters for civilian casualty evacuation was both interesting and widely informed (January 1992 *JRSM*, p 1). However, the article fails to mention the very great contribution that helicopter transport has made to the medical support of offshore oil and gas operations. It would be quite impossible to provide any sort of satisfactory service in this field were it not for the splendid work of the helicopter companies and their supporting personnel.

I have in the past offered to discuss publicly the medical support of this industry, which is so important to the UK's well-being, but my offer failed to attract the selectors' attention!

As one who first saw helicopters used for medical support 40 years ago I have no doubt of the great value of these aircraft to the medical services of the forces, the oil industry and the RNLI.

I K ANDERSON
 Medical Adviser, Phillips Petroleum
 UK Division, Phillips Quadrant,
 35 Guildford Road,
 Woking GU22 7QT

Drs Wilson and Cross (January 1992 *JRSM*, p 1) discussed helicopter usage in terms of saving lives. I have had a small part to play in the authorization of approval of a helicopter system for our regional hospital. The administration personnel of the hospital system and the director of the emergency room expressed few concerns about a helicopter saving lives. There was considerable discussion of a cost-benefit ratio in terms of how visible the hospital would be with a helicopter sitting outside and how many critical care patients could be admitted to the hospital via helicopter so that high priced, intensive care beds would sustain long-term occupancy.

Americans have pioneered the use of the helicopter in rescue missions, but we have sublimed the rationale for helicopter use for a more utilitarian purpose... money. Delighted that you chaps maintain

a sense of perspective concerning the use of helicopters in the delivery of health care.

R E LINDE

Ear, Nose and Throat Specialists of
 Northern Virginia, PC
 The Mark Building, 6231 Leesburg Pike
 Suite 500, Falls Church, VA 22044, USA

We read with interest the review of the first 50 patients of the Careflight project (January 1992 *JRSM*, p 29). Kee speculates that the difference between Careflight's eventual mortality rate of 20% compared with 38% for the land transfer system described by Reeve *et al.*¹ may be attributable to less physiological deterioration associated with helicopter travel. It has been shown that sickness scores do not deteriorate during adequately managed road transfer². Further, Ridley³ demonstrated no significant difference in outcome between patients transported to the base unit by the same land-based group and patients admitted directly.

There are significant differences between the groups of patients compared by Kee. Acute renal failure and head injuries form 31% of the case-load described by Reeve but do not feature in the Careflight group. Removing these patients from Reeve's figures reveals a mortality rate of 22% for a group of patients comparable with that of Careflight. While Kee would be correct historically in claiming benefits for sicker patients based on Bion's 1985 work², the more recent results from our centre showing a mortality of 38% in transferred patients with APACHE II scores over 18³ would refute this assumption.

Kee asserts that a land ambulance would have been unable to transfer 14 patients because of inadequate equipment, power or medical gases. The Glasgow group has not found these to be insurmountable problems in almost 2000 transfers and no transfer has been refused on these grounds.

In the Careflight study, a further 14 cases were refused (over 21% of referrals), the reasons quoted being poor weather, night-time, maintenance and hospitals unwilling to reimburse costs. Land transfer is less susceptible to such problems. When assessing the effectiveness of the Careflight system, the outcome of those patients not transferred for logistical reasons should be included.

We believe that both helicopter and land ambulance have roles in secondary transport. Before embracing the more costly alternative as routine, however,

significant clinical benefit has to be proven. The emperor's new clothes need further assessment.

C M DRYDEN

Division of Anaesthesia

J A H DAVIDSON

Western Infirmary

J R DOUGALL

Glasgow G11 6NT

P G M WALLACE

References

- 1 Reeve WG, Runcie CJ, Reidy J, Wallace PGM. Current practice in transferring critically ill patients amongst hospitals in the West of Scotland. *BMJ* 1990;300:85-7
- 2 Bion JF, Edlin SA, Ramsay G, McCabe S, Ledingham IMcA. Validation of a prognostic score in critically ill patients undergoing transport. *BMJ* 1985;291:432-4
- 3 Ridley S, Carter R. The effects of secondary transport on critically ill patients. *Anaesthesia* 1989;44:822-7

Compensation for injury - re-appraisal

May I correct a possible misunderstanding by Bolt (February 1992 *JRSM*, p 96) in assessing the paper above (p 92)? It is only partially correct that the re-appraisal is founded on the basis that the cost of reparation for damage should fall upon whoever can be proved to be at fault. Precisely because of the difficulties in proving fault, strict liability or causation, the re-appraisal concludes that the response to a damaged individual should be by society at large (through its numerous agencies) and on a criterion of individual need. The state (and not the harmed individual) would recoup part of the cost expended if it were able to prove fault against an individual who had caused the damage. This system is intended to fuse the advantages of a no-fault compensation system with the established legal principle that an individual should only be penalised and expected to pay for damage which he has caused if he can be shown to be at fault. What is suggested is a system of universal application and not limited to medical mishaps: such a system would be more fair in apportioning liability and cost than the recent introduction of strict liability for damage caused by products. Whether or not causation was shown to be a problem in the BMA survey of medical mishaps, it is undoubtedly a major difficulty in many product liability cases. The objective is to find fair solutions to the following questions:

- (1) Should anyone who is injured be cared for by society?
- (2) Should anyone be expected to pay for injuries which have not been caused by their fault?

C J S HODGES

McKenna & Co

Mitre House, 160 Aldersgate Street,
London EC1A 4DD

Mr Hodges' thoughtful reappraisal of compensation for medical (and other) accidents and his proposals for a scheme of care and redress will have wide appeal (February 1992 *JRSM*, p 92). However, I think his confidence in the ability of the adversarial forensic approach to evaluate scientific evidence as typified in the example he quotes (National Childhood Encephalopathy Survey in pertussis litigation) is misplaced. Forensic skills are not an alternative or substitute for scientific training and in my view, a Judicial Review with the professional skills of scientific advisers complementing the forensic skills of a learned judge would have been the proper - and just - way of evaluating the general question of causation in the Loveday case.

J WILSON

The Hospital for Sick Children
Great Ormond Street, London WC1N 3JH

The centre thought to control sensory and motor activity in the early history of medicine

In a fascinating review (February 1992 *JRSM*, p 102) Quin demonstrates convincingly how some aspects of the historical development of theories about which centre (heart or brain?) coordinates sensory and motor activity are difficult to reconcile with Popper's views on how scientific knowledge is acquired, but misses an opportunity to make a powerful objection to Popper's thesis. For Popper, scientific knowledge is acquired when hypotheses are refuted by conflicting evidence. Scientific knowledge, in this sense, is arrived at by deduction, not induction, and is negative, ie knowledge of what is not true. But, following an illuminating suggestion made by Papineau¹, one can argue that induction is useful in science if it generally delivers true conclusions. Part of the task of science is to determine which inductive inferences are reliable (and thus a rational guide to action). This goes a long way to explaining the prestige of science and medicine in the modern world, a prestige which ultimately depends on their manifest success in providing reliable solutions to practical problems.

P CRICHTON

Queen Mary's University Hospital
Roehampton Lane, London SW15 5NP

Reference

- 1 Papineau D. Reliabilism, induction and scepticism. *Philosophical Q* 1992;42:1-20

I was fascinated to read Quin's paper (February 1992 *JRSM*, p 102). His remarks on observation and hypothesis are of particular importance, if we are to see medicine make an orderly advance into the 21st century.

Surely, the essence of hypothesis, whether its germ lies in observation or inspiration, is that it is a creature of fantasy; its origins do not matter. Once propounded, it is up to the originator to devise tests to demonstrate its soundness; if he fails to do this, others may (and indeed should). This process of test involves the adduction of fresh evidence and the application of logic to such further observations as may seem relevant; its outcome is the categorization of the tenets of the original hypothesis as fact or fallacy. However, as our tools of observation become ever more sophisticated, so do we have to face the fact that we treasure but transient truths - the sole constant has to remain logic in our interpretation of what we find.

J K PATERSON

L'Ilot, Les Fitayes,
13640 La Roque d'Anthéron, France

The strange case of Ms Elizabeth Trevers

I enjoyed Dr Jackson's article on 'The strange case of Ms Elizabeth Trevers who was affrighted to an astonishment' (March 1992 *JRSM*, p 173). However, the second of Dr Jackson's two references is incorrect in stating that Harvey's 'Exercitatio anatomica de motu cordis et sanguinis in animalibus' was published in 1618. It was not published until 1628.

Nevertheless, in a restricted sense, Dr Jackson may be correct in saying that Harvey's work on blood circulation 'came out' in 1618. For Keynes says, in one place¹, that Harvey had already been promulgating his views about the circulation of the blood in his