

British hospitals used to have a useful definition of cardiac arrest which specifically excluded expected death. On the noticeboards which illustrated CPR techniques in the 1970s, the heading read 'Cardiac arrest is defined as the sudden cessation of cardiac or respiratory output in *someone who is not expected to die*'. This then raises the crucial question of who is responsible for deciding whether the death is expected, and how that is to be decided.

We are tussling with this issue in Canada and have proposed to amend DNR regulations to include four criteria defining an expected death. The criteria are:

- the patient has a diagnosed condition which is irreversible and fatal;
- no active treatment against the disease process itself is being administered or planned, and all therapy is palliative;
- the patient's death is expected in the near future (up to a few weeks);
- the patient's condition is recognised as one in which CPR is clinically ineffective, inappropriate and/or could only prolong the dying process.

The physician is obliged to hold a sensitive and careful dialogue with the patient and, with terminally ill patients, is under an obligation to explain that the treatment goals are palliative and that no further active treatment against the disease is planned. However, if all of the above criteria are satisfied, then the patient's death is an expected one and a DNR order may be written without the patient's express consent. If however, in the course of the conversation, it transpires that the patient had been expecting CPR, the physician is under an obligation to explain the situation but still does not require the patient's formal consent. If all four criteria are *not* met, then the physician is obliged to ask the consent of the patient before a DNR order may be written.

Some form of policy structure or of recommended guidelines in Britain will probably increase patient-doctor trust, despite initial awkwardness and difficulty in holding 'treatment objectives' conversations with the patient, but I would strongly recommend that some definition of expected death be incorporated in any future guidelines. Without it, Britain will face the same difficulties that we are now addressing in Canada.

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References

- 1 Buckman RB, Senn J. Eligibility of CPR: is every death a cardiac arrest? *Can Med Assoc J* 1989;140:1068-9.
- 2 Blackhall LJ. Must we always use CPR? *N Engl J Med* 1987;317:1281-5.
- 3 American College of Physicians. Standards and guidelines for cardiopulmonary resuscitation and emergency cardiac care. *JAMA* 1980;244:506.

Completing 'interim discharge letter'

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Sir—Dr Fairclough and colleague's letter reported that only 16% of their discharge letters sent to GPs were complete, and highlighted the problem of achieving high quality documentation (April 1992, pages 169-71 and July 1992, page 338). We therefore undertook an audit of the last one hundred general medical 'interim discharge letters' using the fourth copy of the form: 99% had details of the diagnosis and 92% included the outpatient follow-up plans. This is a sustained improvement in quality and content of the discharge letter since its introduction 18 months ago.

We recognise though, that maintaining an improvement requires continued effort and audit cycles; at present only 9% of the forms specified whether the letter had been sent by post or was given to the patient. A significant advantage of the new structured discharge letter is that completion can be readily monitored particularly in specialties where there is a high turnover of junior staff.

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The Brown Kelly-Paterson *not* Plummer-Vinson Syndrome

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Sir—I am delighted that my little paper (*Journal*, July 91, p257) should have stimulated a correspondence. Its purpose was to establish the priority of Brown Kelly and Paterson—or Paterson-Brown Kelly if one is from Cardiff—in describing in 1919 the syndrome of dysphagia (due to upper oesophageal spasm or web), glossitis and anaemia, and to point out the entirely mistaken attribution to Plummer and Vinson.

Dr Logan (*Journal*, April 92, p241) rightly draws attention to the late 18th and early 19th century writings of Baillie, Home, Monro (*tertius*) and Syme. However, their reports, perceptive as they are, are confined to dysphagia due to upper oesophageal stricture. There is no mention of glossitis or anaemia, associations which together provide the characteristic combination of features of the syndrome under discussion.

Dr Baron (*Journal*, October 91, p361), while first agreeing completely with my conclusions, then goes on to question the very existence of the syndrome. He presents evidence from two studies [1,2] that there is no association between cervical dysphagia—with or without an oesophageal web—and iron deficiency. The first [1] curiously overlooks its own documentation of lower serum iron levels in persons with webs compared to other dysphagic subjects. It also seems not to have persuaded all its authors, for in two other papers published around the same time [3,4], the association between iron deficiency and cervical dysphagia—whether cause or effect—is clearly enunciated.