ing the dataset specification; training and continuing support of those who collect data; and the development and testing of in house database and reporting systems.

To date, 108 units have been recruited—that is, 46% of the units in England and Wales.⁵ With a remit to be self financing within two years, the audit is run on a cost recovery basis and makes a small annual charge to each participating unit. It should be noted that the extraction, collection, and entry of accurate data require resources for staff, software, and hardware. The total cost, however, is barely 1% of the annual budget for an average intensive care unit.

The centre was established as a charitable company separate from the society so that it could provide independent and objective audit and research into intensive care. As well as providing feedback to units for audit purposes, the resultant database will be a valuable resource for research. Through the centre's work with the Cochrane Collaboration, identifying and systematically reviewing trials relevant to intensive care, we hope to contribute to the scientific evidence on which future practice in intensive care should be based.

Without a role model to follow, we have learnt several lessons along the way—for example, how to deal with confidentiality and the ownership of data. If the orthopaedic surgeons take up Sochart and colleagues' call we would be happy to share our experiences.

KATHY ROWAN
Director

Intensive Care National Audit and Research Centre,
London WC1H 9HR

1 Sochart DH, Long AJ, Porter ML. Joint responsibility: the need for a national arthroplasty register. *BMJ* 1996;313:66-7. (13 July.)

2 Rowan KM, Kerr JH, Major E, McPherson K, Short A, Vessey MP. Intensive Care Society's APACHE II study in Britain and Ireland. I. Variations in case mix of adult admissions to general intensive care units and impact on outcome. *BMJ* 1993;307:972-7.

3 Intensive Care National Audit and Research Centre. *ICNARC: an introduction*. London: ICNARC, 1994.

4 Intensive Care National Audit and Research Centre. *Case mix programme dataset specification (ICMPDS)*. London: ICNARC, 1995.

5 Metcalfe A, McPherson K. *Study of provision of intensive care in England, 1993. Revised report*. London: Department of Health, 1995.

Current evaluations of information technology in health care are often inadequate

EDITOR,—Two recent articles debated the value of information technology to the NHS and called for further evidence from evaluation studies to inform future investment in information technology.^{1,2} These articles draw attention to both the scarcity of evaluation studies of information technology in health care and the lack of scientific rigour of such studies. They reflect current opinion, which strongly advocates the use of economic analyses and randomised controlled trials.

While we agree with the need to provide evidence of the benefits of information technology in health care, we believe that current thinking and practice concerning evaluation are short sighted and detrimental to long term progress. By concentrating on economic analyses of information technology (the results of which have not been encouraging), current evaluation methods overlook potential benefits, such as improved quality of care through better access to more reliable information. By insisting on evidence from randomised controlled trials we waste precious resources on evaluation work that is methodologically flawed and impractical and at best provides results that are difficult or impossible to generalise. An additional problem is that of timing. Many of the benefits of information technol-

ogy cannot be expected to become evident until several years or more after its introduction, yet we call for evidence of immediate benefits.

In the current climate, in which information technology is viewed suspiciously by many people, it may be argued that evaluation is implicitly used as a barrier to progress. We have inadvertently created a catch 22 situation whereby we cannot move forward with information technology in health care because of the lack of evaluation, yet our failure to deploy systems in routine clinical practice and allow them to mature means that we have nothing of any substance to evaluate.

Information technology is an abstract phenomenon that can be moulded into many forms, often rapidly. It does not operate in a vacuum but is embedded within a complex social and organisational context. The priorities, therefore, should be to develop richer understanding of the effects of its benefits in health care and to develop new evaluation methods that help us to understand the process of implementing it.

Negative results from current inadequate evaluations should not be used to impede general research and development. There is a pressing need to build foundations of fundamental research and development in information technology in health care.

HEATHER A HEATHFIELD
Senior lecturer

Department of Computing,
Manchester Metropolitan University,
Manchester M1 5GD

IAIN E BUCHAN
Research fellow

Medical Informatics Unit,
Institute of Public Health,
University of Cambridge,
Cambridge CB2 2SR

1 Lock C. What value do computers provide to NHS hospitals? *BMJ* 1996;312:1407-10. (1 June.)

2 Donaldson LJ. From black bag to black box: will computers improve the NHS? *BMJ* 1996;312:1371-2. (1 June.)

Placebo mania

Placebos are essential when extent and variability of placebo response are unknown

EDITOR,—We need placebo controlled trials¹ because responses to placebo are remarkably variable. Figure 1 shows the results of randomised double blind trials of anticonvulsants,² antidepressants,³ and topical capsaicin⁴ in painful diabetic neuropathy. The proportion of patients in whom the active drug relieved at least 50% of the pain is plotted against the proportion in whom placebo relieved at least 50% of the pain. The active treatments achieved this degree of pain relief in 50-90% of the patients, while placebo did so in 0-80%.

This variation has no simple cause related to the design of the study or to measurement, nor is such variation rare. Trials in other chronic pain conditions showed a placebo response varying from 7% to 86%² and from 4% to 47%.³ In trials of antiemetics 0-50% of patients given a placebo vomited soon after general surgery,⁵ and after correction of squint the range of the placebo response was 18% to 88%.⁶ In trials of surfactant in the respiratory distress syndrome the range of the placebo response was 24% to 69%.⁷

B Jones and colleagues emphasise the need to know that "both treatments were indeed effective" in a trial of two active drugs.⁸ Only if we know the extent of the placebo response and that it does not vary can this criterion be fulfilled. Performing studies of drug A versus drug B when the extent and variability of the placebo response are unknown can be misleading. As Jones and colleagues point out, the results could mean that both A and B were effective or that neither A nor B was effective. The only current defence is to have a

Fig 1—L'Abbé plot of proportion of patients in whom active drug relieved at least 50% of pain against proportion in whom placebo relieved at least 50% of pain in randomised controlled trials of various drugs in diabetic neuropathy

placebo group. Designs of trials of analgesics 30 years ago understood this and included both standard active and placebo groups.⁹ Only in trials in which sensitivity is proved (that is, standard treatment beats placebo) can correct conclusions of equivalence be made.

If we already know the answer then it is unethical to include a placebo group: the condition of equipoise is not met. If we do not know the answer, and in particular if we do not know the extent and variability of the placebo response, then it is unethical to enrol patients into trials whose design precludes a sensible answer. In these circumstances a placebo group is essential and ethically acceptable.

HENRY MCQUAY
Clinical reader in pain relief
ANDREW MOORE
Consultant biochemist

Pain Research and Nuffield Department of Anaesthetics,
University of Oxford,
Oxford Radcliffe Hospital,
Churchill Headington,
Oxford OX3 7LJ

- Rothman KJ. Placebo mania. *BMJ* 1996;313:3-4. (6 July.)
- McQuay H, Carroll D, Jadad AR, Wiffen P, Moore A. Anticonvulsant drugs for management of pain: a systematic review. *BMJ* 1995;311:1047-52.
- McQuay H, Nye BA, Carroll D, Wiffen PJ, Tramer M, Moore RA. A systematic review of antidepressants in neuropathic pain. *Pain* (in press).
- Zhang WY, Li Wan Po A. The effectiveness of topically applied capsaicin. A meta-analysis. *Eur J Clin Pharmacol* 1994;46:517-22.
- Tramer M, Moore RA, McQuay H. Omitting nitrous oxide in general anaesthesia. Meta-analysis of intraoperative awareness and postoperative emesis in randomised controlled trials. *Br J Anaesth* 1996;76:186-93.
- Tramer M, Moore A, McQuay H. Prevention of vomiting after paediatric strabismus surgery: a systematic review using the numbers-needed-to-treat method. *Br J Anaesth* 1995;75:556-61.
- Soll JC, McQueen MC. Respiratory distress syndrome. In: Sinclair JC, Bracken ME, eds. *Effective care of the newborn infant*. Oxford: Oxford University Press, 1992:333.
- Jones B, Jarvis P, Lewis JA, Ebbutt AF. Trials to assess equivalence: the importance of rigorous methods. *BMJ* 1996;313:36-9. (6 July.)
- Houde R, Wallenstein S, Beaver W. Clinical measurement of pain. In: De Stevens G, ed. *Analgesics*. New York: Academic Press, 1965:75-122.

Placebo controlled trials are needed to provide data on effectiveness of active treatment

EDITOR,—Kenneth J Rothman questions whether there is any point in using a placebo group if blind assessment can be achieved in a comparative trial of two active treatments.¹ He minimises both the practical advantages of placebo controlled trials and their advantages for hypothesis testing. The limitations of statistical