Rotterdam symptom checklist, the instrumental help for daily living scale, the activities of daily living scale, and the social experiences scale. Satisfaction was assessed with the Rand corporation's patient satisfaction questionnaire.

Our trial failed to attain the intended sample size too and we stopped the intake of patients. McWhinney and colleagues reported problems with patient recruitment, attrition, and the exposure of the control group to services equivalent to the home support team. In our case the major problem was patient accrual. As it happened, the number of eligible patients was nearly half that of previous years. To improve patient intake we invited on several occasions 11 associated centres to refer their patients. During two years only 22 patients entered the trial, of whom nine came from associated centres. Furthermore, 22 eligible patients refused to participate in the trial. This was suspected early on in the trial, and the reason for refusal and the characteristics of these patients were registered. Eight patients did not want chemotherapy, 10 refused either continuous infusion or randomisation, and four were not asked by the physician because of medical reasons. In contrast with McWhinney and colleagues' experiences, attrition did not have a major role—four patients died before the first evaluation at two months, and all other patients completed the questionnaires. A possible explanation for this difference could be that the patients in McWhinney and colleagues' trial had a more advanced disease than our patients.

Despite inclusion criteria allowing only patients with disseminated colorectal cancer, without metastasis to the brain, and with normal results of haematology tests, with normal liver and kidney function, World Health Organisation performance status of 2 or more, and no serious comorbidity to participate, our study population turned out to be rather heterogeneous. Intersubject variability of the measures of quality of life was great. Individual plots of quality of life scores over time seemed stable when the clinical situation was stable and seemed to decline around the time of disease progression. No apparent relations between quality of life and satisfaction were found. Small sample size and great intersubject variability do not allow for further statistical analysis.

In conclusion, randomised trials in palliative cancer care are difficult, but not impractical. Our trial failed because of difficulties in patient accrual, but other common problems, such as attrition, contamination of exposure, or selection of adequate outcome measures did not have a major role. Future randomised trials in palliative cancer care should consider multicentre participation and the possibility of patient entry without limitations to type of primary cancer. Moreover, the care intervention should be well defined, and adequate outcome variables should be selected.

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Interpreting hospital death rates

Can be difficult

EDITOR,—We agree that hospital death rates for myocardial infarction can be difficult to interpret.¹ In a guide to hospital statistics and their interpretation we reported hospital death rates in Oxford, which ranged from 8% (27/341) to 73% (73/100).² The hospitals were anonymised because our code of practice is not to identify individual hospitals in publications.² The hospital with a death rate of 8% admitted only convalescent medical patients: its statistics were selectively based on survivors. The hospital with a death rate of 73% was a geriatric hospital: many of its patients were admitted, with conditions other than myocardial infarction, for long stay care. Those whose deaths were certified as due to myocardial infarction contributed not only to the numerator but also to the denominator because they had a fatal infarct while in hospital.

Circumstances such as these should be easy to identify locally, but a more important problem is that of defining a hospital admission when patients with myocardial infarction die close to the time of admission. There may be considerable variation among hospitals in whether such patients have been formally admitted and therefore included in the hospital's statistics.² We also know currently of a hospital computer system in which, when the patient's disposal is unknown to the coder, the default option is that the patient is coded as alive. A useful check in studies of hospital death rates is to identify death certificates that specify that the patient died in hospital and see whether a corresponding hospital record abstract exists, and vice versa. Although problems like these arise, however, they should be addressed in the analysis and interpretation of hospital statistics rather than be regarded as reasons for not auditing hospital care.

England's problem has been not so much the lack of means to link records but the incompleteness of its data since the Körner changes. For example, in the Oxford region from 1979 to 1986 (when collection of data for hospital statistics was managed by the Oxford record linkage study) 97% of hospital activity analysis records (1 557 753/1 609 588) included diagnostic data. From 1987 to 1990 the figure dropped to 79% (1 095 711/1 383 973), and it has only gradually crept back towards earlier levels of completeness. The completeness of clinical data in the national hospital dataset in England fell from the mid-1980s, and the hospital inpatient enquiry and the mental health enquiry—previously rich sources of information—have not been available since 1985.

Mortality after care in hospital can be studied in England.^{3,4} Statistical information about hospital episodes, including data needed to link records, exists in patient administration systems in most hospitals. Death certificates can be made available locally for linkage: copies are held by district departments of public health. Software to match records for linkage has been developed to run on microcomputers, and copies of it can be made available to anyone in the NHS in England.⁵

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Report was intended to promote discussion

EDITOR,—The stated intention of the Clinical Resources and Audit Group in publishing its report on clinical outcomes is to promote discussion among clinicians to determine what lessons can be learnt to improve the treatment of patients—a worthy and proper aim.¹ Contrary to the group's advice, however, the data provided have been put into league tables. This has caused considerable and quite unnecessary anxiety to people living in Fife. Had the opportunity been afforded to trusts to comment on a draft of the document before publication this anxiety would have been avoided by the correction of a simple error—that is, the inclusion of elderly patients in the figures for myocardial infarction. Furthermore, the appropriate and more satisfactory figure would have been attributed to the appropriate trust—namely, the Queen Margaret Hospital Acute NHS Trust.

When it was first suggested that these tables should be published medical directors in Scotland expressed considerable disquiet, being concerned that wrong messages would be given to the public and that the primary purpose, of promoting open discussion among clinical staff, would be undermined. This view is now well justified. The expectation is that purchasers will use the information in the report to influence the contracting process. Therefore if the public is not to be caused unnecessary anxiety, and if the document is to have credibility for clinicians and in the contracting process, basic errors must be eliminated before any further publication.

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Asthma trends

EDITOR,—Helen Booth and colleagues suggest that differences in the output of DeVilbiss No 40 and No 45 nebulisers¹ may have influenced our estimates of the increase in the prevalence of airway hyperresponsiveness in children studied 10 years apart, in 1982 and 1992.² We expressed airway responsiveness both as a dose-response ratio (defined as the percentage fall in forced expiratory volume in one second divided by the dose administered) and as airway hyperresponsiveness (defined as a 20% fall in forced expiratory volume in one second at a dose of 3.92 μmol histamine). As reported, we normalised the dose-response ratios measured in 1992 to take account of methodological differences in collecting the data in 1982 and 1992 and so that we could more accurately compare the dose-response ratios measured in 1982 with those measured in 1992. To normalise the 1982 values we added a further constant to the dose-response ratio so that the cumulative frequency response curve for normal subjects studied in 1992 matched that for normal subjects studied in 1982. This adjusted for systematic methodological changes between the years, including nebuliser outputs, brand of spirometer,

and other details of the protocol. The effect of this adjustment was to reduce our estimated increase in mean dose-response ratio between 1982 and 1992.

Until Booth and colleagues publish the details of their methods and their data, and until clinical data on the practical effects of differences between nebulisers are available, we suggest that our method of adjusting for methodological differences on the basis of our own epidemiological data was the most accurate adjustment that could be made. Using that adjustment, we found that the level of airway responsiveness had increased between 1982 and 1992 in both study regions. This finding, which supported our conclusions that the children studied in 1992 had a higher prevalence of airway hyperresponsiveness than the children studied in 1982, is consistent with other Australian studies that have reported a significant increase in the prevalence and severity of asthma.^{3,4}

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Smoking and death

EDITOR,—It is interesting to contrast the survival curves for the British male doctors studied by Doll and colleagues¹ with what is, to my knowledge, the first published lifetable for smokers and non-smokers. This was computed by Pearl in 1938 in an "investigation of the influence of tobacco upon human longevity" in American men.² The figure shows survival in the British (fig (a) and (b)) and American (fig (c)) studies.

In the British study the population is homogeneous in occupation and social class and is large (34 439 subjects); qualitative and quantitative information is available on smoking habits; and the population has been followed up for 40 years, longer than any other cohort. In the American study the population was heterogeneous in occupation and social class and was comparatively small (6813 subjects); smoking habits were more broadly defined (non-users of tobacco, 2094; moderate smokers, 2814; heavy smokers, 1905); and the population was observed over a restricted period. Still there is a close resemblance between the differences in survival between smokers and non-smokers in the two populations, particularly when

the earlier period (1951-71 (figure (a)) is considered for the British doctors.

Although the average (median) age of survival for subjects older than 35 is nearly 79 for the British doctors and only 69 for the American men, reflecting the differences between the two populations, the studies, and the epochs of observation, the loss of survival for smokers compared with non-smokers (averaging over moderate and heavy smokers in the American data) is five years for the British doctors (figure (a)) and 4.9 years for the American men.

There may be an element of chance in such nearly exact concordance, but it is unlikely to be a major one: Pearl stated that "the smoking of tobacco was statistically associated with an impairment of life duration, and the amount or degree of this impairment increased as the habitual amount of smoking increased." We know today that such an impairment—which has increased in recent times with increasing survival of non-smokers (figure (b))—arises from the specific diseases and deaths caused by smoking. Coincidentally, Pearl's remark is the type of remark that Richard Peto makes in his letter on the nature of the evidence available on the issue of smoking and health in the late 1930s.³

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Care of dying patients in hospital

EDITOR,—Nicholas Albery is shocked by the way in which Mina Mills appeared to neglect the needs of dying patients while she was a non-participant observer researching the care of patients dying in hospital and regrets the delay in publishing the research.¹ Mills and colleagues explain that the delay occurred partly because of antipathy to the findings.² This turning of a blind eye, albeit to the unbearable, underlines the need for the research.

There was a similar reluctance 50 years ago when Bowlby, Spence, Winnicott, MacCarthy, and a few others tried to develop the rational and humane care of children in hospital; the need for unrestricted visiting and, especially, for mothers or fathers to say in hospital with their young children was recognised. In despair, in 1953 James Robertson made his film *A Two Year Old Goes to Hospital*—a film using non-participant observation. Because the film was so upsetting Robertson experienced similar antipathy to that experienced by Mills and colleagues. Some doctors suggested that the film was of a specially selected disturbed child, and one thought that the film was a fake. This film was, and remains, influential.

Had these doctors been able to allow themselves to notice the misery of the young children in their own wards—to be participant observers—Robertson need not have made the film.

Participant and non-participant observation of miserable and neglected patients is hard and requires training. Mills should be congratulated. People need help to cope with the complex feelings of helplessness, despair, shame, and guilt stirred up by caring for those who are sick.³ The help needs to be continuing and is probably best provided in small group discussions led by experienced trained staff. Maybe another film with non-participant observation needs to be made.

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New or old antidepressants?

EDITOR,—In their article on the new antidepressants Harrison and Owens mention that "certain depressed patients may require a broader spectrum of synaptic blockade than that offered by the selective serotonin reuptake inhibitors."¹ Surely the idea is to enhance transmission at serotonergic synapses by presynaptic reuptake blockade, which is rather different from synaptic blockade.

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Author's reply

EDITOR,—It is now clear that there is not a single 5-hydroxytryptamine or noradrenaline receptor but a family of receptors subserving different functions at presynaptic and postsynaptic sites. Different antidepressants seem to have both sensitising and desensitising effects on postsynaptic 5-hydroxytryptamine receptors, and evidence exists of adaptive reduction in receptor binding sites after long term administration.

Because of the evidence of agonistic and antagonistic effects associated with different antidepressants, at both the presynaptic and postsynaptic levels, the term receptor regulation would have been more comprehensive than blockade. The point is that we shall continue to need a range of old and new antidepressants to meet the needs of our patients.

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Breast disease

EDITOR,—Justine Foster advocates aspiration of breast cysts by general practitioners in their surgery because, in her experience, "the first thing a surgeon would do is attempt to aspirate the cyst."¹ While we accept that it is appropriate for general practitioners to aspirate cysts of patients known to have recurrent cystic disease, we do not believe that this is appropriate for patients who

Survival of (a) British doctors 1951-71 and (b) British doctors 1951-91 from Doll and colleagues' study and (c) of American men in 1930s. Figure (c) is adapted from Pearl² with permission of "Science" (copyright 1995 American Association for the Advancement of Science)