Letters

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Satisfaction with nurse specialists in breast care clinics

Nurse led clinics may actually cost more

EDITOR—Garvican et al conclude that since the results of the fine needle aspiration obtained by clinical nurse specialists in their breast clinic were better than those obtained by other clinicians the nurses' clinical expertise compared favourably with that of other clinicians.[1] In a recent four month survey of fine needle aspiration cytology in our unit, 86 (20%) of 432 aspirates were classed as inadequate (C1). This is significantly lower than the rate of 276 (33.5%) of 825 samples ($P < 0.0001$) classed as inadequate in Garvican et al's paper. Two thirds of the aspirations of palpable lesions in Edinburgh were performed by consultants; their rate of inadequate samples was 38/233 (16%). This is significantly lower than the 114 (32%) of 362 samples taken by the nurses that were classed as inadequate ($P < 0.0001$). The nurse specialists performed 362 (44%) of 825 aspirations in their clinic; in Edinburgh non-consultant clinicians performed 35 (28%) of 124 ($P = 0.012$). These results confirm that the experience of the clinician performing an aspiration is an important factor in the success of the technique.[2]

In the breast clinic the ratio of benign samples to malignant samples was 5.1:1, while in Edinburgh it was significantly lower at 1.2:1 ($P < 0.0001$). In Edinburgh all patients are seen by experienced consultant breast surgeons or senior doctors, and immediate access to mammography and ultrasonography is available during the clinic. This may explain the apparently better selection of patients for aspiration cytology, which is the most painful test performed in breast clinics.[3]

We recently introduced a "one stop" clinic in which women have immediate access to breast imaging and to the results of imaging and aspiration. We assessed the satisfaction of patients who required ultrasound imaging or aspiration with a questionnaire which was completed before leaving the clinic. Before the introduction of the new service only 50/125 (40%) women were completely satisfied with their visits; the most common complaint was about the delay in receiving test results. After the introduction of the one stop clinic, 80/114 (70%) indicated that they were completely satisfied. Reasons for a lack of complete satisfaction with the new service were all non-medical and included problems with car parking (24/114, 21%) and poor signposting to the clinic (11/114, 10%). Difficulties in parking increased patients' anxiety, and anxiety relates to the pain experienced during investigations.[3] In comparison with the breast clinic described by Garvican et al we perform fewer fine needle aspirations in cases of benign disease and obtain fewer inadequate samples. Far from cost benefits accruing from nurse led clinics there may be cost implications because of the extra aspirations performed and the higher rates of obtaining inadequate samples.

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Nurses are not as effective as consultants

EDITOR—Garvican et al claim that patients attending a nurse led clinic screening for breast diseases were satisfied with the care they received.[1] Unfortunately these women did not have the opportunity to make a meaningful comparison between clinics run by nurse specialists and standard outpatient care; the women did not experience care in both types of clinics. A surgeon saw women who had been diagnosed with cancer, who were likely to be disappointed with the results of their tests, and therefore less satisfied. Thus, it is not surprising that the women seen by the nurses, all of whom had results that were classed as benign, were satisfied. We need to know the satisfaction rate when patients with cancer are given results by the nurse as compared with the clinician.

Dixon has shown that a dedicated cytologist in a breast clinic can achieve a high rate (99%) of adequate cytology samples.[2] Garvican et al report that the nurses' technical expertise in performing fine needle aspiration was as good as that of clinicians. Although this is apparently true it neglects the fact that guidelines published by the British Association of Surgical Oncology state that <20% of cytological specimens should be inadequate.[3] In this clinic 276 (33.4%) of 825 of samples were inadequate; for some senior clinicians 38/66 (57%) samples were inadequate. The critical issue of the adequacy of specimens aspirated from women with cancer is not addressed.

The guidelines also provide quality objectives to be met and outcomes to be measured.[3] According to the guidelines, all new patients presenting to a breast clinic should be seen by a consultant or a higher surgical trainee. Less than 10% of all new patients should be required to attend more than twice for diagnostic purposes. The high rate of inadequate samples obtained in this clinic almost certainly means that these criteria have not been met. The authors have provided no evidence to support their claim that a clinical nurse specialist can provide adequate outpatient care (in the absence of a second consultant) in terms of the sensitivity or specificity of the detection of cancers. In view of the rate of inadequate samples in this clinic one would be concerned that cancers have been missed.

To conclude that nurses can adequately provide outpatient care in the absence of a second consultant is inappropriate without evidence that the clinic is capable of meeting the quality standards set out in the guidelines.

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Authors' reply

EDITOR—Unfortunately, it was not possible to provide full details of our study in a short report. Our sample did, however, include women who had been diagnosed with cancer. We also indicated that a trial would be required before claims of cost effectiveness could be made and, as suggested by Dixon et al, this clearly should include the costs of cytology. This would minimise the tendency to make spurious comparisons of performance using statistical techniques.[1]

This correspondence highlights the difficulties of implementing the British Association of Surgical Oncology guidelines in the real world.[2] It is inevitable that breast units will concentrate on achieving the targets they

The quality of clinical guidelines according to the St George's appraisal instrument*

Guideline	Type[5] of guideline	Dimension score: Rigour of development	Context and content	Application
Breast cancer				
1	Local	54.0	58.3	83.3
2	Local	16.7	36.1	4.2
3	Local	50.8	48.6	25.0
4	Local	35.0	61.1	37.5
5	Local	44.2	51.4	20.8
6	Local	17.5	34.7	4.2
7	Local	43.0	63.9	41.7
8	Local	32.5	48.6	40.0
9	Local	25.8	38.9	8.3
10	Local	30.8	59.7	29.2
11	Local	23.0	45.8	20.8
12	Local	22.5	12.5	12.5
13†	National	50.8	68.1	76.7
14	National	52.0	58.3	23.3
15	Local	5.8	36.1	16.7
Mean (95% CI)		33.6 (25.3 to 42.0)	48.2 (40.0 to 56.2)	29.6 (16.5 to 42.7)
Other (n=15)‡				
Mean (95% CI)		64.7 (60.0 to 69.4)	66.6 (59.2 to 74.0)	32 (21.0 to 43.0)

*The critical appraisal instrument has three dimensions: rigour of development (20 attributes necessary to validity, reproducibility, the disciplinary process, and schedule review); context and content (12 items addressing reliability, applicability, flexibility, and clarity); and application (five items addressing dissemination and monitoring strategies). Each guideline is given a standardised score ranging from 0 to 100; 100 indicates that all reviewers considered that a guideline fulfilled all attributes within that dimension. Further information is available from: http://www.sghms.ac.uk/phs/hceu/index.htm.
†British Association of Surgical Oncology guidelines.
‡Assessed on behalf of the NHS Executive for commendation to the NHS (personal communication, F Cluzeau).

consider important at the expense of others. Our unit chose to focus on triple assessment by experts (albeit non-medical) and on minimising diagnostic delays. In the units described by Dixon et al and Bramley et al, patients are preselected for triple assessment, and it is therefore inevitable that there will be lower rates of benign or inadequate samples. This may be justified to avoid unnecessary pain but carries a risk of missed cancers, especially in younger women who have had inconclusive results on imaging or clinical examination. There is "fairly strong evidence that triple assessment increases the accuracy and reduces the overall cost of diagnosis when compared with selective use of component tests."[3]

This raises the question of the validity of the British Association of Surgical Oncology guidelines. Our unit has undertaken extensive research with the aim of developing a valid and reliable means of assessing guideline quality.[4] All national guidelines and a random sample of local guidelines on the management of asthma, breast cancer, coronary artery disease, and depression were critically appraised by six independent reviewers.[5] While the British Association of Surgical Oncology guidelines certainly scored higher on attributes associated with quality than locally developed guidelines, they compared less well with the standard now expected of national guidelines (table). In the first dimension of the appraisal, reviewers assess whether those who produced the guidelines have been rigorous in utilising underlying research and minimising bias. Only half of the quality attributes were met by the surgical oncology guidelines. These guidelines were, however, clear and had sought to address the issue of implementation.

There has been little research into the practical aspects of diagnosing breast cancer. For too long management has been based on the opinions of individuals. Well designed studies are urgently needed to provide firm evidence on which to base guidelines. Only then will it be worth auditing quality objectives against outcomes.

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Resolution of peanut allergy

Patients have not been proved to grow out of peanut allergy

Editor—Histories of allergy are known to be unreliable, and this is particularly the case for parents' reports of a child's reactions to foods. When double blind food challenges were used, parents' reports could be confirmed in only 37 (28%) of 133 children with reported food intolerance; in another study only 27 (33%) of 81 reports of food intolerance in children could be confirmed.[1 2]

Hourihane et al performed an open food challenge in children with a history of possible peanut allergy.[3] Serious doubt that some of these children had genuine peanut allergy arose either because the result of a skin test was negative (this is rare in subjects who have allergic reactions to peanut) or because the child was reported to have eaten peanut without problems (which suggested that peanut allergy was not present). When challenged, some patients had no reaction. The authors concluded, reasonably, that one should be prepared to challenge preschool children with reported peanut allergy because some of them will turn out to be tolerant.

The unresolved question is whether children who fail to react to a challenge ever had peanut allergy in the first place. This paper contains no proof that those with "resolving" peanut allergy ever had peanut allergy, so caution is needed about the suggestion that some patients with peanut allergy grow out of the problem. Close examination of other claims of patients growing out of peanut allergy casts doubt on the original diagnosis. To prove that a patient has grown out of a food allergy one has to prove that he or she had the allergy in the first place.

Some of the "resolvers" in Hourihane et al's study had had up to seven reported reactions to peanut, which suggests that they really did have peanut allergy. In my experience, however, it is remarkable how "definite" food allergy can evaporate once it is exposed to the test of a proper food challenge.

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1 Bock SA. Prospective appraisal of complaints of adverse reactions to foods in children during the first 3 years of life. *Pediatrics* 1987;79:683-8.
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Authors' reply

Editor—We agree with David's statement about the rarity of peanut allergy in a person with a negative result of a skinprick test to peanut. Since our paper was published one of us (SAR) has identified a child in whom a challenge gave a negative result after having given a positive one previously. In a previous study of adult subjects we showed that a history of peanut allergy was 100% sensitive and 86% specific (60/69 histories supported by positive challenge).[1] Similarly, a positive result of a skinprick test to peanut was 100% sensitive and 96% specific (2/62 subjects with a positive result were negative to peanut on challenge).

David's "unresolved question" is discussed at length in our paper. We stated that our results suggest with some caution that

some children grow out of peanut allergy, and we accept that absolute proof of resolution is absent.

Allergic reactions to peanut are usually stereotyped, and the absence of typical features predicts absence of peanut allergy. Our patients were reported to have typical features of peanut allergy. We could find no other explanation for their symptoms. Some had negative results to skinprick tests, and after food challenge testing we concluded that clinical reactivity was absent. Other children with positive results to skinprick tests were found to be negative on challenge. Again no historical feature distinguished them from controls with persisting peanut allergy. The size of the response to the skinprick test distinguished the groups.

If David's main point is that no child should be diagnosed as allergic to peanuts in the absence of a positive result on challenge testing he is wrong. Many children are too severely affected for challenge to be justifiable. Our experience suggests, however, that there is another group of young children who, in all good faith, have been treated as allergic to peanuts but do not have persisting disease. To identify such children without a challenge is impossible. The opportunity should not be lost to remove the severe anxiety that families have because of suspected peanut allergy. We attempted to identify historical features that may help paediatricians "flag" these children for challenge.

We deplore the paucity of nationwide facilities for the appropriate management of children with acute and life threatening allergies; unfortunately this means that the wait for a food challenge test is unacceptably long.

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Odds ratios should be avoided when events are common

Editor—A news item stated that "a review article written by authors with affiliations to the tobacco industry is 88 times more likely to conclude that passive smoking is not harmful than if the review was written by authors with no connection to the tobacco industry."[1] We are concerned that readers may have interpreted this huge effect at face value. The proportions being compared (which were not given in the news item) were 29/31 (94%) and 10/75 (13%). The relative risk here is 7, which indicates a strong association but is an order of magnitude lower than the reported odds ratio of 88.[2] This value is correct but is seriously misleading if presented or interpreted as meaning that the relative risk that affiliated authors would draw favourable conclusions was 88, as it was in this news item.

The odds ratio is valuable in case-control studies where events are usually rare and the relative risk cannot validly be estimated directly. In prospective studies interpretation of the odds ratio as an approximation to the relative risk becomes unreliable when events are common, and thus its use for prospective studies, especially randomised trials and systematic reviews, has been criticised.[3 4] The distortion is especially large when the event rate is high in only one group, as in this example. The odds ratio should not be interpreted as an approximate relative risk unless the events are rare in both groups (say, less than 20-30%).

The odds ratio remains especially useful when researchers need to adjust for other variables, for which logistic regression is the usual approach. While such analyses are valid, when the objective is to communicate study results to an audience unfamiliar with the relation between odds ratios and relative risks, surely it makes no sense also to report the relative risk when this differs markedly from the odds ratio.

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1 Wise J. Links to tobacco industry influences review conclusions. *BMJ* 1998;316:1554. (23 May.)
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Aspirin prophylaxis for vascular disease

Knowledge needs to be used in clinical situations

Editor—We read with interest the recent articles from the North of England evidence based guideline development project.[1 2] We represent the Birmingham Clinical Effectiveness Group and admire the rigour with which these guidelines have been developed. The effort required to synthesise this amount of evidence cannot be underestimated, but has the effort been worth while?

None of the guidelines provides new evidence, and, given the basic requirement for evidence based medicine to answer a clinically relevant question, their value is questionable. For example, it is well known that aspirin after myocardial infarction is beneficial.[3] The challenge lies in increasing the use of aspirin in this clinical situation, in changing behaviour rather than simply improving knowledge. We wonder whether these guidelines will effect change such that aspirin use is increased. Similarly, it has been recognised for some time that the use of angiotensin converting enzyme inhibitors is beneficial for patients with cardiac failure, and the challenge remains the same.

Maybe the effort and funding expended in reinforcing what is already known might have been better directed in trying to change clinicians' behaviour in those areas where there is already an identifiable research to practice gap. We have shown that by adapting the North of England guidelines for angina for local use, the uptake of aspirin for patients who have had an infarction can be considerably increased, at fairly low cost, in inner city practice. Evidence needs to be synthesised, and these guidelines are useful source documents, but we believe that the primary focus of guideline research must now shift to implementation of extant knowledge rather than summarising existing research. The challenge does not seem to be in improving the knowledge base, but in improving the utilisation of that knowledge within real life clinical situations.

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Carotid endarterectomy should have been mentioned

Editor—Eccles et al's article describes guidelines for the use of aspirin in the secondary prophylaxis for vascular disease in primary care.[1] In patients with stroke or transient ischaemic attacks we are advised to prescribe aspirin for four years, at which point we should then continue the treatment indefinitely.

We were surprised, however, that in the face of two major randomised controlled trials showing a clear benefit for carotid endarterectomy over aspirin alone,[2 3] there is no mention of the "evidence based" benefits of this operation. It is depressing how few patients are offered this treatment,[4] and one reason for this must be a lack of education. A brief mention of carotid endarterectomy should have been included, and, as it stands, this article is misleading and therefore irresponsible.

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Authors' reply

EDITOR—We agree with Fitzmaurice and Thomas that guidelines are not self implementing and require strategies to help healthcare professionals to use them, and that such activities are best planned with locally obtained knowledge of potential barriers and facilitators.[1 2] We cannot agree, however, that effort spent on developing guidelines is not worth while and should be solely devoted to implementing extant knowledge rather than summarising existing research. Guidelines cover not only effectiveness but also safety, appropriateness, side effects, quality of life, and resource implications—all expressed in a form that is helpful to clinical decision making. If local groups are to have a sound basis on which to act then the quality of guidelines is of fundamental importance and guideline developers have to maximise the validity of their guidelines.

Validity is maximised by systematic review as the method of synthesising evidence, by having a multidisciplinary guideline development panel, and by having evidence linking within the guideline.[3] Fitzmaurice and Thomas describe the local adaptation of a guideline resulting from such a process, and it seems surprising when they argue that the development of such guidelines is not worth while. They imply that knowledge has not changed between our stable angina guideline and aspirin guidelines. The area that they cite—aspirin as an antithrombotic in patients with stable angina—is an example of a clinical situation where the evidence has changed. The angina guideline based its original recommendation on the analysis of the Antiplatelet Triallists' Collaboration.[4] This relied on patients with stable angina being regarded as "high risk," and the direct evidence from trials in patients with stable angina was equivocal. Adding the findings of the Swedish Angina Pectoris Aspirin Trial[5] (published after the analyses of the Antiplatelet Trialists' Collaboration) has offered a firmer basis from which to derive recommendations for practice and permitted clear recommendations on the dose of aspirin that is effective.

One of the aims of rigorous guideline development is to make the process of deriving recommendations explicit. The recommendations (mis)cited by Chant et al have different strengths. Up to four years the recommendation is based on evidence from randomised controlled trials and thereafter on assumptions from extrapolation. Chant et al's request for a brief mention of carotid disease is special pleading—the focus of the guideline is quite clearly stated as the use of aspirin, not the management of carotid artery disease. The guideline in no way detracts from the further appropriate investigation of patients with any of the indications for aspirin use and is thus neither misleading nor irresponsible.

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Internet can be accessed from NHSnet

EDITOR—Keen raises several issues in his article about the cost effectiveness of the NHS computer network, NHSnet, for general practices.[1]

Firstly, he seems to present the internet as an alternative to NHSnet, implying that practices can choose between one or the other. In fact the internet can be accessed from NHSnet though a secure gateway (though internet users cannot browse NHSnet). Thus practices connected to NHSnet can access useful internet sites, such as Medline, Bandolier, and the Cochrane Database, as well as a range of on line journals including the *BMJ*. These can be accessed direct from their internet addresses or via sites designed to bring together useful sites of electronic information.

Secondly, Keen has not taken into account the rate of development of general practice computing. Increasing numbers (around 10%) of general practitioners are now "paperless" and consulting just electronically (NHS Executive information management group, May 1998, personal communication). Many of these receive their pathology reports electronically but have to rely on scanning or manual summaries of paper hospital letters to ensure that there is an electronic record of these hospital consultations. These documents could be word processed by the consultants' secretaries and sent to the practice by electronic mail, which would cut out the expense of printing, postage, and handling and manually entering or scanning the data into the general practice system. Large cost savings can be made if this information can be sent within a secure network. An ISDN (high speed digital phone line) connection to the NHSnet can be acquired for less than the cost of setting up a scanning system.

Thirdly, creating links to sources of good evidence is not given sufficient emphasis. The importance of clinical governance has been emphasised in the white paper on the new NHS.[2] Primary care groups for populations of around 100 000 will have to communicate and share policy; the NHSnet provides a secure medium within which this should take place. Information systems giving timely access to sources of evidence based medicine are a part of this. We have found in our project—the doctor's desk,[3] in which pilot general practices have been given access to these sources of evidence based medicine via NHSnet—that having this information on the consulting room's personal computer can be useful in decision making. The NHSnet can be an information source to foster good practice.

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3 http://drsdesk.sghms.ac.uk

Visual impairment is not enough to assess need for treatment

EDITOR—Reidy et al found that 6% (92 of 1547) of an elderly population had serious and potentially remediable visual impairment (<6/60) and far higher numbers had less serious impairment.[1] Since many of these people were not in touch with eye services it would be easy to conclude that, for example, the volume of cataract surgery should be substantially increased. The findings are important but caution is needed in drawing conclusions for service provision. Case definitions are critical in epidemiological investigations. The authors chose a visual acuity threshold of <6/12 in the worst affected eye as part of their case definition, giving rise to high prevalences. However, this choice needs to be justified because it may critically affect the implications of the study. Firstly, it is not the level of visual acuity that is important but the impact of visual impairment on a person's life—in other words the degree of visual handicap.[2 3] A level of 6/18 or even 6/24 may not interfere with some people's lifestyles, particularly if vision in the other eye is better. In the absence of a standard measure of visual handicap it would have been helpful to present a range of prevalence ratios depending on the visual acuity threshold

adopted for treatment. Using a treatment threshold of 6/24, as has been applied elsewhere, would have suggested far lower levels of unmet need.[4] Secondly, the additional health benefits from expanding current treatment may be lower than the average benefits now being realised. Unless the additional costs are also lower, cost effectiveness, in terms of the extra benefits per pound spent, will diminish. Cataract surgery at 6/36 which restores vision to 6/6 is likely to generate greater health improvement than surgery at 6/18 but costs the same. For priority setting within ophthalmology services, it would therefore be valuable to have information on different base levels of need. More generally, such data would assist health boards and authorities in decisions about the costs and benefits of different levels of service provision since they must compare the health improvements which would be generated by treating more patients at 6/18 with those from investing resources in other ophthalmological treatments or in other specialties. Finally, it would be easier to judge the generalisability of the study's findings, particularly in terms of the apparent unmet need, if the currently available services in the area had been described—for example, the adequacy of primary care services and recent cararact surgery rates.

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Provision of intensive care for children

Effective transport systems are essential

Editor—Ratcliffe's recommendations for paediatric intensive care are well supported by the improved outcomes she refers to.[1] However, she did not address the reasons why sick children are treated in small, low activity, and ill equipped units. These reasons may seem self evident—for example, community preference for local care and the perceived disadvantages of or harm caused by transfer to a distant city.

Community perceptions have to be changed so that best care is seen as preferable to nearby care. This depends on a rapid response medical retrieval service with expertise in intensive care that can be deployed to the referring hospital quickly enough to create the impression that the paediatric unit is closer than it actually is. This may require retrieval services with a high enough activity to maintain a 24 hour service with medical, nursing, and support staff on immediate standby. Our experience is that an activity of over 1000 retrievals a year is needed to meet this goal, which may mean having regional retrieval services acting for several paediatric intensive care units rather than one for each unit. The service must have dedicated ambulances to minimise delays. Regional services can deploy teams independently of staffing constraints on a particular unit yet can form close links with units to maintain professional standards and expertise.

To avoid another child dying in transit any new system would have to include a "teletriage" process offering immediate telephone access to senior clinical advice. It would also need the collaboration of relevant clinicians and ambulance staff to ensure that care before transfer was appropriate and that a management plan was devised (including the optimal destination).

Successful regionalisation of paediatric intensive care depends on an effective and responsive infrastructure for transporting patients. The infrastructure must be developed around the needs of the referring hospital and the patients; it should launch teams to patients regardless of shortage of intensive care beds or other problems. Unless medical retrieval is made an important part of the system the death of a child while being moved from a hospital that has been told not to provide intensive care will inevitably lead to calls to reverse the regionalisation process.

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1 Ratcliffe J. Provision of intensive care for children. *BMJ* 1998;316:1547-8. (23 May.)

Tertiary centres are unproved

Editor—Like Dr Ratcliffe we would like to improve paediatric intensive care.[1] We support the need for specialist tertiary paediatric intensive care units and an investment in training and organisation. However, we do not agree that all, or even most, acutely ill children have medical needs which are fundamentally different from those of critically ill adults. The experience of the child who died after being moved between several hospitals is repeated regularly by adults requiring intensive care.[2]

The excess mortality among paediatric intensive care patients reported in Trent[3] may reflect the general underprovision of intensive care in the United Kingdom.[2] Gemke and Bonsel concluded that differences in mortality among paediatric intensive care units were largely explained by differences in severity of illness.[4] Indeed, for the low risk patients mortality was higher in the tertiary centres than in non-specialist centres after case mix was adjusted for.

Many children, particularly the older ones, have straightforward intensive care problems. They show the same pathophysiological response as adults and depend on essentially the same equipment and principles that are used in general intensive care. For example, an adolescent with multiple injuries may be better cared for in a centre dealing regularly with trauma than in a hospital concentrating on neonates and infants. There are many disadvantages to overcentralising care, including deskilling of local hospitals, the breakdown of family centred care, and the additional cost of transporting patients.

We feel that the framework document[5] relies heavily on data skewed towards neonatal and infant care, inadequately represents general intensive care opinion, and doubt the ability to provide level 2 and 3 care as described. If no difference in outcome can be shown, children with critical illness are best cared for close to where their parents live. Resources for intensive care are scarce. They may be better spent on improving the majority of units and providing additional support for straightforward paediatric admissions rather than on an elaborate, expensive, and unproved paediatric intensive care system. There needs to be some mechanism for deciding when a child requires the special services provided by a tertiary paediatric centre. Perhaps clinical judgment could be used rather than a decree from on high that anyone less than 16 years old needing intensive care has to be treated in a specialist unit.

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5 National Coordinating Group on Paediatric Intensive Care. *Paediatric intensive care: a framework for the future*. London: Department of Health, 1997.

Evidence does not support tertiary care

Editor—Ratcliffe states that several studies have shown that paediatric intensive care services should be centralised.[1] However, the evidence to support this policy is not clear cut.

The comparison of Trent region in England with Victoria State in Australia, which showed that risk adjusted mortality in the centralised Victoria system was half that in the distributed Trent system, was not a study of children in the two areas but of admissions.[2] All children admitted to local intensive care services and then transferred to central facilities (mainly in Victoria) were double counted: firstly, as survivors from the local unit and then as deaths or survivors from the central unit. This may help to explain why length of stay was so short in Victoria (because it was actually length of stay for that admission not for that episode) and could explain the difference in risk adjusted mortality between areas.

Ratcliffe refers to two other studies. Both found that for children who have the highest risk of death care in tertiary facilities is associated with a reduction in that risk. However, the Dutch paediatric intensive care assessment of outcome (PICASSO) study also found an increased risk of death in tertiary facilities for low risk children,[3] and the only unit in that study whose mortality significantly exceeded that expected after adjustment for case mix was one of the largest units. The other study excluded all transfers and deaths before admission,[4] which makes the value of these data for assessing the benefits of a tertiary referral system doubtful.

A positive relation between volume and outcomes has not been shown to hold true generally, and has been shown specifically not to hold in adult intensive care.[5] This is an uncertain evidence base on which to implement a policy of centralisation based on "lead" centres identified mainly by their volume of activity rather than production of good outcomes.

Ratcliffe observes that there is no validated paediatric scoring system for severity of illness in the United Kingdom and no information about long term outcome. Until these gaps in the knowledge base are remedied, it will be difficult, if not impossible, to identify the optimum configuration for paediatric intensive care services in the United Kingdom.

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Author's reply

Editor—The correspondence highlights the question whether the structure of the paediatric intensive care service should be changed before detailed information on outcomes is available from research in the United Kingdom.

Such research must assess premorbid clinical state, morbidity, and mortality. It will take at least five years to complete, and research proposals have been submitted. The importance of such research was acknowledged by the national coordinating group, which identified an urgent need to improve the organisation and integration of the service.[1] The group defined the standards of care that are essential for managing a critically ill child from acute presentation onwards, and the configuration of the service developed from this. These standards included appropriately experienced multidisciplinary staff, sufficient patient throughput to maintain skills, and the resources to staff and run a transport service. The stand alone transport service suggested by Berry would not fit easily into the distribution of tertiary paediatric centres within the United Kingdom.

Goldhill and Withington state that the framework for the future document[1] is skewed towards infant and neonatal care. Numerically, the paediatric intensive care population is concentrated in the younger age range, with 40% of patients being younger than 1 year and 70% younger than 5.[2] There are overlaps with some aspects of neonatal intensive care but it is a distinct area of practice.

Nicholl suggests that the research supporting centralisation of paediatric intensive care is not clear cut. Pearson and Shann have reanalysed their data in the light of his comments (personal communication). Admission rates were similar for the two populations but crude mortality was 45% higher in Trent, and this difference remained after adjustment for severity of illness. The lower lengths of stay for Victorian children were not explained by possible double counting in the process of transfer. Using a logistic regression model without ventilation, the odds ratio for risk of death rose from 2.09 to 2.37; a worse outcome for Trent children. In the Dutch study,[3] the increased rate of death for lower risk children in tertiary facilities related to severe and incurable chronic disease which the PRISM score does not encompass.

I believe there is enough evidence to change the organisational configuration of paediatric intensive care to provide a more integrated service. The next stage must be informed by detailed United Kingdom paediatric intensive care research.

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Results of Trent and Victoria study are valid

Editor—Several of the electronic responses on the *BMJ*'s website have referred to our study of all children from Trent (England) and Victoria (Australia) who received intensive care over 12 months.[1] The admission rate was 1.2 per 1000 children in both places but crude mortality was 45% higher in Trent. This difference persisted after severity of illness was adjusted for. We suggested that mortality was low in Victoria because almost all children are admitted to a single, large specialist paediatric intensive care unit staffed by full time paediatric intensivists and nurses.

Nicholl suggests that the higher risk adjusted mortality in Trent might be because more children from Victoria were transferred from one intensive care unit to another and were therefore counted twice. In fact most children were transferred directly, bypassing their local unit, whereas Trent children were more often admitted to their local intensive care unit. Only seven transfers were counted twice in Victoria compared with 24 in Trent. Transfers do not explain the differences in mortality or length of stay.

Nicholl also suggests that a lower rate of ventilation in the first hour might explain the higher risk adjusted mortality in Trent. However, without ventilation the odds ratio for risk of death rises from 2.09 to 2.37. So the outcome in Trent is even worse if ventilation is ignored.

The poor results we found in Trent do not reflect underprovision of intensive care. In fact, the admission rates were almost identical, but children stayed 84% longer in intensive care in Trent. This meant many more bed days so more money was being spent on looking after children in intensive care in Trent with a higher mortality.

Berry rightly stresses the need for high quality retrieval services. However, 1000 retrievals a year are needed only if there is a freestanding transport service, as in New South Wales. The retrieval service in Victoria transports roughly 200 patients a year. The cost of this system is lower than a freestanding one because the existing intensive care infrastructure is used to support the transport service.

Our study provides clear evidence, which is supported by studies in other countries, that very ill children are best looked after by medical and nursing staff who work full time in paediatric intensive care. Surely it is time to stop looking for excuses for the high mortality in Trent and for Britain to ensure that all children who are intubated for more than 12-24 hours are looked after in large specialist paediatric intensive care units.

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1 Electronic responses. Provision of intensive care for children. *eBMJ* 1998;316. (www.bmj.com/cgi/eletters/7144/1547; accessed 27 October)

Knowledge of cardiothoracic ratio adds to cardiovascular risk stratification

Editor—Hemingway et al report that cardiothoracic ratio in healthy middle aged men predicted coronary heart disease, using data from 1203 men in the Whitehall study.[1] This increased risk was mainly restricted to men with the greatest cardiothoracic ratio, with little evidence of a dose-response association. We have repeated the analysis on 5734

Adjusted hazard ratios (95% CI) for effect of cardiothoracic ratio on all cause mortality and mortality from coronary heart disease (CHD) in men aged 35-64 in collaborative study

	All cause mortality (1636 deaths)			Mortality from CHD (623 deaths)		
Cardiothoracic ratio (fifths)	No of deaths	Adjusted hazard ratio*	Adjusted hazard ratio with extra adjustment†	No of deaths	Adjusted hazard ratio*	Adjusted hazard ratio with extra adjustment†
0.42	294	1	1	87	1	1
-0.44	306	1.23 (1.04 to 1.44)	1.21 (1.03 to 1.42)	114	1.48 (1.12 to 1.96)	1.41 (1.06 to 1.87)
-0.46	318	1.19 (1.01 to 1.39)	1.18 (1.01 to 1.39)	117	1.43 (1.08 to 1.88)	1.35 (1.01 to 1.78)
-0.48	287	1.13 (0.96 to 1.33)	1.14 (0.96 to 1.35)	122	1.58 (1.20 to 2.08)	1.46 (1.10 to 1.94)
>0.48	431	1.25 (1.08 to 1.46)	1.21 (1.03 to 1.42)	183	1.64 (1.26 to 2.13)	1.46 (1.11 to 1.92)
Trend		P=0.018	P=0.08		P=0.0002	P=0.018

*Adjusted for age, systolic blood pressure, diastolic blood pressure, cholesterol concentration, smoking, Rose angina, and electrocardiographic evidence of ischaemia.
†Also adjusted for social class, father's social class, deprivation category, car use, MRC bronchitis, body mass index, and forced expiratory volume in one second.

men from the west of Scotland collaborative study,[2 3] who were aged 35-64 when they were screened in 1970-3 and had their cardiac and thoracic diameters measured from a chest *x* ray film. Over a 21 years' follow up 1636 men died, 623 of the deaths being due to coronary heart disease (ICD 9 codes 410-414 and 429.9).

The table shows hazard ratios adjusted for the same variables as in the Whitehall study except for heart rate, which was not measured in the collaborative study. An additional column with extra adjustment for socioeconomic and other relevant variables is also given. For mortality from coronary heart disease the hazard ratio for men with a cardiothoracic ratio in the top fifth in the models with similar adjustments in the Whitehall and collaborative studies were virtually the same: 1.65 and 1.64 respectively. Unlike in the Whitehall study, however, we found evidence of a gradient of increasing risk, moving from the fifth with the lowest cardiothoracic ratio to that with the highest. Although the association between cardiothoracic ratio and mortality from coronary heart disease was attenuated when further adjustment was made for additional risk factors (including socioeconomic position, lung function, and symptoms of respiratory disease), the mortality associations remained substantial.

The people who benefit most from efforts aimed at preventing coronary heart disease are those who have most to gain because they are at highest risk.[4] Identifying the level of coronary risk is therefore important for targeting interventions such as cholesterol lowering drugs.[5] As Hemingway et al suggest, knowledge of the cardiothoracic ratio could usefully add to such risk stratification, although the fact that routine or screening chest radiography is no longer done means that the necessary information will often not be available to the healthcare provider.

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Guideline may help in prescribing vigabatrin ◑

EDITOR—In 1997 a severe visual field defect was reported in three adults who had taken vigabatrin for two or more years.[1] The following is a consensus guideline from a paediatric advisory group addressing the prescription of vigabatrin in children.

(1) The defect seems to be specific—a bilateral and symmetrical peripheral constriction with relative temporal sparing which, rarely, may be severe. The incidence in adults is estimated to be 10-20%. Its pathogenesis is unclear; it may or may not be reversible. The defect is not reliably identified by confrontation testing.

(2) If a pre-existing defect might be present, then perimetry should be done by an ophthalmologist or optometrist before vigabatrin is prescribed.

(3) Children already taking vigabatrin and who have a cognitive age of more than 9 years, should have perimetry assessed by the Goldmann or Humphrey technique. In children aged under 9 there is currently no reliable method of assessing the effect of vigabatrin on visual fields.

(4) Standard electrophysiological tests (visual evoked potentials or electroretinography) are of no value in assessing the effect of vigabatrin on visual fields.

(5) Ideally, visual fields should be tested every 6-12 months in children continuing to take vigabatrin.

The fundamental issue when prescribing vigabatrin is one of risk versus benefit—the potential risk of the visual field defect developing against the potential benefit of seizure control; this must be discussed with the family.

(1) Children who are already certified blind will have an altered benefit:risk ratio, possibly in favour of the drug.

(2) Children who have or who are at risk of developing a visual field defect should not be prescribed vigabatrin.

(3) Children taking vigabatrin whose seizures are well controlled should not automatically stop taking the drug. Evidence suggests that the defect is unlikely to develop if perimetry gives normal results after more than two years of vigabatrin treatment. Also, progression is unlikely after drug withdrawal, and recovery may occur. If the defect is identified the continued use of vigabatrin will depend on the overall clinical situation.

(4) Vigabatrin currently remains the drug of choice for infantile spasms. Limited data suggest that vigabatrin could be withdrawn without a relapse in infants who have not had any spasms for six months. An exposure time of six months may be too short for the visual field defect to develop.

(5) Vigabatrin is currently regarded as the drug of first choice (for children with seizures caused by tuberous sclerosis) or the drug of second or third choice for children with other symptomatic or cryptogenic partial epilepsies.

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1 Eke T, Talbot JF, Lawden MC. Severe, persistent visual field constriction associated with vigabatrin. *BMJ* 1997;314: 180-1.

(This guideline reflects current evidence as of August 1998. The names of the members of the advisory group are available from Dr Appleton.)

◑A longer version of this appears on our website (www.bmj.com).

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