Letters

Death rates from leukaemia are higher than expected in areas around nuclear sites in Berkshire and Oxfordshire

EDITOR—As a result of the report that a fire at the United States Air Force base at Greenham Common in 1958 may have caused radioactive contamination near Newbury, Green Audit (Wales) has compared the number of deaths from leukaemia in children aged 14 years and younger from 1981 to 1995 in the Newbury area with that within nearby county districts.

The table shows results for the triangular area defined by Oxford, Newbury, and Reading. It is notable that the districts with significantly higher relative risks are those that contain the outfalls for licensed releases of radioisotopes from the nuclear sites at the Atomic Energy Research Establishment, Harwell; the Atomic Weapons Establishment, Aldermaston; and the Royal Ordnance Factory, Burghfield. Bithell et al, however, found no significant excess of leukaemia between 1966 and 1987 within a 25 km radius of the 23 nuclear installations that they studied.[1]

In 1989 the Committee on Medical Aspects of Radiation in the Environment reported on childhood leukaemia in west Berkshire and confirmed a significant increase in incidence (relative risk 1.3; $P<0.05$) between 1972 and 1985.[2] The committee established that since 1948 all three nuclear sites had been releasing radioactive gases into the immediate surroundings and liquid effluents into the river Thames at Sutton Courtenay (from the Atomic Energy Research Establishment) and at Pangbourne (from the Atomic Weapons Establishment), and into the river Kennet (from the Atomic Weapons Establishment and Royal Ordnance Factory). Geographical constraints would concentrate most of the radioisotopes within the two river valleys; inhalation and ingestion could result in differential contamination of the populations. Data in the committee's report suggest that south Oxfordshire would be most strongly affected followed by Newbury, which would be a little less strongly affected, and that both these areas would be much more strongly affected than more remote districts upwind or upriver of the nuclear sites.

There is no sea dilution effect in this area and the pollution is likely to remain in the local environment, unlike releases from British Nuclear Fuels at Sellafield. Recent measurements of plutonium-239 and plutonium-240 confirm this. Croudace et al found soil concentrations as high as 10 Bq/kg.[3] This is more than 10 times the highest amounts expected from fallout from weapons testing—compare with the range 0.17-0.41 Bq/kg.[2]

It is possible that these radioactive emissions might have harmful effects on those living near the sites or in areas close to the rivers where effluents are discharged. The committee concluded that levels of exposure were too low to cause any measurable increase in leukaemia both at Sellafield and in west Berkshire.[2] The risk factors that were used to support this view, however, are derived from the studies of Hiroshima, which are of short term, high dose external exposure. Concern has been expressed recently that these risk factors may be unsuitable when used to measure the effects of long term, low dose internal exposure.[4]

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Table 1 Comparison of deaths from leukaemia (ICD 204-208) in children aged 0-14 in county districts in Oxford, Reading, and Newbury areas near nuclear sites, 1981-95. (Source: Office for National Statistics)

	Person years at risk	Observed deaths (O)	Expected deaths (E)	Relative risk (O/E)	Poisson P† (x>O-1)
Oxford city	283 930	3	3.9	0.78	0.75
Cherwell	392 380	7	5.3	1.3	0.27
West Oxford	265 000	5	3.6	1.4	0.29
South Oxfordshire	361 750	12	4.9	2.45**	0.0047
Vale of the White Horse	326 490	3	4.4	0.68	0.815
Newbury	420 240	11	5.7	1.93*	0.031
Reading	379 840	6	5.2	1.15	0.42
England and Wales	145 775 000	1980	—	—	—

*P<0.05, **P<0.01.
†Probability, assuming a Poisson probability distribution for deaths from leukaemia with expected mean E, that a number of deaths equal to or greater than the number observed should occur.

1 Bithell JF, Dutton SJ, Draper GJ, Neary NM. Distribution of childhood leukaemias and non-Hodgkin's lymphomas near nuclear installations in England and Wales. *BMJ* 1994;309:501-5.
2 Committee on Medical Aspects of Radiation in the Environment. *Third report: the incidence of childhood cancer in the west Berkshire and north Hampshire area in which are situated the atomic weapons research establishment, Aldermaston and the Royal Ordnance Factory, Burghfield.* London: HMSO, 1989.
3 Croudace IW, Saunderson DCW, Warwick PE, Allyson JD. *A regional study of the radiation environment of Greenham Common, Newbury district and surrounding areas.* Southampton: Southampton Oceanography Centre, 1997.
4 Bramhall R, ed. *The health effects of low-level radiation: proceedings of a symposium held at the House of Commons, April 1996.* Aberystwyth: Green Audit, 1997.

MRI scanning to diagnose osteomyelitis in United States and Glasgow

Astute clinicians and experienced paediatric radiologists are the essential factors

EDITOR—Gordon C S Smith asks whether doctors at the Royal Hospital for Sick Children in Glasgow would have felt inhibited about asking for a magnetic resonance scan in another trust had his daughter presented with osteomyelitis.[1] The answer is no: we do such scans as often as required. He also asks how we would make the diagnosis without a scanner of our own. We use ultrasonography regularly, and I have accurately diagnosed bilateral tibial osteomyelitis with subperiosteal collections using this modality. Bone scans and computed tomograms are often diagnostic, and magnetic resonance imaging has not yet, in my experience, been essential for the diagnosis. An astute clinician and an experienced paediatric radiologist are the essential factors in achieving a correct diagnosis.

A new magnetic resonance imaging scanner, for which money is being raised by public appeal, will undoubtedly be useful, but Smith is wrong to assume that basic paediatric pathology cannot be diagnosed without one.

Finally, he asks why a much smaller proportion of the gross national product is spent on health care in Britain than in the United States. The answer is simple: for the past 20 years the electorate has voted for governments of low taxation.

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1 Smith GCS. Resonant images from the United States. *BMJ* 1997;315:133-4. (12 July.)

Children at Royal Hospital for Sick Children, Glasgow, have MRI scans if necessary

EDITOR—We were disappointed to read Gordon C S Smith's account of the diagnosis of osteomyelitis in his daughter after they moved to the United States.[1] While we wish him well in his career in basic research, we would give him two pieces of advice that might help him in this: firstly, never publish on a topic about which you are not well informed; and, secondly, given a particular set of facts, do not believe that they can be interpreted in only one way. If that were the case, Alexander Fleming would have sacked the laboratory cleaner.

On the basis of the clinical details that Smith has provided, we offer here an alternative scenario. This is what might have happened had his 5 year old daughter been seen at the Royal Hospital for Sick Children, Glasgow, rather than the small district hospital in the United States to which he refers in his article. When seen in the emergency department with a fever and refusal to bear weight, Smith's daughter would have been referred to the orthopaedic surgeon on call—either a specialist registrar or one of the paediatric orthopaedic fellows. She undoubtedly would have been admitted. She would have been seen by one of the five paediatric orthopaedic surgeons at the hospital, either that day during the evening ward round or the next morning. (Incidentally, she would have been seen by her consultant, as are all patients in the department, at least once a day while under his or her care.)

Blood tests and radiography would undoubtedly have been done, and, in view of the history, a bone scan would have been obtained the next day. This would have shown, without doubt, an area of increased uptake in the left fibula. Antibiotic treatment would then have been started, and Smith's daughter would have had an excellent chance of complete recovery as the development of the abscess would have been aborted.

Smith fails to appreciate that the early diagnosis of osteomyelitis is a clinical one and depends neither on blood tests nor on radiological investigations. These are certainly useful in the management of the late complications of bone infection, but, as we never tire of telling our students, complications are due to late diagnosis more often than to late presentation.

Although a magnetic resonance scanner is not necessary in such cases, we have recognised the need for one, and fundraising is currently under way. In the meantime we regularly refer children who would benefit from such an investigation to the facilities at the Western Infirmary or the Institute of Neurosciences.

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⁂We received four other letters making similar points.—EDITOR

Advice to authors

We receive more letters than we can publish: we can currently accept only about one third. We prefer short letters that relate to articles published within the past four weeks. We also publish some "out of the blue" letters, which usually relate to matters of public policy.

When deciding which letters to publish we favour originality, assertions supported by data or by citation, and a clear prose style. Letters should have fewer than 400 words (please give a word count) and no more than five references (including one to the BMJ article to which they relate); references should be in the Vancouver style. We welcome pictures.

Letters, whether typed or sent by email, should give each author's current appointment and full address. Letters sent by email should give a telephone and fax number when possible. We encourage you to declare any conflict of interest. Please send a stamped addressed envelope if you would like to know whether your letter has been accepted or rejected.

We may post some letters submitted to us on the world wide web before we decide on publication in the paper version. We will assume that correspondents consent to this unless they specifically say no.

Letters will be edited and may be shortened.

Funding is important for randomised trials of surgery

EDITOR—In their editorial on removing bias in surgical trials A G Johnson and J Michael Dixon mention the safety and efficacy register of new interventional procedures.[1] We consider this register to be an encouraging start, but it is not a substitute for randomised controlled trials of new surgical techniques.

The opportunity to participate in randomised controlled trials should be offered to all suitable patients because randomisation minimises bias and moderate bias may obscure or exaggerate moderate differences between treatments.[2] Some patients may decline to participate in randomised controlled trials,[3 4] but they should still be systematically followed up so that the outcomes among those who were randomised and those who were not can be compared, as was done in respect of coronary artery bypass surgery.[5] In addition, such cohort studies may enable the detection of multiple, rarer, or unexpected secondary effects.

Unlike drug trials, surgical interventions are often irreversible, and we believe that this is an even more compelling reason to undertake high quality randomised trials of surgical techniques that can give generalisable results. To obtain adequate statistical power and to know more about variabilities among surgeons, multisurgeon and multicentre studies are required.[2] The Medical Research Council's international subarachnoid aneurysm trial accepted entries only from surgeons who had already undertaken 30 such procedures. We believe, though, that randomisation should begin as soon as possible because it is important to obtain some idea of the learning curve associated with a new procedure. The inclusion of early cases does not prevent the collection of later cases, and allowance can be made for a surgeon's experience in the analysis of results.

In the West Midlands the urgent need to stimulate high quality randomised trials in surgical and other disciplines has been recognised through regional research and development funding of a clinical trials unit and senior clinical time being dedicated to facilitating surgical and other trials work. This is a collaborative arrangement between providers and purchasers of health services and local universities, all of whom recognise the need for such work. As Johnson and Dixon point out, surgical trials are not easy to undertake, but they are urgently needed to help surgeons maximise the population's health through their interventions. Concern over funding barriers to randomised controlled trials should be alleviated by future national guidance on responsibility for funding of excess treatment costs of randomised controlled trials—that is, extra costs that would be incurred if the new treatment became standard practice.

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1 Johnson AG, Dixon JM. Removing bias in surgical trials. *BMJ* 1997;314:916-7. (29 March.)
2 Collins R, Peto R, Gray R, Parish S. Large-scale randomised evidence: trials and overviews. In: Weatherall D, Ledingham JGG, Warrell DA, eds. *Oxford textbook of medicine.* Vol 1. Oxford: Oxford University Press, 1996: 21-32.
3 Toynbee P. Random clinical trials are one of life's biggest gambles. *BMJ News Review* 1997 Mar:34.
4 Lilford RJ, Jackson J. Equipoise and the ethics of randomisation. *J R Soc Med* 1995;88:552-9.
5 Hlatby MA, Calift RM, Harrell FE, Lee KL, Mar D, Pryor B. Comparison of prediction based on observation data with the replies of randomised controlled clinical trials of coronary artery bypass surgery. *J Am Coll Cardiol* 1988;ii:237-45.

Who is responsible for child mental health?

The increasing fragmentation of health services for children is the main problem

EDITOR—Robert Goodman is right to point out that a huge number—a fifth or possibly even more—of children and teenagers experience distress or are maladjusted.[1] It does not follow, however, that child mental health is, or even should be expected to be, the responsibility of child psychiatrists alone. Parents and other caregivers have a major responsibility, but many professionals other than child psychiatrists manage children with mental health problems. A

recent report indicated that the "paediatric profession deals with more children and young people with emotional and behavioural disorders than any other single discipline." [2] Paediatricians, especially those practising in predominantly non-inpatient settings, can, by virtue of their holistic and developmental perspective, help in "recognising the boundaries between disorder and extreme form of distress," as envisaged by Sue White.[1] A considerable proportion of my own workload as a paediatrician is "behavioural paediatrics."

In their origin and expression, mental health problems in children are influenced by the interaction between biological and environmental factors. Their amelioration, and the promotion of mental health generally, necessitates coordination at strategic level and at clinic level between health, social services, and educational agencies. Health professionals—for example, health visitors, general practitioners, paediatricians, psychologists, and psychiatrists—have a part to play in prevention, early detection, and management of mental health problems. Rather than abdicate their responsibility by leaving the management of some major mental health problems to social services and education, health professionals should aim to maintain their legitimate role in partnership with the two agencies, at all levels.

The main problem at present is the increasing fragmentation of health services for children, which makes it extremely difficult to achieve the goal of close, professional multidisciplinary functioning. Developing innovative approaches to collaborative working in helping to solve the problems of individual children and families and convincing politicians of their worth are the real challenges confronting all professionals interested in child mental health.

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1 Goodman R. Child mental health: who is responsible? *BMJ* 1997;314:813-7. [With commentaries by A Hall, B Daines, S White.] (15 March.)
2 Kurtz Z, Thornes R, Wolkind S. *National survey of mental health services for children and young people. Report to the Department of Health.* London: DoH, 1994.

Everyone should work together

EDITOR—There were many valid comments in both Robert Goodman's opening salvo on who is responsible for child mental health and the invited commentaries.[1] None of those engaged in the debate, however, referred to the accepted working practices instituted after the dreadful death, over 20 years ago, of Maria Colwell, a child who was killed by her stepfather after being returned to her mother and stepfather from foster parents against her wishes. Don't we all know that the health and welfare of troubled children are too important to be left to the advice of a social worker, psychiatrist, or educational psychologist acting alone?

Moreover, I could not but help mourn the fact that the work of the community paediatrician was omitted from this debate despite the recognition of this work in numerous recent reports.[2-5] Yes, we are the doctors who are out there in the local clinics, in the nurseries and schools, receiving advice from our frontline nurse colleagues, the health visitors and school nurses. They are out there in the homes and in the classrooms of such troubled children and see much and report more. These children do not suffer what I would call "ordinary misery." Poverty, loss of home or of a parent's job, unsuitable accommodation, parental ill health, loss of loved parents through separation and divorce, the arrival of serial partners—all contribute to the aggressive, forlorn, acting out, and bullying behaviours that we see.

Of course, the doctors—whether they be child psychiatrists or community paediatricians—do not have the answer. But let's be honest and recognise that we all have something to contribute; only by working together do we have any hope of bringing relief to these troubled and troublesome children.

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2 Hall D, ed. *Health for all children.* 3rd ed. Oxford: Oxford University Press, 1996.
3 Polnay L, ed. *Health needs of school aged children.* London: British Paediatric Association, 1995.
4 Department of Health. *Child health in the community: a guide to good practice.* Leeds: NHS Executive, 1996.
5 Department of Health. *A handbook on child and adolescent mental health.* Leeds: NHS Executive, 1995.

Too many children are falling through the cracks in a confused system

EDITOR—It was reassuring to read the debate on the responsibility for child mental health.[1] While children with purely medical problems and their prevention are dealt with so well by the primary healthcare team, there are other children, equally or more vulnerable, who could be recognised by health visitors and general practitioners at an early age, with subsequent support and treatment coming from appropriate sources.

Some conditions, such as attention deficit disorder, dyslexia, autism, and dyspraxia, are recognised and treated as medical conditions; others are not. Another group of children—those from disturbed or single parent families, those caring for disabled parents, and those with one or both parents dependent on alcohol, depressed, or addicted to drugs—may be severely understimulated, neglected, or abused. All these children deserve recognition, understanding, and positive counsel, at least.

It is agreed that the earlier most problems are addressed the better the outcome, yet a reluctance to acknowledge and tackle certain conditions prevails when support and treatment are elusive. It is during childhood when most problems germinate and when preventive intervention is viable, and it seems inane that "child mental health services are fortunate if their funding reaches 5% of that of adult services." [1] A suggested figure for the proportion of prisoners with dyslexia (50%) exemplifies the need for a gradual reallocation of funds.

Currently too many children are falling through the cracks in a confused system. Perhaps the best safety net we can provide—until parents and medical, social, and educational services combine and coordinate resources—is acknowledgement, advice, and preschool opportunity. The children would be saved from frustration and failure, parents from anxiety, and peers and teachers from the effects of disruption. Boosting the confidence and maximising the potential of our disadvantaged young people would be a well rewarded investment.

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People rely on medicine and psychiatry to explain the vicissitudes of life

EDITOR—Robert Goodman's analysis of the limitations of the medical model in child mental health holds true beyond child psychiatry.[1] Most patients seen by forensic services or drug dependency units have problems that are as much the product of social as of individual processes.[2] Only a minority show clearcut psychiatric disease, which is arguably the core remit of psychiatrists and the main justification for our lengthy and expensive medical training. Yet it is psychiatrists who generally have the highest positions, prestige, and salaries in these specialisms. In my field I witness the rapid psychiatrisation of the impact of disasters and wars, which extends from patients attending clinics for help to encompass, supposedly, the post-traumatic stress of whole populations in war zones.

These professional developments are underpinned by wider changes within Western culture this century. Medicine and psychiatry have displaced religion and come to be major providers of explanations for the vicissitudes of life and of the vocabulary used by ordinary people to describe them. This does not just serve the interests of doctors. Politicians may find it convenient if the medicalisation of distress obscures its social and political origins. To many citizens it now seems that their entitlements—whether to scarce social resources or to official recognition of victimhood—are better preserved if their distress is reframed as pathology and through sick roles endorsed by doctors. As the social fabric frays in Britain the medical model will be more than ever pressured to deliver and more than ever exposed when it fails to do so. The stubborn fact remains: the biopsychomedical basis for static diagnostic categories cannot routinely encompass the dynamic interplay between cultural, social, and situational processes and individual mental life and behaviour.

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1 Goodman R. Child mental health: who is responsible? *BMJ* 1997;314:813-7. [With commentaries by A Hall, B Daines, S White.] (15 March.)
2 Summerfield D. Some reflections on dynamics and dilemmas in a DDU. *Br J Addict* 1990;85:589-92.

Professionals must offer a multifaceted approach

EDITOR—By arguing for a narrow definition of mental health and a restricted remit for child mental health professionals, in particular for child psychiatrists, Robert Goodman has played into the hands of those seeking to divest themselves of any responsibility for the wellbeing of children.[1] His challenge for debate about the medicalisation of childhood problems has been offered at a time when many politicians, parents, and professionals are seeking quick-fix solutions to problems that lie not solely with individual children but also within a context of disrupted attachments, inappropriate parenting, and increasing inequality. As Bob Daines pointed out in his commentary, one response to this complexity or untidiness might indeed be to emphasise the boundary around the individual.[1] "Unrealistic expectations" may then be neatly offloaded on to other agencies. However, this neither makes the problems more amenable to intervention nor helps us with furthering our understanding about the nature of child mental health disorder.

Surely, the crux of the argument concerns our understanding of the aetiology, developmental course, and efficacy of intervention in child mental health disorder. Nowhere does Goodman think this through from first principles; he merely acknowledges that "there is need for debate." He is not therefore in any position to justify his implied acceptance of a role for child mental health professionals in, for example, hyperkinesis or autism. Why not leave it all to paediatric neurologists or community paediatricians?

In describing the potential role of child psychiatrists Goodman seems to be ignorant of the specifications of the Joint Committee on Higher Psychiatric Training. Training should include not only paediatric neurology but a range of models of understanding that inform assessment, treatment, supervision, and consultation work. Such training leaves child psychiatrists in a unique position when presented with worried or worrying children; they are able to determine whether their deviation from an appropriate developmental trajectory is likely to be a temporary state fairly easily rectified by brief direct or indirect intervention or constitutes a disorder for which a more intensive treatment package is required.

Child mental health problems can be understood only within the framework of many layered contexts, ranging from the molecular through to the societal. Professionals who can offer a multifaceted yet clearly focused approach that encompasses biological, behavioural, pharmacological, psychoanalytic, and systemic principles of assessment and intervention are essential if these problems are to be managed effectively.

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Attention deficit and hyperactivity disorder needs to be recognised

EDITOR—Bob Daines complains that attention deficit and hyperactivity disorder is hard for him to diagnose and that doctors prescribe methylphenidate with little discrimination.[1] If that is so then I suggest that the responsibility rests largely with the British psychiatric and psychology establishments and their ignorance and prejudice about this condition.

Britain is years behind the United States and most developed countries in relation to attention deficit and hyperactivity disorder, with a continued reluctance to accept the underlying organic basis of the condition.[2 3] General practitioners, as in other mental health issues, tend to look to the specialists for guidance, but here they often look in vain. What little guidance they receive tends to be tinged with the prejudice that "there's no such thing" or that the disorder is more to do with parental inadequacy or unrealistic expectations of childish behaviour.

So, faced by distracted children and distracted parents, the general practitioners manage as best they can, often relying on items in the medical tabloid press to guide them. Or else they may use the information pressed on them by patients, as there is a growing consumer lobby, including family support groups, of parents who are rightly incensed at the establishment's neglect of what can be a devastating condition for children and their families. While a report of a working party of the British Psychological Society on attention deficit and hyperactivity disorder shows the traditional reluctance to acknowledge that the condition is one more of nature than of nurture (though nurture remains a factor), at least the society has produced a report, with an expressed commitment to provide help to children and their parents.[4] My family found that the medical, educational, and social services failed in that regard with my son, who has severe attention deficit and hyperactivity disorder and oppositional defiance disorder, and consequently we have moved to Australia. Here the National Health and Medical Research Council is about to release its final version of a report on advice for health professionals and educators about attention deficit and hyperactivity disorder.[5]

Is it not now time for the Royal College of Psychiatry and its paediatric counterpart in Britain to examine the issue objectively and produce a similar report with guidance for their members and for general practitioners on the diagnosis and management of this condition? Until doctors have authoritative guidance on good practice they should not be criticised for trying to help their patients as best they can.

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1 Goodman R. Child mental health: who is responsible? *BMJ* 1997;314:813-7. [With commentaries by A Hall, B Daines, S White.] (15 March.)
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4 Attention deficit hyperactivity disorder. *Psychologist* 1996;9:435-6.
5 Australian National Health and Medical Research Council. *Draft report of the working party on ADHD.* Canberra: ANHMRC, 1996.

Responsibility for services for runaway children must be shared

EDITOR—F Lawrenson's editorial on children who run away from home highlighted what the charity ChildLine has long been aware of—that child abuse is commonly a precursor to children running away.[1] In a study of telephone calls to ChildLine from 2205 runaway and homeless children we found that over a third had run away because of child abuse. Children mainly reported physical abuse (593 child callers).[2] Of course, children may well be less likely to name sexual assault, but, even so, 169 child callers reported sexual assault, with 26 of these describing both sexual and physical assault. Our studies of children in care found that over a third who had run away were trying to escape bullying or assault.[3 4]

Our callers were extremely reluctant to involve police or social services; they feared that they would simply be returned home. This exposed them to a precarious, dangerous existence on the streets. The provision of safe houses and street projects are inadequate for the number of runaway children aged under 16.

Running away does indeed need to be taken seriously. The editorial argued for more coordinated services but asked who should take responsibility for them. In ChildLine's view the responsibility must be shared, and the newly proposed local authority committees (involving health, education, police, welfare, youth, and voluntary childcare organisations), which will plan services for children in need, offer a way forward, but only if they operate on a truly cooperative basis.

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1 Lawrenson F. Runaway children: whose problem? *BMJ* 1997;314:1064. (12 April.)
2 Barter C, Keep G, MacLeod M. *Children at crisis point.* London: ChildLine, 1996.
3 Morris S, Wheatley H. *Time to listen.* London: ChildLine, 1994.
4 MacLeod M. *Children living away from home.* London: ChildLine, 1997.

Reducing morbidity from insertion of chest drains

Clamping may be appropriate to prevent discomfort and reduce risk of oedema

EDITOR—Jonathan Hyde and colleagues' description of the insertion of chest drains was both clear and helpful.[1] The authors state, however, that there is no definite indication for clamping a chest drain.

In our oncology practice most drains are inserted to drain pleural effusions. Large pleural effusions drain rapidly, and most patients experience considerable discomfort, with chest tightness and coughing. In addition, removing a large collection of either air or fluid from the pleural cavity carries a recognised risk of inducing pulmonary oedema, which has been reported as occasionally fatal.[2-4] The risk of chest discomfort and pulmonary oedema occurring as a result of rapid lung re-expansion is thought to be related to how long the lung has been compressed and airless.[2] Clamping the chest drain after removing one litre of pleural fluid is a simple manoeuvre that allows the lung to re-expand in a controlled manner. This lessens chest discomfort and virtually eliminates the risk of inducing pulmonary oedema.

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1 Hyde J, Sykes T, Graham T. Reducing morbidity from chest drains. *BMJ* 1997;314:914-5. (29 March.)
2 Trapnell DH, Thurston JGB. Unilateral pulmonary oedema after pleural aspiration. *Lancet* 1970;i:1367-9.
3 Janocik SE, Roy TM. Re-expansion pulmonary oedema: a preventable complication. *J Ky Med Assoc* 1993;91:143-8.
4 Isaacs SM. Fulminant reexpansion pulmonary oedema in a patient with AIDS. *Ann Emerg Med* 1994;24:975-8.

Patients must be disconnected from positive airways pressure before insertion of drains

EDITOR—Jonathan Hyde and colleagues are correct in asserting that knowledge of basic principles and use of appropriate equipment would help in reducing the morbidity and occasional deaths associated with the insertion of chest drains.[1] Unfortunately, there is an important omission from their discussion, from the standard surgical texts, and from the manuals on life support.[2 3] Serious complications can occur when chest drains are inserted in patients receiving positive pressure ventilation. It is essential to recognise that the behaviour of the lungs under conditions of positive airway pressure differs from that during spontaneous breathing, when the airway and pleural pressures are either negative or atmospheric.

When inserting a drain in a ventilated patient it is essential to disconnect the patient from positive airways pressure as the pleura is breached, in much the same way, and for the same reason, that one asks for the lungs to be deflated before one opens the chest. If the airways pressure remains positive as the pleura is breached, the lung will be forced up against the insertion site and out through the wound rather than collapsing away from the drain insertion site as occurs during spontaneous breathing. This tendency is exaggerated if a high positive end expiratory pressure is applied. Unless the lungs are deflated it is difficult to pass any instrument or drain into the pleural cavity past the lung without damaging it. This error has resulted in numerous incidents of chest drains being inserted into the lung,[4] which in turn leads to considerable morbidity and sometimes death. Abandoning the trocar during insertion of the drain does not entirely protect against intrapulmonary placement of the drain unless the lungs are also deflated. We would like to see this important and simple message included in all discussions on the subject of the insertion of chest drains.

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1 Hyde J, Sykes T, Graham T. Reducing morbidity from chest drains. *BMJ* 1997;314:914-5. (29 March.)
2 American College of Surgeons Committee On Trauma. Thoracic trauma. In: *Advanced trauma life support program for physicians: instructor manual.* Chicago: American College of Surgeons, 1993.
3 Advanced Life Support Group. Chest drain insertion. In: *Advanced paediatric life support: the practical approach.* 2nd ed. London: BMJ Publishing, 1997.
4 Peek GJ, Firmin RK, Arsiwala S. Chest tube insertion in the ventilated patient. *Injury* 1995;26:425-6.

Setting target rates for breast feeding would probably be a waste of resources

EDITOR—In their paper on setting targets for increasing the breast feeding rates in Scotland Harry Campbell and Anne Gibson state that targets should be related to actions known to be effective, yet the evidence they quote in support of this is weak.[1]

Of the six papers showing that improving hospital practices can increase the rate of breast feeding, none were British. Their generalisability must be questioned, given the cultural diversity in breast feeding practices. One paper is a review based on a meta-analysis, which emphasises the methodological heterogeneity of the studies and that nearly all intervention studies are hospital based.[2] Given the increasing tendency in Britain for early discharge after childbirth, this can provide only part of the answer to the problem of increasing the duration of breast feeding. One paper is a randomised controlled trial of counsellors on breast feeding, which showed no effect on duration of breast feeding.[3] Of the remaining papers, one reports an increase in the frequency of exclusive breast feeding among mothers who had postnatal support from a paediatrician in a hospital clinic in Istanbul and the others look at policies for mother-infant separation or gift packs that include formula milk.

The authors then quote experience from Norway, Denmark, Australia, and Canada showing that coordinated interagency action can substantially increase breast feeding rates. There is a danger here of assuming a retrospective cause and effect relation. Unfortunately, we do not know which component of the interagency action, if any, was responsible for the upward trend in breast feeding in these countries, while breast feeding rates in Britain have changed little since 1980. A more critical review of the evidence would have suggested that setting target rates for breast feeding would probably be a waste of resources.

As a researcher in this field I also wish to highlight the recurrent problem of unclear definitions in the measurement of breast feeding rates. Campbell and Gibson do not clearly differentiate between the incidence and the prevalence of breast feeding. The quinquennial surveys of infant feeding by the Office for National Statistics define being breast fed as having been put to the breast even if only once,[4] but many research studies do not clearly state definitions or exclusivity of breast feeding. This is essential before targets are set and to create a more robust body of evidence about how we can promote breast feeding.

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Slutsky effect does not seem to explain circaseptennial rhythm in ear growth

EDITOR—M J Campbell states that the circaseptennial rhythm in ear growth is probably due to the Slutsky effect.[1] Slutsky found that the use of moving averages might induce cycles in purely random series.[2] It is not clear how and to what extent this effect was relevant to our analysis and how it distorted the P value in Fisher's κ test.[3] Campbell generated "some random data," applied our smoothing procedure, and obtained a peak in the periodogram at six years, which he declared significant ($P = 0.066$) on Fisher's test. We do not find his argument convincing because he generated only a single set of random data and because the peak was at six and not at our predicted seven years.

To examine the relevance of the Slutsky effect for our analysis we approximated the simultaneous sampling distribution of the period associated with the maximum periodogram value and Fisher's κ statistic by a Monte Carlo test.[4] We generated 9999 datasets of 54 scores randomly sampled from a standard normal distribution, smoothed them over six scores (which resulted in 49 scores), performed a spectral analysis, and

counted the number of times the period associated with the largest periodogram value was seven and Fisher's κ coefficient was equal to or greater than our observed value.

Altogether 374 four of the datasets resulted in a maximum at seven and a Fisher's κ value equal to or greater than our observed value. If the observed value is added to the count then P = 0.0375 (95% confidence interval 0.0339 to 0.0413). Although this value is larger than the P value we obtained without taking the Slutsky effect into account, it is still evidence against the null hypothesis that the ear growth series is pure white noise. The Slutsky effect does not seem to be sufficient to explain the circaseptennial rhythm in the ear growth data.

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Anaesthetists are younger than other doctors

Editor—D J M Wright and A P Roberts are on difficult ground if they persist with their claim that some groups of doctors, particularly anaesthetists and those from the Indian subcontinent, die earlier than others.[1] Kay-Tee Khaw has emphasised their classic error of using the unknown denominator.[2] The authors say, in reply, that they "know no source" for "a valid denominator" and that in particular they "are not sure why Professor McManus regards anaesthetics as a young specialty."[3]

A useful statistical source is the Medical Directory on CD ROM. The winter 1996-7 edition describes 128 417 doctors in the United Kingdom, of whom the oldest is 105, having been born in 1892 and qualified MD in Leipzig in 1915. Doctors who qualified before 1979 are likely to have fixed on their career specialty, and they form the main group among whom death may be expected. The table shows the age distribution of the 70 476 doctors who qualified before 1979 and the proportion who are anaesthetists (defined as all doctors who described their specialty as anaesthetics or mentioned anaesthetics in their entry).

Proportion of anaesthetists among doctors listed in Medical Directory, by year of qualification or registration

Year of qualification or registration	Approximate median age (years)	No of doctors	No (%) of anaesthetists
1910-9	104	5	0
1920-9	94	376	4 (1.06)
1930-9	84	3 183	51 (1.60)
1940-9	74	9 622	221 (2.30)
1950-9	64	13 460	479 (3.56)
1960-9	54	17 086	963 (5.64)
1970-9	44	26 744	1262 (4.72)

The mean age of doctors, estimated from median group ages, is 53.4 years for anaesthetists (n = 2980) and 56.6 years for other doctors (n = 67 496), a difference of 3.2 years, which immediately explains most of the effect found by Wright and Roberts. The clear decrease in the proportion of anaesthetists with increasing age is unlikely to be explained by the only obvious artefact present in the Medical Directory—that some doctors remove their names after retirement. Unless Wright and Robert wish to come up with an explanation for why their stereotypical "tense and introverted anaesthetists, happier with more solitary rather than social pursuits,"[3] are also more likely to remove themselves from the directory then I think that they must accept that they have no evidence for the earlier death of anaesthetists. I leave it to them to use the same source to confirm that doctors who qualified in the Indian subcontinent are also younger than other doctors and to accept that their data tell us nothing useful about differences in mortality between different groups of doctors.

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Audit of diagnosis and management of hypertension in primary care

Interpractice variation in prevalence of hypertension is due to inadequate detection

Editor—An interpractice audit of the diagnosis and management of hypertension in primary care found that hypertension remained uncontrolled in over three fifths of hypertensive patients despite the implementation of an intensive audit; this is disappointing.[1] Another common deficiency in the management of hypertension—the failure of health care services to detect and diagnose the condition—was not, however, addressed.

Data from table 1 in the paper can be used to calculate the crude prevalence of hypertension in individual practices. This varied between 1.61% and 5.01% (mean 3.2%) during phase 1 of the study, which suggests that the detection of hypertension varied widely. This is confirmed by data from the MEDICS (morbidity and epidemiology data interchange and comparison scheme) project in Northumberland. Prevalence data for common chronic diseases have been collected from computerised records at 33 practices covering a population of 200 000 people. In March 1995 the crude prevalence of recorded hypertension in adults (aged > 15) varied from 3.0% to 13.2% (mean 6.7%)—from 2.6% to 10.9% (mean 5.5 %) in men and 3.4% to 15.6% (mean 7.9%) in women. This variation persisted when data were indirectly standardised for age and expressed as standardised morbidity ratios.

MEDICS practices should probably be better than most at detecting and recording hypertension because they are all computerised, provide data on the prevalence of hypertension for the health promotion banding scheme, and participate in a project collecting morbidity data. They are also situated in a district where improving the care of patients with hypertension has had a high priority (through initiatives such as district-wide audits of hypertension and the local development and dissemination of best practice guidelines).

Despite this, MEDICS practices had difficulty detecting and recording hypertension: the mean prevalence of hypertension after direct standardisation for age was 6.0% for men and 7.5% for women, compared with 19.3% and 18.4% respectively in the national health survey (which was standardised to the European standard population).[2] One practice, however, increased its recorded prevalence of hypertension by 62% (from 4.4% to 7.1%) over four months by searching computer records for those of patients prescribed antihypertensive drugs or with high blood pressure but no diagnosis of hypertension.

Good management of hypertension requires systematic identification of cases and structured review and intervention over long periods.[3] The evidence of interpractice variability in recorded hypertension, a reduced prevalence of hypertension recorded in practice computerised databases compared with national data, and the ability to identify more hypertensive patients from practice computerised records suggest that future audits should address adherence to best practice in detecting and recording hypertension.

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1 Mashru M, Lant A. Interpractice audit of diagnosis and management of hypertension in primary care: educational intervention and review of records. *BMJ* 1997;314:942-6. (29 March.)
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Definition of uncontrolled blood pressure used in study is unclear

EDITOR—Mahendra Mashru and Ariel Lant present interpractice audits of the management of hypertension in primary care, performed before and after educational activity.[1] Their study contains methodological problems which cast doubt on their conclusions.

The study did not have a control group, yet random allocation to intervention and control groups is required to assess the quantitative impact of education.[2 3] Only one partner in each practice formally received the educational message, and the authors relied on unmonitored diffusion of education within the group practices. The educational message may have been weakened further by the major and potentially confusing differences between the three guidelines.

It is unclear what time period was examined to determine whether three records of blood pressure were present in either the phase 1 or phase 2 audit. The number of readings used to make a diagnosis of hypertension is itself a poor guide to the quality of the diagnosis—for example, for practices that use only the last record of blood pressure to make the diagnosis the presence of two earlier, normal, records is irrelevant.

The definition of uncontrolled blood pressure used in the study is unclear, defined both as "not lowered to $<160/90$ mm Hg" and "$>160/90$ mm Hg." Patients with a blood pressure of exactly 160/90 mm Hg are handled inconsistently. As significant digit preference exists in the recording of blood pressure[4] many patients may have had single readings of 160/90 mm Hg.[5] Furthermore, the number of readings used to assess control was not stated. The apparently poor impact of education may merely reflect the effect of digit preference in reducing the responsiveness of the measure of control.

The timing of the audits is also suspect. Because the initial planning occurred before phase 1, yet may, as the authors state, have contributed to the educational intervention, an unknown improvement may already have occurred before the phase 1 audit. The change presented—that between the phase 1 and phase 2 audits—may be only part of the change produced by the intervention. Furthermore, we do not know the precise interval between the two audits (the audits could have been consecutive in some practices).

Primary care may be performing better than predicted by the rule of halves, and education may be helpful in producing improvement. Mashru and Lant's study does not really help us to decide.

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Authors' reply

EDITOR—Kevin Allan and colleagues' comments that there is interpractice variation in the prevalence of hypertension must be correct, but the assumption that it is due largely to inadequate detection of hypertension seems an oversimplification. We do, however, agree that good management of hypertension requires systematic identification of cases and structured clinical review and that future audits should address adherence to best practice in detecting and recording hypertension.

We agree with Stuart Barton and Mike Cranney's suggestion that a control group would have allowed appraisal of the quantitative impact of education by random allocation, and we pointed this out in our paper. We disagree, however, that the quantitative impact of education cannot be determined in the type of study design that we used; the study did not have parallel groups but looked instead at individual practice behaviour before and after clearly defined educational interventions. The educational method researched in our study was based on the concept of the relay of messages by one practice partner to the rest of the clinical team. Our conclusions therefore can relate only to this technique.

The three nationally recognised guidelines for the detection and management of hypertension do have differing messages. However, we specifically selected domains for our study after consensus was established between us and the participating practices at the outset. Barton and Cranney claim that the paper is unclear about what we defined as hypertension. We stated that the diagnosis depended on three separate readings of blood pressure $>160/90$ mm Hg. In reality this demanded a set of three abnormal values $>160/90$ mm Hg. Digit preference is important, and we have reported on it previously.[1] We do not think, however, that this factor would have had a significant impact on the overall conclusions of our study.

We do not agree with Barton and Cranney about the timing of the study as this was precisely controlled. The sequencing of practices entering phase 1 or 2 was rigidly adhered to by careful timetabling to avoid variability.

We agree with Barton and Cranney's suggestion that education ought to be helpful in producing improvement in the management of blood pressure. We would, however, emphasise the final sentence in our paper: not enough research has been undertaken to determine which particular types of educational method would be most effective in changing clinical behaviour.

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1 Feher MD, St John-Harris K, Lant AF. Blood pressure measurement by junior hospital doctors—a gap in medical education. *Health Trends* 1992;24:59-61.

Medical practice is more complicated in remote locations

EDITOR—John Rees's commentary on David Berger's account of the management of pneumothorax caused by a vicious fish in the Solomon Islands seemed rather negative.[1] Rees considered it wrong that Berger opted for a canoe rather than an aeroplane to transport the patient to hospital. Having worked in the Solomon Islands for more than a year in an even more remote province, I applaud Berger's treatment of the patient and the fact that he requested comments on his actions.

There are so many variables that affect medical practice in the Solomon Islands that a doctor's decision to use one form of treatment (or in this case transport) rather than another should never be condemned. Many factors have to be taken into account when we make similar decisions every week: sudden changes in the weather, aircraft fuel loads, the patience of the aircraft's captain (and the other passengers), time of day (the small aircraft here are often unable to land at night), the state of the provincial health budget, etc. Berger's patient survived so the decision was correct. If the patient had not survived then the decision not to fly him to hospital may still have been correct.

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*Conflict of interest: A filler article in the *BMJ* written by David Berger encouraged me to contact him and resulted in my current post.

1 Berger D. A fish induced pneumothorax: dilemmas in the remote management of a sucking chest wound. [With commentary by J Rees.] *BMJ* 1996;313:1617-8. (21 - 28 December.)

Correction

Is it time to stop searching for MRSA?

An editorial error occurred in the sixth letter of this cluster on screening for MRSA by Peter Wilson and L J Dunn (5 July, p 58). Editing changed the sense of the last sentence of the fourth paragraph. The sentence should have read: "Consistent with these correlations, in the 1980s, unlike the current decade, there were no cases of MRSA in samples from our community, indicating that in our setting MRSA is no longer exclusively a hospital organism."