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Because we receive many more letters than we have room to publish we may shorten those that we do publish to allow readers as wide a selection as possible. In particular, when we receive several letters on the same topic we reserve the right to abridge individual letters. Our usual policy is to reserve our correspondence columns for letters commenting on issues discussed recently (within six weeks) in the BMJ.

Letters critical of a paper may be sent to the authors of the paper so that their reply may appear in the same issue. We may also forward letters that we decide not to publish to the authors of the paper on which they comment.

Letters should not exceed 400 words and should be typed double spaced and signed by all authors, who should include their main degree.

Childhood leukaemia and radioactive discharges at Seascale

SIR,—Dr D Jakeman's letter (20 September, p 760) about the uncertainty of any link between leukaemia in early childhood (say before the age of 5) and the discharges at Seascale in the early 1950s restates a commonly held belief that very young children are at highest risk. There is no supporting evidence, however, for supposing that such children are indeed much more radiosensitive to the induction of leukaemia, dose for dose, than adults.

After radiotherapy of the "chest" in infancy (age 0-2 months) for supposed thymic enlargement thyroid cancer was increased about 100-fold (24 cases observed, 0.29 expected), but in the same population after a 20 year follow up there were seven cases of leukaemia in some 2800 subjects, with 2.27 expected.¹ The combined results of three other, similar surveys showed two cases of leukaemia in some 2600 subjects, with about two expected.^{2,4} Only two of the total of nine cases were in children under 5 years old. Thus irradiation of a substantial fraction of the body, including bone marrow and lymphoid tissue, in over 5000 infants to a tissue dose of 1000 mSv and more did not cause a statistically established increase in leukaemia in early childhood. A cohort study (after the exclusion of deaths within the first year of life) of nearly 5000 children after cardiac catheterisation under x ray guidance showed three cases of leukaemia (all at more than five years after the procedure), with 1.88 expected.⁵ An average thoracic dose of 74 mSv for cardiac catheterisation in infants and children was cited.

Even the fetus in utero is little more radiosensitive than the adult to the induction of fatal malignant disease (2-2½%/Gy v 1-2%/Gy uniform whole body irradiation), with leukaemia forming about one third of the cases induced in each group.^{6,7}

This evidence seems to show that if the Sellafield discharges caused an increase in leukaemia the radiation dose to the tissues of origin of childhood leukaemia must have been much more effective than the 50 mSv or the 10× natural background dose cited by Dr Jakeman. Most attention has been centred on

the possibility that longlived bone seeking α emitters in the Sellafield discharges (for example, plutonium isotopes) are absorbed into the tissues to a much higher degree in very young children than in older people, so leading to bone marrow doses much larger than expected from conventional considerations. Whether radiation induced acute lymphatic leukaemia in childhood originates in bone marrow rather than in some other irradiated tissue, however, is not known.

It seems likely, when the next Seascale child dies of leukaemia, that the coroner would order tissue analyses for longlived radionuclides. However, this could not provide evidence relating to children averaging 4 years old at the time of the releases of radioactivity in the early 1950s, who, according to Dr Jakeman, would have been most at risk of contracting leukaemia. Direct evidence would be provided by similar tissue analyses of exhumed bodies of children dying from any cause in the same locality during those relevant years. If non-leukaemic children were found not to contain unexpected concentrations of α emitters there would be no direct support for the hypothesis that young children in general absorb these radionuclides from food, water, or inhaled air to a greater extent than adults. It might be argued that the young children at risk of leukaemia in the 1950s were specifically those who did absorb α emitters to a greater extent than other children (because they either were exposed to more or were intrinsically better absorbers). The smaller the minority of children with exceptionally high tissue levels, however, the fewer will be the children at the higher risk and therefore the larger must be the frequency of induced leukaemia per unit dose to cause a given number of cases of induced leukaemia. If "the population of children under 5 living in Seascale in 1955 was about 250" it seems unlikely that any of them would have died from any cause and, if so, there seems no possibility of finding direct evidence in support of Dr Jakeman's suggestion that there was a relatively transient period of increased risk from enhanced radiation exposure of tissues during the early 1950s.

Dr Jakeman's conclusion that uncertainty re-

mains applies inescapably to any single statistical correlation, however good (or bad) it may appear, but the evidential weight of that uncertainty in the Seascale case, taken on its own, must be greatly reduced by the evidence cited above from much larger numbers of children exposed to much larger tissue doses than he has estimated for the children at Seascale.

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Orchidectomy versus oestrogen for prostatic cancer: cardiovascular effects

SIR,—The trial of Dr P Henriksson and Professor O Edhag (16 August, p 413) showing that oestrogen given for prostatic cancer apparently

increases the incidence of cardiovascular disease may be very relevant in interpreting a strong association of factor VII activity and of fibrinogen concentration with the incidence of ischaemic heart disease in men that we have recently reported from the Northwick Park heart study.¹

There is little doubt that oestrogen contributes to the increased risk of arterial disease in women using oral contraceptives. The relation between oestrogen administration as hormone replacement therapy in older women and the risk of arterial disease is less clear and may actually suggest a benefit. Whatever the explanation for the different effects of oestrogen on the risk of cardiovascular disease in oral contraceptive users and patients receiving hormone replacement therapy, data from randomised controlled comparisons in men are of special interest.

The Swedish study showed that 25% of the patients treated with oestrogen experienced major cardiovascular events during the subsequent year, of which just over half were due to arterial disease, deep vein thrombosis accounting for most of the other events. Although numbers were small, the excess of events in the patients treated with oestrogen was seen in those without evidence of atherosclerosis as well as in those in whom there was such evidence—suggesting a thrombotic explanation, further supported by the speed of the effect. In addition, oestrogen on its own seems to lower blood cholesterol and raise high density lipoprotein cholesterol values (though it may raise triglyceride values), and this tends to argue against an atheroma enhancing explanation.

Thus it was appropriate to raise the possibility that the pathogenesis of the cardiovascular complications was through altered coagulability. Dr Henriksson and Professor Edhag considered this explanation chiefly in terms of decreased antithrombin III activity. Factor VII coagulant activity and the plasma fibrinogen concentration are other components of the coagulation system that are altered (both are raised) by oestrogen, both in those taking oral contraceptives and in those receiving hormone replacement therapy.^{2,3} There is an obvious parallel between the oestrogen dose dependent response in factor VII activity⁴ and the risk of arterial disease⁵ in users of oral contraceptives. Thrombin generation increases with the rise in factor VII coagulant activity after cold activation.⁶ The findings of the Swedish trial are similar to those of the higher dose oestrogen arm of the coronary drug project.⁷

It would be valuable to measure factor VII activity, plasma fibrinogen, and other components of the coagulation system in further studies of men receiving oestrogen preparations, particularly within a randomised controlled comparison. Meanwhile, the results of the Swedish trial provide suggestive, if circumstantial, evidence that the relations of factor VII coagulant activity and of fibrinogen with the risk of ischaemic heart disease are of pathogenetic importance.

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- 1 Meade TW, Mellows S, Brozovic M, *et al.* Haemostatic function and ischaemic heart disease: principal results of the Northwick Park heart study. *Lancet* 1986;ii:533-7.
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- 6 Miller GJ, Seghatchian MJ, Walter SJ, *et al.* An association between the factor VII coagulant activity and thrombin activity induced by surface/cold exposure of normal human plasma. *Br J Haematol* 1986;62:379-84.
- 7 Coronary Drug Project Research Group. The coronary drug project. Initial findings leading to modification of its research protocol. *JAMA* 1970;214:1303-13.

Obstetric ultrasound: getting good vibrations

SIR,—In his leading article on obstetric ultrasound Mr Tom Lind (6 September, p 576) asks whether the procedure is safe (answer, almost certainly) and whether an explanation should be offered during the scan (answer, in a perfect world, yes). I agree but think that the two questions are linked together, because when we show and explain we are also likely to increase the exposure to ultrasound. Some practical suggestions may be useful.

Firstly, to reduce exposure time the image should preferably be frozen during verbal sessions. Mothers, however, like to see the movements of their baby, so we have tried another approach. We connected a video recorder to the scanner and told the mothers that we would record the whole examination and thereafter would play it again and talk. It worked, the examiner could perform an undisturbed and unextended study, but the complete procedure took more time for the examiner.

Secondly, it should be possible to reduce the intensity of the ultrasound to a level that despite the increased noise in the image still offers the desired diagnostic information. This is especially important in studies in early pregnancy, when the energy passes virtually undamped through the full bladder to the uterus. It is most disappointing that many manufacturers of ultrasound equipment omit this simple facility (so as to avoid unpleasant and "unnecessary" noise in the images?), but we have found it quite easy for the hospital engineer, by trial and error, to install one or more resistors that can be switched into the transmitter circuit whenever possible. (It is far more complicated to measure how much the output is reduced.)

Thirdly, the target for the ultrasound should be considered, and the sex of the fetus is only rarely of diagnostic importance. It appears unwise, and may even be unethical, to expose fetal gonads to unnecessary ultrasound. To avoid any pressure from mothers we have the policy never to try specifically to determine the sex and never to tell it should we incidentally observe the sex during the scan.

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Treatment of functional abdominal pain by transcutaneous nerve stimulation

SIR,—Ms K Sylvester and colleagues reported the outcome of treatment by transcutaneous electrical nerve stimulation in 29 patients with the irritable bowel syndrome (23 August, p 481); results of physical examination were stated to be normal in all cases. Most such patients would complain of tenderness in various sites during routine abdominal examination, usually giving the impression of deep tenderness through a relaxed abdominal wall. Examination of the mildly contracted abdominal wall often discloses areas that are equally or more tender on palpation but obviously more superficial.

The explanation for these tender areas of muscle,

occurring in functional abdominal pain, would seem to be a viscerosomatic reflex arc. Had these areas been looked for some indication of which patients would respond to transcutaneous electrical nerve stimulation might have been obtained. Simple demonstration of the site of tenderness in muscle can often reassure the patient.

The pain of myocardial ischaemia is easier to study. Lewis and Kellgren found that local anaesthetic block of left chest wall trigger points generally doubled exercise tolerance in angina.¹ Conversely, injecting similar points in the right chest wall with hypertonic saline caused the pain of angina to be referred to the right side. Nicholas and colleagues described somatic signs occurring in myocardial infarction.²

Currently, such signs are sought for and treated only by alternative medical practitioners. Ms Sylvester and colleagues have drawn attention to a much easier method of treatment, and their results are, indeed, encouraging.

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- 1 Lewis T, Kellgren JH. Observations relating to referred pain (trigger point production by injection of hypertonic saline). *Clin Sci* 1939;4:47.
- 2 Nicholas AS, Domenic AD, Ehrenfeuchter W, *et al.* A somatic component to myocardial infarction. *Br Med J* 1985;291:13-7.

SIR,—While the report (23 August, p 481) on the treatment of functional abdominal pain by transcutaneous electrical nerve stimulation was laudable in its aims, no tests of significance were mentioned. Unfortunately, the data that were given were of the wrong type to allow the reader to calculate these for himself using the three tests most applicable for this trial design—namely, the sign test, the Wilcoxon matched pairs signed rank test, and the *t* test for correlated means. If the least favourable conditions are assumed then according to a sign test results for the first month are significant ($p=0.0129$) but those at three and six months are not. Thus uncertainty remains over whether or not any significant result was present after one month. This is particularly unfortunate as one of the central tenets of the ensuing discussion is that the prolongation of effect past two months implies that it is not solely a placebo effect. There is nothing in this report to suggest that transcutaneous nerve stimulation works by anything other than a placebo response or for longer than one month.

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AUTHORS' REPLY—Dr Hall points out that some patients with the irritable bowel syndrome have areas of localised abdominal tenderness. We recognise this sign, and some of our patients exhibited it. We also recognise patients who have localised points of tenderness on the abdominal wall that persist on tensing the abdominal wall musculature, thus indicating that the tender point is located in the muscle wall or more superficially. Many of these people seem to have functional abdominal pain rather than musculoskeletal disorders, and, as he suggests, it is likely that these points represent a viscerosomatic reflex arc (referred pain with cutaneous hypersensitivity).^{1,2} In some patients we have infiltrated the tender spot with local anaesthetic agents (with or without steroids), but this has not relieved their pain, nor has cryotherapy performed at the local pain clinic. We have also noted that these sensitive spots seem to move around the abdomen and are not constant in position, which also suggests that they are of a referred nature. Unfortunately, however, we have