

except in a hope that domiciliary fetal monitoring will promote more rational and convenient monitoring policies.^{5,6} We agree that there is a risk of overuse,⁴ and we are sure the debate on the value of antenatal cardiotocographs and the mission to deliver healthy babies should continue in the widest context.

Meanwhile, we fully support any call for further evaluation of domiciliary fetal monitoring, and we have recently embarked on a major collaborative research programme in the south Wales valleys. This new application is potentially useful and deserves a fair hearing.

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Do treatment protocols improve end results?

SIR,—Drs Sakari Karjalainen and Ilmari Palva found that relative survival for multiple myeloma improved by over 10% in 17 Finnish health districts undertaking trials of new chemotherapy compared with historical experience and the experience of districts not included in the trials.¹ There were some problems in the study design. For the trial area “emphasis was placed on including only patients with symptoms . . . or clearly progressive disease” (which seems to have excluded only 10 of 338 patients), but for the reference area patients were “probably not followed as systematically.” Was like being compared with like? Also, the substantially lower age adjusted incidences in the historical controls during 1959-68 than in later years could have been due to a lower rate of diagnosis rather than lower true incidence, and these years should perhaps have been excluded from the analysis. Lastly, the authors attribute the improved survival to use of a protocol in the trial districts without evidence of what regimens or protocols were actually used in the reference districts.

Nevertheless, the strength of Drs Karjalainen and Palva's approach is that it is population based. In Mr C A Stiller's review of survival of patients treated for cancer² all but one³ of the studies reported were based on treatment centres and were thus open to the important confounding factor of

selection bias. Mr Stiller describes two reports written by physicians participating in Medical Research Council trials showing longer survival for their patients with acute leukaemia compared with those treated by non-specialists.^{4,5} But in a population based study of care for childhood leukaemia in south east England the treatment regimen was a determinant of survival independently of whether care was provided locally or at a special centre.⁶ And interpretation of the available evidence should be unbiased. In a review of trends in survival in childhood cancer⁷ Mr Stiller attributes poorer survival of patients with neuroblastoma at paediatric oncology centres than at other hospitals to differential stage distribution; yet where treatment of other tumours in paediatric oncology centres resulted in better survival rates no information about stage was given.

There has been far too little research on how the organisation of cancer services affects the survival and quality of life of patients. The Finnish study yielded the surprising result that a randomised controlled trial of myeloma treatment that showed the new treatment to be worse than standard treatment produced a population improvement in survival. Drs Karjalainen and Palva attribute this to following a protocol (management of relapses and the side effects of treatment) and urge a formal randomised trial of protocols. Mr Stiller contends that centralised treatment is advantageous for childhood cancers. Would clinicians confirm this hypothesis with a randomised trial of site of care? What all agree on is that cancer registers are the most appropriate base for evaluation studies. We need further studies using good information on the populations served, accurate diagnoses and inclusion criteria, and clear data on treatment to provide good advice for policy decisions about cancer care.

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Body weight and arterial hypertension

SIR,—The conclusion reached by Dr Stig Sonne-Holm and his colleagues that “changes in body weight have a great influence on arterial hypertension,” especially in obese subjects,¹ may not be valid because of faulty methodology.

Blood pressure was measured with a “cuff measuring 12×22 cm” for all arms with a circumference less than 46 cm; and for arms with circumferences greater than this a “cuff measuring 15×38 cm” was used. (We are assuming that the quoted measurement refers to the bladder rather than cuff dimensions.) A cuff containing a bladder with the dimensions 12×22 cm will be accurate only for arms with a circumference that is 20% greater than the bladder length: 27 cm.² As no information was given on the distribution of arm circumferences in the population studied we assume that “cuff hypertension” was present in at least some of the men with arm circumferences

between 27 and 46 cm.^{3,4} Thus the observed increased prevalence of hypertension in obese subjects over controls may be, at least in part, an artefact of measurement rather than a valid association.

A London School of Hygiene sphygmomanometer, which has been shown to be inaccurate,⁵ was used in the study, and it is not stated if the arm in which blood pressure was measured was supported or not.⁶ When taken together with the possible inadequacy of the bladder the potential for inaccurate measurement of blood pressure becomes such as to cast some doubt on the high prevalence of hypertension in obese men reported in this study.

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- 1 Sonne-Holm S, Sørensen TIA, Jensen G, Schnohr P. Independent effects of weight change and attained body weight on prevalence of arterial hypertension in obese and non-obese men. *Br Med J* 1989;299:767-70. (23 September.)
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AUTHORS' REPLY.—Our study¹ was carried out in the settings of the established second survey in the Copenhagen city heart study (Østerbundersøgelser).^{2,3} In this survey a cuff with a bladder 12×26 cm was used routinely (our paper¹ and the reference paper⁴ state 12×22 cm, which is an error; this was the bladder size in the first survey). When the arm circumference exceeded 46 cm, as marked on the cuff used routinely, another cuff with a bladder sized 15×38 cm was used. We have no individual measurements of the arm circumferences.

The arm was supported during blood pressure measurement.

We agree with Drs O'Brien and O'Malley that the estimated prevalence of arterial hypertension in a population, particularly among obese people, may depend on various aspects of the technique of blood pressure measurement. Thus applying another technique in our study could have produced lower absolute values of blood pressure in this population and hence lower prevalence rates in general and a less steep increase in prevalence by current degree of obesity.⁴ The true prevalence is difficult, if not impossible, to define and ascertain, but according to numerous other studies the basic finding of an increasing prevalence of hypertension with increasing degree of obesity seems to be fairly well established.⁵ Our study addressed the influence of weight history at a given current degree of obesity on current prevalence of hypertension. To our knowledge there is no evidence indicating possibly biased blood pressure measurement depending on weight history at the same current degree of obesity.

We believe that the essential message of our study about the influence of weight history is valid as a basis for further inquiry into the mechanisms behind this influence.

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Rape and subsequent seroconversion to HIV

SIR,—The article by Dr S Murphy and colleagues verifies the unpleasant possibility of HIV transmission through sexual assault.¹ A possible method of preventing this tragedy has been the subject of our recent review.² We suggested topical application of nonoxonyl 9 for rape victims: this substance inhibits HIV in vitro and does not destroy the evidence for sperm analysis. A vinegar douche should also be active against the virus but may be deleterious to the evidence.

Another consideration is to give zidovudine, and this also should be initiated as quickly as feasible after exposure. Many hospitals now offer this type of prophylaxis for health care workers who are the victim of needlestick injuries. Given the potential toxicity of zidovudine, it is unlikely that most would advocate its use if the serologic state of the assailant was not known. One potential advantage of the local treatments advocated is that they are not associated with any deleterious consequences.

Although topical application of virucidal agents and systemic antiviral agents are potentially useful, neither has been proved to prevent HIV transmission in humans or animals when given after the exposure has taken place.

An important issue raised in our report was that only 5% of hospitals in Maryland designated as rape treatment centres had policies for victims of sexual assault. We certainly agree that all such victims should be offered HIV testing and counselling as advocated by Dr Murphy and colleagues, but these rape treatment centres should not overlook the opportunity for preventive interaction.

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What a waste of human resources

SIR,—I was pleased to see that the *BMJ* can accept an article that concerns unacceptable behaviour by colleagues.¹ Although the writer is female, the main thrust of her complaint is not sex discrimination per se. I wonder if she got any help from her defence organisation?

They advise doctors, "Do not criticise your colleagues," but what they really mean is, "Do not criticise your colleagues, but if you do, and the criticism is unfair or unreasonable, don't worry because we don't wish to know about disputes between colleagues, and it would be too risky and expensive to go through any legal procedure," so

that there is in fact no deterrent or punishment for such behaviour.

Having experienced similar treatment to that described and total frustration in obtaining any redress, I believe that the defence organisations must apply themselves to help doctors in this position and not hide behind the excuse that they are reluctant to get involved in disputes between doctors because both doctors might belong to the same defence organisation. In my case my defence society initially gave me advice to resist an unreasonable request to resign a hospital appointment; then it did nothing to support me and didn't want to know; then it changed its mind and said it would support me; and finally it decided that it wouldn't.

Although we believe such incidents couldn't really happen to us, I suspect that they are increasingly common and we need some better mechanism to help. It is not sufficient to call this type of problem "personality clash" and assume that the cause is six of one and half a dozen of the other. The legal system is grossly deficient, and a professional body would seem to be the only way forward.

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- 1 Anonymous. What a waste of human resources. *Br Med J* 1989;299:1037. (21 October.)

Immunisation: causes of failure and strategies for success

SIR,—Dr Stephanie Chadwick's experience of trying to improve immunisation uptakes is instructive and her fears that the targets set in the new contract will be counterproductive is shared by many doctors.¹ She reports the view, however, that epilepsy in a first degree relative is a contraindication to pertussis vaccination. This is not the case: the green book recommends that "the advice of a consultant paediatrician . . . be sought before a decision is made to withhold vaccine."² My advice is always to give the vaccine in such circumstances unless the parents themselves are opposed. I hope the Department of Health's advice will be even clearer on this point in the next edition as there is no evidence of adverse effects when pertussis vaccine is given to the first degree relative of an epileptic.

If those five children had been immunised, Dr Chadwick's uptake rate for pertussis immunisation would have shot up to 88.5%—not so far off 90%.

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- 2 Joint Committee on Vaccination and Immunisation. *Immunisation against infectious disease*. London: HMSO, 1988. (The green book.)

SIR,—I think that Dr Stephanie Chadwick is being too pessimistic.¹ If she started now with the same population at 3 months old I am sure that 90% would be immunised.

It is too late to persuade mothers of 2 and 3 year old children who have originally refused to have their children immunised for pertussis to accept immunisation. The only opportunity is the birth of a second child, when they may appreciate the risk to the baby posed by an unprotected older child. The persuasion has to take place when the baby is 2-3 months old, and in the face of a predicted outbreak of pertussis in the near future I am sure that Dr Chadwick would find persuasion much easier.

Five children had a first degree relative with epilepsy. This is not an absolute contraindication, and most of these children can be protected. In Oxfordshire doctors seeking further advice are usually encouraged to immunise such children.

The parents of half of her non-immunised children were contacted only by letter, which would certainly not change minds. A discussion at the 6-8 week examination is much more effective.

Dr Chadwick's positive attitude to immunisations early on would encourage mothers to get their children immunised promptly and persuade at least some of the refusers to accept. Therefore she would easily achieve 90% uptake.

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Brain stem death and organ donation

SIR,—Dr A Bodenham and colleagues refer to the hypotension (systolic blood pressure <80 mm Hg without inotropic support) seen in 33 of 52 potential organ donors as one of the "complications directly attributable to brain stem death."¹ This should be recognised as an essential feature of death of the brain stem, which includes the vasomotor centres. It must be concluded therefore that the brain stems of the 19 other potential organ donors were not truly dead, although death would have been certified on the basis of the so called brain stem death criteria.

The absence of diabetes insipidus in 18 of these patients is worthy of note and provides further evidence of persisting hypothalamic-pituitary function after brain death had been diagnosed according to the British criteria, as reported by Hall *et al* in 1980.²

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Infection after caesarean section

SIR,—Any policy that simultaneously reduces patient morbidity, reduces stress and discomfort in the postnatal period, and results in substantial economies as shown by Ms Miranda Mugford and colleagues must be welcome.¹

We believe, however, that two points raised in this paper deserve further attention in any unit before instituting such a policy. Though the Oxford group have access to a computerised database giving details of wound infections after caesarean section, many units fail to audit such events and the incidence of wound infections can only be estimated from memory or anecdote. A closer audit of infections may well point to a higher than expected rate and to other areas where substantial improvements can be made.

A study of the history of wound infection following caesarean section has recently been published.² In papers dating from the 1860s to the present day the same recurring risk factors have been noted: labour with ruptured membranes for more than six hours, excessive numbers of vaginal examinations, and poor standards of obstetric care. Attention to such detail and use of prophylactic