rising incidence of invasive cancer in younger women?

Unscreened women need cervical smears from the age of 25, earlier if possible. Unscreened women under 35 need smears so that their preinvasive cancer is treated and cured with minimal morbidity. Unscreened women over 35 need smears so that their invasive cancer is treated, which by that age means an extensive hysterectomy or radiotherapy. Some will survive. Some may die sooner or later and become statistics.

The reason why deaths from cancer of the cervix show little decrease is a half-hearted screening programme, perpetuated by the DHSS, that boosts the figures for total screening (with 61% repeat smears) but does little to diagnose preinvasive cancer in unscreened women under 35.¹

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¹ Davies SW, Kelly RM. *Br Med J* 1971;iv:525-6.

Work load produced by breast-cancer screening

SIR,—The recent article by Mr W D George and others (6 September, p 653) provides an excellent guideline for anyone who intends to set up a comprehensive breast-cancer screening programme. Their conclusions are derived from the analysis of the referral and biopsy rates of the women who actually accepted the invitation for screening. But it should be remembered that a screening campaign has a significant impact on non-responders and those who are outside the stipulated age limit. A proportion of these too become alerted about longstanding breast symptoms or fear about breast cancer and seek medical advice from general practitioners. This inevitably leads to increased hospital referrals and biopsies. The overall increase in the hospital work load due to a screening programme can therefore be calculated only by monitoring the increase caused by the screened population as well as the non-responders and women outside the age limit; in other words the entire female population.

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Bladder cancer as a prescribed industrial disease

SIR,—Professor R A M Case (30 August, p 621) fears that my statement (17 May, p 1229) about the withdrawal from industrial use by the mid-'60s of the carcinogens responsible for urothelial tumours will tempt clinicians to ignore a patient's more recent working history when considering whether to advise that a PD 39 claim should be made. I should be surprised and disappointed if that were to happen. My letter was designed to help the claiming process, not to frustrate it. The benefit is available to anyone who is found to be suffering from the disease and who meets the prescribed conditions. Provided the individual satisfies these criteria the question whether or not any of the substances specified in the prescription are still in general industrial use is irrelevant.

The Industrial Injuries Advisory Council,

an independent body which advises the Government on the working of the industrial injuries benefits scheme, is currently reviewing the schedule of prescribed diseases. I am sure that their secretary (room 216, Friars House, 157-168 Blackfriars Road, London SE1 8EU, telephone 01-703 6380 ext 4111) would be pleased to receive from Dr Case or anyone else any evidence suggesting that the benefit conditions for PD 39 should be amended.

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Site of action of intrathecal morphine

SIR,—Mr J Selwyn Crawford (6 September, p 680) suggests that intrathecal and extradural morphine produce their effects by diffusion in the cerebrospinal fluid up to the brain and points to such effects as nausea and respiratory depression as powerful evidence that this occurs. He also describes the lack of effect of a hyperbaric solution of morphine, which he says "would have produced a block to T10 had we injected a hyperbaric solution of local anaesthetic."

This is not evidence that morphine does not act on the cord, however. Local anaesthetics can act on nerve roots as they cross the subarachnoid space, but intrathecal morphine is said to act on cells in the substantia gelatinosa in the cord itself.¹ Since the spinal cord is much shorter than the vertebral column the highest segment of the cord at the level of T10 vertebra is the second sacral.² This anatomical point is not taken account of in many publications and may account for some failures of intrathecal or epidural morphine.³ Success would depend on cephalad diffusion at least to the level of the appropriate cord segments if not to the brain itself.

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¹ Snyder SH. *Sci Am* 1977;235:44.

² Warwick R, Williams PL, eds. *Gray's Anatomy*. 35 ed.

Philadelphia: Saunders, 1973.

³ Magora F, et al. *Br J Anaesth* 1980;52:247-52.

SIR,—Mr J Selwyn Crawford's letter (6 September, p 680) on the site of action of intrathecal morphine contains a number of statements which I feel are unfounded. He states: "In almost every account the assumption is made that the drug has acted by virtue of a direct effect on cells in the spinal cord. Repetition appears to be establishing credence for this assumption in the opinion of many. Yet there appears to me to be no sound basis for this belief."

On the contrary elegant studies have been performed demonstrating specific opiate receptors in the spinal cord.¹ Furthermore, experiments in animals showed that the injection of small doses of narcotic analgesics into the spinal subarachnoid space produced a potent analgesia that could be antagonised by naloxone.² The narcotics acting only at spinal level changed cord function to block not only spinal reflexes but also the operant response to painful stimuli. This prompted Wang and co-workers to examine the analgesic effects of intrathecal morphine in human subjects with promising results suggestive of a spinal action.³

Subsequently a number of clinical reports have been published on intrathecal and extra-

dural opiates with mostly encouraging results. A few less comforting reports have of late been forthcoming, and at present doubt exists whether spinal opiates work. What is required now are carefully constructed and controlled clinical studies.⁴ It is apparent, however, that the basis for the use of intrathecal opiates is sound and rests on very much more than an assumption. It is not repetition that gives credence to the proposition of the spinal action of opiates but rather sound and enlightened research work backed up by an appropriate clinical response.

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¹ Pert CB, Kuhar MJ, Snyder SH. *Proc Natl Acad Sci USA* 1976;73:3729-33.

² Yaksh TL, Rudy TA. *Science* 1976;192:1357-8.

³ Wang JK, Nauss LA, Thomas JE. *Anesthesiology* 1979;50:149-51.

⁴ Anon. *Lancet* 1980;ii:962-3.

Pleural aspiration and biopsy

SIR,—In emphasising the lack of myelo-suppression produced by intrapleural bleomycin as an advantage over intrapleural thiotepa in malignant pleural effusions, thus perhaps justifying the considerable extra cost of the former, Dr M J Ostrowski's evidence (6 September, p 681) seems incomplete. Although the theoretical risk of marrow suppression from systemically absorbed thiotepa is appreciated, I wonder how often this proves clinically important at the intrapleural doses and frequency used, and what observations may have been made concerning this when separated from the effects of simultaneously administered systemic cytotoxic chemotherapy?

I look forward to the results of the multi-centre study which he and his colleagues hope to complete in the near future, especially if it is randomised to include a group receiving intrapleural thiotepa, a group receiving perhaps mepacrine (which has also had its supporters), as well as a bleomycin group and a control group. I hope too that it is performed double blind with the prime emphasis on comparing adverse effects which actually lead to symptoms (as well as the length of interval between aspirations) in this context of incurable disease.

Finally, I did not suggest, as was stated, that one should not use bleomycin because it is four times as expensive as thiotepa, but merely that the cost (£93.20 for a 120 mg instillation) is borne in mind and weighed against any real advantages.

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Taking blood and putting up a drip in young children

SIR,—I read with interest the article "Taking blood and putting up a drip in young children" by Dr Andrew Whitelaw and Dr Bernard Valman (30 August, p 602). For anaesthetists involved in both operating-theatre and intensive-care work the problems of venepuncture and the siting of a reliable drip in a neonate or a premature baby are often compounded by previous multiple venepunctures and insertions of drips for intravenous therapy. This results in one being faced with a neonate with no visible peripheral veins. A technique in this hospital that has been employed with