investigations without priority, selection criteria, or cost-benefit considerations. The brief mention of vesicoureteric reflux gives no indication of which adults require investigation, and both here and in respect of investigation for renal scarring there seems to be confusion with paediatric practice.[9] Are we really to believe that adults require dimercaptosuccinic (DMSA) scans?

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AUTHORS' REPLY,—We acknowledge the increasing prominence afforded ultrasonography compared with intravenous urography, particularly by radiologists. Though we accept the usefulness of the technique when it is performed by experts, doctors may be less enthusiastic about some of the results obtained in routine practice, especially as the pictorial records are often not easy to interpret with precision. We have encountered bad errors with ultrasonography, such as a kidney being reported as present despite a previous nephrectomy and one being reported as absent but subsequently shown clearly by intravenous urography; but lesser errors such as the failure to describe calculi may be of common concern. As Spencer and Lindsell acknowledge, ultrasonography does not define the pelvicaliceal system in detail, which has implications for the diagnosis not only of frank papillary necrosis but also of lesser caliceal clubbing and therefore of chronic pyelonephritis and its differentiation from ischaemic scars. It cannot reliably identify ureteric calculi, may miss small renal calculi, does not directly identify urothelial carcinoma, is unreliable in diagnosing early acute obstruction, and gives no information regarding renal function.

We did not advocate urography for microscopic haematuria but for persistent microscopic haematuria; likewise we would not routinely advocate it in young women with overt haematuria complicating cystitis, but we would do so in older women or younger women with recurrent haematuria not associated with urinary tract infection. We agree that anatomical renal abnormalities are uncommon in young women with cystitis, for whom exposure to ionising radiation is undesirable and routine ultrasonographic investigation unnecessary and logistically impossible. We recommend that intravenous urography should be used in recurrent persistent (bacteriological) infection in women, some of whom will have the changes of chronic pyelonephritis, which are better defined by intravenous urography than ultrasonography. Furthermore, the elderly men whose urinary obstruction Spencer and Lindsell have defined by ultrasonography will probably need functional radiographic or radioisotopic studies of their kidneys.

Ultrasonography and intravenous urography are complementary in the investigation of urinary tract infection. In certain circumstances ultrasonography will be preferred, but in others doctors would be wise not to forsake the longer established technique.

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Open cast mines and asthma

EDITOR,—Fiona Godlee's summary of the interested parties in the debate over whether there is a link between open cast mining and asthma raises several points.[1] Firstly, would a cross sectional study of an occupational group be expected to answer the important questions raised in the report from Glynneath?[2] The answer is probably not—largely because of the healthy worker effect, and also because the subjects most susceptible to the effect of dust inhalation are excluded from study or are lost within a non-occupational control group. This is the precise question that must surely be addressed.

Perhaps British Coal and the Transport and General Workers Union might now feel inclined to fund a community based cohort study since they are confident that the issue would be resolved in favour of the industry as it is now and not as it might be improved. For instance, another hypothesis in keeping with the study's finding that asthma was less common in the workers than in the general population is that the dust exposure in the general population is quite different from the occupational dust exposure. The larger, non-inhalable particles could be the prime exposure at the workplace while the smaller, more easily dispersable, respirable dust is the material inhaled away from the mine.

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Social class differences in infant mortality

EDITOR,—Our comparison of social class differences in infant mortality in Sweden with those in Britain[1] has been commented on by Richard G Wilkinson[2] and Fabrio Parazzini.[3]

Wilkinson criticises us for not taking into account the different distributions of the population across classes in the two countries. Though we accept that the risk ratios on which we place some emphasis do not give the whole picture, we believe that the standard epidemiological index of the proportion of deaths avoidable that we use does take account of the different class distribution in the two countries.

Wilkinson argues that the best method of measuring class differences is the "slope index of inequality." This uses class specific death rates (weighted for the relative size of the class) to calculate a linear regression line, whose slope is presumed to measure "the mortality gradient across the population as a whole."

We have reservations about this recommendation. It presupposes a ranking of the British registrar general's social classes that is debatable, particularly when applied in international comparisons.[4,5] In fact, much sociological thinking has now moved away from unidimensional ranking of social classes.[6] The method suggested by Wilkinson also assumes that the residual class categories of "sole registration" and "other" may be ranked in relation to classes I to V. But why should women without a partner or registering a birth on their own be ranked in a lower class than everyone else? If the ranking of our eight categories is of dubious validity, then so is any measure that assumes the ranking.

Our conclusion is that no one index adequately captures all the important aspects of inequalities in infant mortality (or any other aspect of health or disease). Our analysis of relative class differences in infant mortality in Sweden and Britain highlighted one aspect of this issue, particularly the contrast between the neonatal period (no differences in Sweden but a difference in Britain) and postneonatal period (similar relative differences in Britain and Sweden). To quantify the relevance of socioeconomic differences in disease to public health the proportion of deaths avoidable (as also used by Parazzini[3]) is simpler to understand and makes fewer assumptions than the slope index of inequality.

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Sudden infant death syndrome in Scotland

EDITOR,—A decrease in mortality from the sudden infant death syndrome similar to that in England and Wales[1] has been seen in Scotland.[2] The mortality from the syndrome in Scotland fell from 2·07/1000 live births in 1988 (137 deaths) to 1·99/1000 in 1990 (131 deaths) and 1·33/1000 in 1991 (89 deaths). In Fife a mean of 10 deaths from the syndrome occurred annually during 1980-9, while in 1991 three deaths occurred (0·68/1000 live births). Although the numbers are small, the decrease is significantly greater than would be predicted by the Poisson distribution if such deaths occurred randomly over time ($p=0·008$).

As health visitors are important sources of information we looked at whether this decrease in mortality from the sudden infant death syndrome was associated with a change in the advice given to parents by this professional group. Eighty three health visitors in Fife returned a completed questionnaire asking about the advice given to parents about the syndrome (a response rate of 70%). Seventy two respondents stated that they had changed their advice (two respondents were recently qualified, and nine stated that their practice was unchanged). Changed advice about sleeping position was cited by 62, overheating by 34, smoking by 23, and breast feeding by two; 18 health visitors did not specify exactly what aspects of their advice had changed.

Forty five stated that their change in practice

occurred before mid-1991. The information from the chief medical officers was issued in November 1991, and in Fife this does not seem to have been the main factor in promoting change. The health visitors attributed their change in practice to journal articles (59), the mass media (53), the west Fife sudden infant death syndrome project (43), the chief medical officers' circular (42), colleagues (31), personal interest (26), and their manager (21). This suggests that the professional press and general media are the most important influences (the west Fife project is a pilot study that used a risk score for the sudden infant death syndrome). The importance of these two types of media in influencing professional practice should not be underestimated and certainly in this instance seems to have been more important than, for example, briefings from managers.

This study shows that most health visitors in Fife have changed the advice that they give about the sudden infant death syndrome and that this seems to have been concurrent with the national decline in deaths from the syndrome (which seems to have begun at the end of 1990).[2] The link between these two observations is uncertain. It will be important to assess whether a similar change in parents' care of young children occurred over this period.

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Natural remedies

EDITOR,—Minerva[1] reported that a Chinese herbal remedy bought for the treatment of eczema was noted to contain as its active ingredient a potent steroid available only on prescription. Recently I was involved with an analagous case.

We saw a 54 year woman at the gynaecology clinic after an episode of postmenopausal bleeding. At dilatation and curettage histological examination showed "proliferative endometrial activity with focal decidual change whose features were consistent with exogenous hormonal stimulation." At her initial consultation she mentioned that she had consulted with a homoeopath for perimenopausal symptoms and that he had given her a remedy called APITOP-F.

She subsequently brought along the information leaflet, which stated that APITOP-F was a "vitamin-hormone" compound that in addition to helping most conditions was important in the treatment of "impaired potency from endocrine causes, climacteric complaints, cycle anomalies [and] frigidity." The list of ingredients, consisted mainly of vitamins but also included ethinyloestradiol 10 μg. The instructions for use stated that in the absence of any clear instructions from one's practitioner one could take the preparation once a day on a long term basis. No warning was mentioned. Ethinyloestradiol taken continuously effectively explained this woman's symptoms and pathology results. The danger of unopposed oestrogens in women who have an intact uterus are obvious.

These cases raise the question of the safety of such remedies, which seem to be outside the "prescription only medicine" status even though they contain as their active ingredients prescription only medicines.

The ethical nature of these remedies must also be questioned. Here are complementary medicines which contain conventional drugs as active ingredients. How many of these remedies actually contain such ingredients? Are there any complementary remedies for asthma that contain salbutamol? This area has potential for racketeers to use "Natural is best" (and almost by implication cannot cause harm) to market drugs under the banner of being natural remedies.

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Sexual behaviour

EDITOR,—John Bancroft's editorial on research into sexual behaviour emphasises not only the current expansion in such research but also the practical difficulties in surveys that ask such personal questions.[1] Bancroft concludes that such research does not necessarily require large scale surveys and that reliability depends in part on there being a good, adequately explained purpose.

Increasing openness about the sexual activity of elderly people among the general public, support groups, and primary health care workers and in the media[2] has led to increasing demand for the investigation and treatment of impotence. Much of this demand has fallen on urology departments. Most recent studies on sexual functioning have concentrated on a younger cohort, the emphasis being the threat of AIDS. The study reported in the Kinsey report, the standard, included only two men over the age of 80.[3] I have just completed a face to face study of sexual functioning of 100 male urological patients over the age of 60 who were awaiting urological surgery for problems not related to impotence. Urology consultations, especially with discussion of possible postoperative urogenital problems, lead easily into such intimate questioning. Twenty of the 37 patients aged 60-69 with partners were sexually active, averaging coitus just less than once a week. Overall, 29 had complete erectile failure and nine were actively unhappy with their sex lives. The most important single factor determining sexual activity was the availability of a partner.

This study highlights the high level of sexual activity in some sections of the elderly population, which many young doctors regard as "sexless." Secondly, the findings concur with Bancroft's suggestions that if the techniques are suitable some valuable information can be obtained without large scale surveys.

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EDITOR,—John Bancroft points out the difficulty in finding out the truth in surveys into sexual behaviour.[1] The new sex survey has fallen into the same trap that Kinsey *et al* and Schofield stumbled over in the 1950s and '60s[2,3]: it has produced results that are misleading in that they either greatly overestimate the number of partners of male heterosexuals or grossly underestimate those of women, or both. This could lead to policy on the spread of HIV being based on false information.

In all, the study found that men had three times as many partners as women: men had a lifetime average of 9·9 partners while women had an average of only 3·4. Such a discrepancy is logically impossible. As I have explained elsewhere, every time a woman has a new partner so does a man, and worldwide the total number of heterosexual partners must be equal.[4] The only way the figures from the latest survey could be true would be if men in the survey had had two thirds of their sexual partners outside Britain. In fact, evidence suggests that women not only have slightly more heterosexual partners than men but also begin their sexual activity younger. This is logical as women on average date and marry older men. When researchers have engaged in participant observation of youth instead of conducting their research by questionnaires they have usually confirmed that women started their sexual activity younger. For example, in his classic study of Elmstown's youth Hollingshead found that schoolgirls were about four times as likely to have sex as boys.[5]

One reason why sex surveys produce the wrong figures is that a man can gain status in his group by claiming to be sexually active. It is in casual sex that the widest discrepancies are likely to arise. Although there is not the shame associated with extramarital sex these days and over a third of births occur outside marriage, recent research I have carried out with young teenagers shows that there is still a prejudice against women having multiple sexual partners. Such behaviour by men is a sign of success.

I suggest that in future the way to get nearest to the truth about sexual behaviour is to ask men about their prior experiences and the age of their partners and work out the female data from this information. This would give rise to some problems in allowing for experience with foreigners and in correcting for male overstatement. I suspect, however, that it would allow the best opportunity for accurate data.

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Boxing injuries

EDITOR,—In answer to J E U Moxon's letter,[1] the BMA's opposition to boxing is based not on moral considerations but on the available medical evidence. The board of science and education's report on boxing due to be published in June reviews research carried out since the BMA's report on boxing in 1984.[2] This additional body of scientific evidence confirms that those who participate in boxing at amateur and professional levels are at considerable risk of sustaining chronic and acute brain injury. The association will therefore continue to monitor the evidence for the risks associated with boxing and to press for its abolition. I am sure that Moxon would wish the association to take all steps to prevent avoidable deaths and injury as part of its remit to highlight issues affecting the public health.

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