study was important because of other contemporary reports.^{2,3}

Gardner *et al.*'s results² were not published until after our interviews were completed. Thus they cannot have led to any recall bias related specifically to the prenatal period. Nevertheless, there was geographic overlap and the time period and methods of control selection were similar so that our results were not independent of theirs. We have now been able to cross check our entire study population against their data base (which includes more recent cases and controls added since the original report). We excluded all subjects who were present on their data base and analysed all remaining case-control sets that were discordant for paternal radiation exposure (in one of the three time periods) using exact methods of analysis with the statistical package EGRET.

The table provides exposure classifications for the subjects in this analysis. The overall numbers are reduced, as was anticipated; in particular, the numbers classified as certainly exposed to ionising radiation have become almost negligible (one case "exposed" before conception and one control exposed postnatally, both with extremely small lifetime doses).

Exposure of fathers to ionising and non-ionising radiation

Timing	Exposure*	No of cases	No of controls
Before conception only	Ionising radiation:		
		Certain	1†
		Possible	1
		Unlikely	2
	Non-ionising radiation	2	0
	Total	5	4
Preconception, periconception, and during gestation	Ionising radiation:		
		Certain	0
		Possible	3
		Unlikely	1
	Non-ionising radiation	2	0
	Total	6	0
Postnatal only	Ionising radiation:		
		Certain	0
		Possible	1
		Unlikely	0
	Non-ionising radiation	0	0
	Total	1	3

†Total lifetime dose=1.11 mSv. ‡Total lifetime dose=2.98 mSv.

Ionising: certain= registered radiation workers (National Register of Radiation Workers or British Nuclear Fuels Ltd); uncertain=others whose job description is consistent with exposure to ionising radiation (industrial radiographers and contract workers at nuclear sites); unlikely=unlikely to have been exposed to ionising radiation but exposure cannot be ruled out.

Several points noted in the first report persist. Firstly, our data provide no evidence of independent risk associated with paternal postnatal exposure (three controls, one case). Secondly, they do show significant associations of leukaemia risk for paternal exposure before conception (11 cases, four controls; odds ratio 5.08, 95% confidence interval 1.50 to 21.99); a stronger effect but based on smaller numbers remains evident for periconceptional exposure (six cases, no controls; 2.14 to ∞). These results include four cases and no controls exposed to non-ionising radiation and two cases unlikely to have been exposed. If all of these are excluded the strengths of the associations are reduced and they are no longer significant (preconception: odds ratio 4.59, 0.74 to 45.58; periconception, 0.83 to ∞). The evidence for increased risk is not confined to West Cumbria. All the case children included in this analysis of prenatal parental exposure had diagnoses of acute lymphoblastic leukaemia.

These results yield a modest independent contribution to the scientific debate concerning possible effects of paternal radiation exposure before the birth of children. We recommend extreme caution in their interpretation: the numbers are very small; almost none of the exposures have been validated; these fathers had had a variety of other occupational exposures; and

entirely different exposures³ may be relevant in these data.

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Classifying suicide

EDITOR,—Derrick J Pounder's argument regarding the official misclassification of suicide¹ is based on the assumption that all deaths by hanging are suicidal. However, accidental death by hanging is a recognised hazard of sexual practices in which partial asphyxia, by using nooses or other means, is deliberately induced as an adjunct to masturbation.^{2,3} Public awareness of this practice has increased in recent years and the increase in fatal hangings classified as "undetermined" may reflect this fact. The extent of misclassification occurring in official suicide statistics can be clarified only by research using data on individual cases.

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1 Pounder DJ. Classifying suicide. *BMJ* 1992;305:472. (22 August.)

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EDITOR,—I share Derrick J Pounder's view that a change in the burden of proof for suicide verdicts is overdue.¹

During the 25 years from 1965 to 1989 there were 250 deaths at the Beachy Head cliffs. The inquests returned 134 suicide verdicts, 100 open verdicts, and 16 other verdicts. All the suicide or accident verdicts were substantially correct. Of the deaths with an open verdict, 97 had a suicide profile with at least three factors.^{2,3} In total, the balance of probability is that 236 were suicides.

To give an example: a 1975 victim had been in a psychiatric hospital with depression. In the fortnight before his death he made four serious suicide attempts. At Beachy Head he ran to the edge, where he was held by a witness, whereupon the victim said, "For God's sake let go, I am a mad brute.... If you don't let me go you are coming with me." At the inquest the coroner stated there was no note and "at no time on the cliff edge did he say he was going to take his life." He returned an open verdict: by any standards an obtuse verdict.

Coroners aim at consistency and accept that it is their duty not to presume suicide, but certain interpretations of "clear intent" have confined suicide verdicts to those who left a note.

Verdicts have never represented the "true" suicide rate. A review of Sussex coroners' inquests for 1485 to 1558 states, "Because of the stigma

attached to suicide... suicides must have been cases about which there could be no doubt." In 1912 M R Cozens-Hardy, reviewing a verdict, said, "Suicide is never presumed... the Judge seems to have thought it was more likely that the man committed suicide than anything else. The Judge is not entitled to act on a surmise of that nature." Even after the Suicide Act of 1961, the Court of Appeal reiterated that suicide requires an intention to kill oneself which must be affirmatively proved.⁴

Dr C A H Watts wrote that "the coroner... bends over backwards to avoid the verdict of suicide, so that the statistics are an underestimate,"⁵ at which a coroner pointed out that "it is not a question of bending... but of giving a decision in accordance with the law of the land."⁶

Presumption of suicide will harmonise British criteria with those of the Scandinavian countries.^{9,10} If others followed, comparison of suicide rates would be less of a voyage of serendipity.

The feelings of the bereaved must be considered (less publicity would help and might reduce suicides at Beachy Head), but despite the 1961 act a suicide verdict is not acceptable for some relatives, who will appeal against the coroner's decision unless there is change.

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1 Pounder DJ. Classifying suicide. *BMJ* 1992;305:472. (22 August.)

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Monitoring ambulatory blood pressure in general practice

EDITOR,—We accept that the introduction of non-invasive ambulatory blood pressure monitoring in general practice should occur within the context of research and audit, as described by Chris Silagy and colleagues,¹ since this was a major thrust of our original letter.² We remain concerned, however, that this may then lead to wholesale introduction of the technique before the levels of systolic and diastolic blood pressure requiring drug treatment have been defined. These levels will depend on whether "day" or "night" (currently imprecisely defined) or 24 hour measures of ambulatory pressure are used. At present we rely on a consensus view³ based on meta-analysis of several population studies that a daytime average blood pressure of more than 135/85 mm Hg may indicate hypertension.

The correspondence generated by our original letter illustrates the lack of consensus in practice. Coats and Clark have recently suggested that treatment should be recommended for a daytime average pressure of more than 160/95 mm Hg,⁴ while Coope and Coope recommend using a 24 hour average (which is usually lower than the daytime average) diastolic pressure of 90 mm Hg.⁵ Even more confusing, Conway, a cosignatory with Clark and Coats in response¹ to our letter, has recently advocated widespread use of ambulatory monitoring of blood pressure in general practice without providing any guidelines as to what constitutes normal or raised blood pressures.⁶ It is not our intention to suggest that "general practitioners are incapable of interpreting current evidence,"⁷ but rather, as suggested elsewhere,⁷ that specialists in the field have yet to agree a basis on which

intervention can be recommended. In addition, use of a daytime average of 160/95 mm Hg must deny treatment to many patients who would receive it on the basis of the World Health Organisation criteria,⁴ and may deny some of them the benefits in terms of preventing stroke. Furthermore, as a separate issue, we are concerned that the common assumption that patients with "white coat" hypertension are not at risk may be flawed.⁵

We believe that there is a need for prospectively collected data on the clinical outcome of patients managed on the basis of ambulatory rather than clinic pressures and suggest that this is an urgent concern. Such data might be collected by the joint action of academic centres working in this research area. Until such data have been collected, wholesale introduction of this technique in clinical practice may be premature.

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Frequency of citation and outcome of cholesterol lowering trials

EDITOR,—When I answered the critics of my paper¹ I had not read the letter from Gilbert R Thompson,² who thinks that frequent citation of trials reflects their quality, not their outcome. I have grouped the trials according to their quality (table) scoring -1 for unblindedness or use of a drug with frequent and typical side effects (trials 11-17, 19, 23-26, 28, 31, 32, 34 in the original paper); fewer than 500 subjects (trials 11-17, 21, 26); multifactorial approach (trials 28, 31, 32); not using intention to treat data (trial 27); not using randomisation (trials 11, 32); and sponsorship by a drug company (trials 12, 13, 21, 25, 33). I have followed the objection from Durrington and colleagues³ as regards randomisation; I apologise for having misunderstood the methods on that point in some of the papers.

As seen the best trials (groups 0 and -1) were cited four times more often than the others (groups -2 and -3). In the three best groups, however, the trials considered supportive by their directors were cited four to seven times more often than un-

Mean annual citation of trials arranged in four quality groups. Figures in parentheses are numbers of trials

	Quality groups			
	0	-1	-2	-3
All trials	38.8 (5)	48.3 (6)	13.1 (9)	8.8 (4)
"Supportive" trials	59.0 (3)	67.5 (4)	22.5 (4)	7.7 (3)
"Unsupportive" trials	8.5 (2)	10.0 (2)	5.6 (5)	12 (1)

supportive trials. Admittedly, the scoring is crude and arbitrary; nevertheless it is obvious that frequent citation may also reflect irrelevant matters such as a favourable outcome.

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- 1 Ravnskov U. Cholesterol lowering trials in coronary heart disease: frequency of citation and outcome. *BMJ* 1992;305: 15-9. (4 July.)
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Health care for elderly in Japan

EDITOR,—Yuzo Okamoto's paper on the crisis in the care of elderly people in Japan does not mention one reason why the problems exist and are likely to become worse.¹ Having visited Japan as part of a global survey of primary care, I believe that the basic fault in the Japanese health system is a near total neglect of primary personal care and social care. Since the beginning of this century, and more so since the second world war, the emphasis has been on specialist and hospital services. This has been due to the influence of German medical educational methods before the first world war and American health care philosophies since the second. As a consequence primary care is not recognised as a specialty. There is no special training and no strong professional grouping or political representation.

First contact physicians do exist but are drop outs from hospital specialty training. They set up in the community to provide primary care and endeavour to combine this with their specialist interests. There are 80 000 of them—one per 1500 population—and most work alone. A most unusual custom is that they are allowed to provide clinics as part of their service. These have up to 20 beds, in which they can treat their patients for extra fees: remuneration is by fees for services. These services, however, are not coordinated with community needs and facilities. Home visiting is rare, and primary health teams do not exist. In fact, there is competition to provide primary care between hospital emergency rooms and specialists, to whom Japanese people have free access.

To prevent the crisis to which Okamoto refers a reconstruction of the whole health system is urgently required, in which primary medical and community services are properly planned and funded.

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- 1 Okamoto Y. Health care for the elderly in Japan: medicine and welfare in an aging society facing a crisis in long term care. *BMJ* 1992;305:403-5. (15 August.)

The death penalty

EDITOR,—In his editorial on the death penalty Richard Bonnie drew attention to the paradox faced by some of our medical colleagues, a paradox many of us choose to ignore—due in part, I suspect, to the disturbing questions, the uncomfortable facts, and lack of easy solutions posed by the problem.¹ The problem is, of course, that of medical participation, medical collaboration, or medical acquiescence in punishment (physical, mental, social, legally sanctioned or otherwise) and torture, both of which may at the extremes result in death.

It is time we looked again at the Hippocratic oath of "first do no harm." As stated in the editorial, "doctors hold their medical knowledge in trust for the collective wellbeing of mankind and they must

guard against the exploitation of that knowledge in ways that compromise the supreme aspiration of the profession." Where there is a conflict does harm refer to the long term or short term, to the individual or to the community? The monitoring of the degree of isolation which an individual can withstand means that the individual concerned has at least some safeguards. However, does such monitoring perpetuate a dehumanising punishment; does it impair the development of better or worse forms?

In the absence of crime it is the doctor who decides whether a person is mentally ill, mentally handicapped, in need of treatment, etc. In the event of crime the ultimate decisionmaker in any contested case as to whether a person is competent to be executed is the judge, not the doctor. I feel that Bonnie is drawing a fine line when he seems to differ from the working party that claimed that determining competence for execution is necessarily equivalent to medical permission. It certainly is a major step in determining whether the ultimate punishment of the death penalty will be applied. As for treatment of the incompetent prisoner or patient, surely he or she should be dealt with as are other incompetent patients. A prisoner remains a person with rights.

"The dignity of the condemned should not be subordinated to the dignity of the medical profession." Nor should the dignity of the medical profession be used to legitimise or maintain dehumanising practices, some of which are in current use; practices which many of us ignore. It is time for all in the medical profession and in the media in general to acknowledge how medicine and medics are used in punishment and torture practices and perhaps to campaign actively to abolish the word "punishment" and refocus on the word "rehabilitation." Unless and until we reclaim and redeclare "first do no harm" we will as a profession—be it by default, neglect, or reluctance to enter into and maintain the needed debate until alternatives are found—continue to acquiesce to the practice of "minimise the harm."

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- 1 Bonnie RJ. The death penalty. *BMJ* 1992;305:381-2. (15 August.)

EDITOR,—Richard J Bonnie, a professor of law, contends that doctors should participate in capital cases—those in which a guilty verdict may result in the defendant being judicially executed.¹ A doctor's evidence may promote a guilty verdict, for instance, by excluding a mental disorder or from the presentation of forensic medical evidence.

But if a doctor's professional testimony contributes to a guilty verdict for which the sentence is execution then, in effect, that doctor's professional expertise has become one of the tools used to kill someone. It is difficult to see any real distinction between that and the circumstance Bonnie rightly condemns, where a doctor is "serving as an agent of the state's punitive apparatus" if he decides whether or not a person is fit for a punishment or directs how a punishment should be administered. The inconsistency of the professor's position is that in the former instance he exhorts doctors to influence verdicts which happen to carry the death penalty and in the latter he condemns medical influence facilitating the predictable consequences of the same guilty verdict. The niceties of the distinction are likely to be lost on the person being executed.

A recent infamous case illustrates the point. Judith Ward was found guilty of carrying out IRA bombings, in part, as a result of the psychiatric report. The widespread disapprobrium heaped on the psychiatrist's head was excited not so much by Miss Ward being found guilty of a crime she did