insurance is relevant to other health care workers in this situation.[1] I was interested to read that a few companies would treat him as a special case if he subsequently acquired HIV infection in the course of his duties. The onus of proving causation would, however, be on him.

The NHS injury benefit scheme would also compensate infected workers for loss of earning ability.[2] Again, however, it is stated that "work related infection would have to be established." To do this "a record of a specific injury and evidence of seroconversion are not regarded as essential but would be helpful in proving causation."

Astbury and Baxter found that only 18% of such injuries were reported on an accident form and 5% were notified to the occupational health service.[3] Thus in most cases the health care worker is unlikely to have either an official record of the injury or evidence of seroconversion after exposure. Moreover, it has been pointed out that HIV infection acquired occupationally is not a prescribed disease, unlike hepatitis B,[4] which seems to be an anomalous state of affairs.

What advice, then, should we give injured health care workers? Certainly, they should report the injury on an accident form, and to the occupational health department. HIV antibody testing would be considered only after careful counselling. I have argued that testing in these circumstances should not affect the workers' ability to obtain life insurance cover at normal rates.[5] A record of the injury and documentation of subsequent seroconversion if it occurs would allow the workers to obtain injury benefits more easily and give them a better case should they wish to obtain compensation from their employer through a civil claim.

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1 Gibbons P. Life insurance and HIV antibody testing. *BMJ* 1992;**305**:1093. (31 October.)
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EDITOR,—Simon Barton and Peter Roth's editorial on life insurance and HIV antibody testing[1] highlights a problem that has adversely affected the acceptance of screening for HIV infection. Yet should we be so surprised by the requirements of the insurers, who, like the reformed NHS, are running businesses not charitable societies?

Current practice is to screen patients only with their informed consent after pretest counselling. Part of this procedure is an attempt to quantify the patient's risk on the basis of reported behaviour. Few people attending a genitourinary medicine clinic are likely to be at no risk, but some might believe that their risk is too low to justify future problems with insurance or mortgage.

Perhaps we should try to understand the insurance companies' point of view. At present a positive test result signifies death within a decade for most patients, but a survey in Riverside Health Authority showed that more than a quarter of patients would not divulge their HIV status to an insurance company.[2] Testing by consent leads to the conclusion that acceptance implies a degree of increased risk. In these circumstances is a request for further information not to be expected?

From the public health point of view, the situation is even less satisfactory. What incentive is there for a person at high risk to be tested? For a person, ignorance of infection may be preferable to the modest extension of lifespan afforded by early intervention; for the community, however, this view begs the question of transmission of HIV by those unaware of their status. All this serves to frustrate monitoring of prevalence and control of HIV infection.

Anonymised unlinked testing could solve this problem, but only if it was done routinely on all blood collected for other purposes and was not subject to the informed consent of each patient. I think it a pity that the Department of Health has not exercised greater authority in promoting this approach, which most responsible bodies consider to be justified and ethical. Without compromising the autonomy of the individual person, universal anonymised testing could enable actuaries to take into account the overall risks among different sections of the population and enable insurance companies to be less intrusive to those who take a responsible approach to their health in line with the objectives of *The Health of the Nation.*

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1 Barton S, Roth P. Life insurance and HIV antibody testing. *BMJ* 1992;**305**:902-3. (17 October.)
2 Hulme N, Smith S, Barton SE. Insurance and HIV antibody testing. *Lancet* 1992;**339**:682-3.

EDITOR,—We cannot allow Paul Gibbons's curious concept of life insurance to go unchallenged.[1] He alleges that a claim would not be paid if his death was related to AIDS. There would be little point in having a life insurance policy if certain modes of death were excluded, and it would be unusual for a bank or building society to accept such a policy to protect a loan. Provided the questions on the proposal form are answered honestly and the proposal for life insurance is accepted, the agreed benefit will be paid on death, whatever the cause.

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Decreasing quality of semen

EDITOR,—Time and again supposed evidence for a decrease of semen quality is reported. Carlsen and colleagues presented results of a skilful review of publications on semen quality in men without a history of infertility.[1] On the basis of statistical analysis of data published in 61 studies from 1930 to 1990 they concluded that semen quality has declined during the recent 50 years. They took into consideration that environmental factors might be responsible for both the decrease of semen quality and the increase in occurrence of some genitourinary abnormalities.

We reanalysed data from 48 studies published since 1970 by using the SPSS statistical package and found quite different results. Regression analysis weighted by number of subjects in each study revealed a significant increase of sperm concentration over the past two decades ($B=0{\cdot}38\times10^6$/ml, SE=0·02, $p<0{\cdot}0001$). R^2 is an alternate estimate of how well the data fits the population. In the above regression model the proportion of the variation in sperm density explained by the model is rather small ($R^2=0{\cdot}01$), so that this relation, although significant, should not be overestimated.

We conclude from our calculation that the decrease of sperm concentration observed is not a continuous development, at least not for the past two decades. As only few data are available for the period 1950-70 linear regression is not a useful model to describe the time related decrease of sperm concentration. There is no doubt that the historical values of mean sperm concentration between 1938 and 1969 are significantly higher than those found between 1970 and 1990, but from a statistical point of view there is little reason to claim a linear development and care should be taken when discussing a causal relation with environmental factors.

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1 Carlsen E, Giwercman A, Keiding N, Skakkebæk NE. Evidence for decreasing quality of semen during past 50 years. *BMJ* 1992;**305**:609-13. (12 September.)

Managing transient ischaemic attack and ischaemic stroke

EDITOR,—Martin M Brown and Peter R D Humphrey's comment concerning the lack of suitable patients referred by general practitioners for further investigation of transient ischaemic attack is particularly important.[1] We recently carried out a postal survey of all 122 general practitioners in one district health authority about their attitudes to carotid endarterectomy; we received 102 replies (84%).

Although 61 of the general practitioners thought that carotid endarterectomy was of benefit in selected patients, the remainder either felt unable to give an opinion (36) or thought the procedure unacceptably hazardous (five). The results of the recent international trials had been noted by only 21 of the general practitioners.

The age of the patient was the commonest deterrent to referral (40 respondents) despite the authors' assertion that this should not be a definite barrier. In our sample the general practitioners referred for further investigation only a third of patients whose symptoms they attributed to carotid disease.

Our survey confirms the suspicion that many patients in the community will benefit from investigation and carotid endarterectomy only if efforts are made to increase awareness among general practitioners.

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1 Brown MM, Humphry PRD. Carotid endarterectomy: recommendations for management of transient ischaemic attack and stroke. *BMJ* 1992;**305**:1071-4. (31 October.)

EDITOR,—After reading Martin M Brown and Peter R D Humphrey's recommendations for the management of transient ischaemic attack and ischaemic stroke[1] I calculated the implications for my district health authority of 140 000 patients and 70 full time general practitioners. About 250 patients have a stroke each year, of whom 100 die within one month, 90 remain disabled, and 60 make a good recovery. A further 60 patients suffer a transient ischaemic attack. Of the 120 patients who make a good recovery or have a transient ischaemic attack, 80 are younger than 75 and would require neurological referral for duplex scanning; this would show that 13 had a

carotid stenosis of greater than 70% requiring endarterectomy. Of these 13 patients, 1·66 would have a stroke in the next three years, compared with 2·86 if no operations were performed—an annual saving of 1·2 strokes.

Though such treatment is of paramount importance to those patients spared a stroke, in relative terms the need for high quality acute care and subsequent rehabilitation remains of prime importance to the 249 other patients who will continue to suffer strokes each year.

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EDITOR,—We agree with Martin M Brown and Peter R D Humphrey that duplex scanning should be available to all patients with a transient ischaemic attack or stroke in the carotid territory.[1] We take issue, however, with the statement that carotid angiography should necessarily be performed in patients with moderate or severe stenoses on duplex scanning.

The authors report that if carotid endarterectomy was performed on all appropriate patients 500 major strokes might be prevented each year. Given the risks of carotid angiography,[2] however, if all eligible patients underwent this procedure 50 severe and 150 mild strokes might be provoked.

Duplex scanning has been reported as sufficiently accurate for assessing patients considered for carotid surgery,[3-5] although in cases of suspected subtotal occlusion we agree that angiography may still be required. We believe that, except in these cases, duplex scanning alone will suffice. Not only is it cheaper and safer but it allows characterisation of plaque morphology, thus identifying higher risk subgroups of patients.[6] We propose that all centres should have access to duplex scanners with experienced vascular technicians and see this as a future trend requiring appropriate funding.

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Outcome of breech delivery at term

EDITOR,—In their letter[1] Gary Thorpe-Beeston and colleagues make a robust but incomplete defence of their paper[2] without re-examining the data that correspondents query.[3] In particular, what were the detailed causes of the nine deaths? They also do not indicate whether the completeness and accuracy of the St Mary's maternity information system have been validated.

Obstetric colleagues have expressed legitimate and serious doubt about the validity of the conclusion of their paper: that a retrospectively determined perinatal loss with vaginal delivery approaching 1% should be quoted prospectively in an antenatal clinic to women with a breech presentation at term. The paper may be an important landmark but, alternatively, may be fatally flawed (for example, if the deaths occurred in undiagnosed breech presentations). We have not been given the extra detail needed to judge. It is not adequate for the authors to reply that their database system "cannot provide the fine detail necessary." It should not be difficult to review nine sets of notes. The authors should answer the correspondents' questions.

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1 Thorpe-Beeston G, Banfield P, Saunders N. Outcome of breech delivery at term. *BMJ* 1992;**305**:1092. (31 October.)
2 Thorpe-Beeston JG, Banfield PJ, Saunders NJStG. Outcome of breech delivery at term. *BMJ* 1992;**305**:746-7. (26 September.)
3 Correspondence. Outcome of breech delivery at term. *BMJ* 1992;**305**:1090-2. (31 October.)

EDITOR,—J G Thorpe-Beeston and colleagues' observations on the apparent influence of the method of delivering infants presenting by the breech at term[1] prompted us to look at the outcome for singleton infants presenting by the breech at term (>37 weeks' gestation) in our unit from January 1981 to December 1990. The table gives the results together with the outcome for comparable pregnancies with a non-breech presentation; during this time the annual number of confinements in the unit ranged between 5700 and 6700.

Elective caesarean sections for breech presentation were performed for 36% of both primiparous and multiparous women (395/1085 and 310/861 respectively), but caesarean sections during labour were performed for 30% of primiparous women (324/1084) but only 13% of multiparous women (112/861). We considered it appropriate to analyse overall fetal and neonatal outcome according to whether delivery was by caesarean section before labour or labour and vaginal delivery were seemingly intended.

Our results differ from those of Thorpe-Beeston and colleagues. The proportion of babies with a low Apgar score (<7 at five minutes) was lower, intubation rates were higher, and there were lower admission rates to the special care baby unit. We found little difference in these outcome measures between babies delivered by elective caesarean section and those delivered vaginally or by caesarean section during labour.

The perinatal death rate (excluding antepartum deaths and those associated with congenital anomaly) associated with breech presentation in our unit (0·36%) was similar to that reported by Thorpe-Beeston and colleagues (0·26%) despite our lower use of caesarean section (58% *v* 72%). However, we found little difference in the perinatal death rate between those born after the onset of labour (0·32%) and those born by caesarean section before labour (0·42%). The perinatal death rate for non-breech presentation was identical with that quoted by Thorpe-Beeston and colleagues (0·08%). Our results thus suggest that the outcome is less favourable in breech presentation than cephalic presentation irrespective of the mode of delivery and that some unrecognised lethal factor may be associated with breech presentation.

Considerable caution should be used when examining results obtained from the small numbers in our audit and that of Thorpe-Beeston and colleagues. Prospective randomised treatment is recognised to be the best way of assessing the relative merits of two different management strategies. Although caesarean section may seem a safer option for the baby, little evidence supports that conclusion and it should not be promoted at the expense of maternal health.[2]

The figures presented were provided by the Oxford obstetric data system. We thank the staff who collected and verified the data.

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1 Thorpe-Beeston JG, Banfield PJ, Saunders NJS. Outcome of breech delivery at term. *BMJ* 1992;**305**:746-7. (26 September.)
2 Department of Health, Welsh Office, Scottish Home and Health Department, Department of Health and Social Security of Northern Ireland. *Report on confidential enquiries into maternal deaths in the United Kingdom 1985-1987.* London: HMSO, 1991:119-27.

EDITOR,—Among the several responses to J G Thorpe-Beeston and colleagues' paper[1] two correspondents question the accuracy of the database analysed.[2,3] As part of a wider study of the validity of routinely collected clinical data CASPE Research recently assessed the accuracy of the North West Thames database, which Thorpe-Beeston and colleagues used (a report is in preparation). A qualified midwife received almost 900 sets of case notes, sampled from three units. For each case 17 data items, covering both quantitative and categorical elements, were recorded. The resulting abstracts were compared with matched records drawn from the North West Thames database. A matched record was obtainable for every case, and only one field at one unit was completed with a frequency of less than 99%.

The level of agreement between the computer record and the midwife reviewer was generally high. Two thirds of the items showed agreement of over 95%, with only two items falling below 80% agreement. Where substantial disagreement was found the pattern of errors indicated clear, and rectifiable, problems of definition. As might be expected, simple categorical fields were the best recorded. Of particular relevance to the study of Thorpe-Beeston and colleagues are the agreement rates of 99·7% and 100% obtained for mode of delivery and stillbirth, respectively.

Outcome for pregnancies with breech and non-breech presentations in one unit, 1981-90. Figures are numbers (percentages)

	Vaginal delivery	LSCS during labour	All intended vaginal deliveries	LSCS before labour	All deliveries
			Breech presentation		
No of babies	805	436	1241	705	1946
Apgar score <7	21 (2·6)	7 (1·6)	28 (2·3)	8 (1·1)	36 (1·8)
Intubation	113 (14·0)	99 (22·7)	212 (17·1)	58 (8·2)	270 (13·9)
Admission to special care baby unit	33 (4·1)	30 (6·9)	63 (5·1)	46 (6·5)	109 (5·6)
Perinatal death	4 (0·5)	0	4 (0·32)	3 (0·43)	7 (0·36)
			Non-breech presentation		
No of babies	48 512	2218	50 730	1935	52 665
Apgar score <7	385 (0·8)	74 (3·3)	459 (0·9)	35 (1·8)	494 (0·9)
Intubation	2265 (4·7)	506 (22·8)	2771 (5·5)	140 (7·2)	2911 (5·5)
Admission to special care baby unit	1416 (2·9)	232 (10·5)	1648 (3·2)	200 (10·3)	1847 (3·5)
Perinatal death	34 (0·07)	4 (0·18)	38 (0·07)	7 (0·36)	45 (0·09)

LSCS=Lower segment caesarean section.