False positive findings of mammography will have psychological consequences

EDITOR,—We applaud E Lidbrink and colleagues' attempts to quantify the substantial disadvantages of false positive findings in patients invited to attend for breast screening.¹ Clearly, a financial cost of over £250 000 from a single round of screening at a well organised centre in Stockholm represents a huge sum that could have been better spent, though to us the more important feature of the report relates to the adverse psychological events. The authors state that they did not study the "possible adverse psychological consequences of the false positive mammograms in these women," but experience from daily life teaches us that nothing is so corrosive and enervating as a persisting uncertainty about the possibility of serious illness or upset. After six months only two thirds of the women (219/342) had been declared free of cancer, so one third must have spent even longer under the sword of Damocles.

Many of the malignant lesions proved ultimately to be non-invasive—23% and 29% in the first and second rounds respectively. Since we do not know that identification and early treatment of these tumours lead to a worthwhile survival benefit, the true value of the screening must have been correspondingly reduced.

Finally, we are forced to wonder whether some of the centres that pride themselves on having low ratios of benign to malignant findings on biopsy in Britain might be those with the worst record for prolonging the period before declaring patients free of cancer so that they increase their specificity ranking.

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1 Lidbrink E, Elfving J, Frisell J, Jonsson E. Neglected aspects of false positive findings of mammography in breast cancer screening: analysis of false positive cases from the Stockholm trial. *BMJ* 1996;312:273-6. (3 February.)

Seasonal variation in deep vein thrombosis

Fatal pulmonary embolism is increased in both autumn and winter

EDITOR,—Henri Bounameaux and colleagues state that "two previous reports...suggested an increased incidence in winter of fatal pulmonary embolism...but the conclusions were derived from small numbers."¹ In fact, however, there have been at least 23 reports,²⁻⁵ comprising nearly 11 000 cases.

Figure 1 shows the data from these reports. In all four sets of series more cases occurred in one or both of the quarters October to December and January to March than in either of the other quarters. In addition, however, there were in total as many cases in October to December (2893) as in the mostly colder months of January to March (2898), and in July to September (2646) as in the mostly less warm months of April to June (2651).

If these results were found to recur then the role of temperature might prove to be indirect.

Fig 1—Number of cases of fatal pulmonary embolism in various series by quarter of occurrence, standardised to 31 day month

One possibility is that in autumn the steep but irregular fall in temperature interacts adversely, conceivably through melatonin, with the irregular fall in the hours of sunlight and the steady fall in the length of daylight.

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Daily measurements of temperature should have been used

EDITOR,—Although Henri Bounameaux and colleagues admit that their study has limitations, they do not mention the potential problems associated with grouping events into monthly or arbitrary seasonal periods.¹ The months of the year are not of equal length, and wide variations in weather conditions may occur within any given month; this is a particular problem in Britain.

Studies that have shown a relation between ambient temperature and, for example, risk factors for ischaemic heart disease² or epistaxis³ have used daily recordings of temperature. If Bounameaux and colleagues had access to similar meteorological data for Geneva it would be interesting to know whether the negative findings of their present study were confirmed.

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Author's reply

EDITOR,—T M Allan and A S Douglas's additional data on the increased risk of fatal pulmonary embolism in the cold months are interesting. As we stated in our paper, however, these data do not necessarily contradict our observation of a lack of seasonal variation in deep vein thrombosis, the source of pulmonary embolism. Indeed, comorbidities related to the winter might contribute to a reduced tolerance to small emboli. Alun Tomkinson questions our crude grouping of events by monthly or arbitrary seasonal periods, though this makes sense in a study that aimed at assessing monthly and seasonal variations. Admittedly, our data do not exclude the possibility of daily variations in the occurrence of deep vein thrombosis related to changes in ambient temperature (or barometric pressure or humidity). Since we had no access to these meteorological data, we are unable to study this interesting hypothesis.

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Stridor in rheumatoid arthritis may be caused by laryngeal amyloidosis

EDITOR,—I agree with D H Bossingham and F G Simpson that the investigation of dyspnoea and stridor (even when intermittent) in a patient with rheumatoid arthritis should routinely include laryngoscopy.¹ The authors state that cricoarytenoid fixation may cause such symptoms, but in addition stridor may be caused or compounded by laryngeal amyloidosis, which can occur as a complication of rheumatoid arthritis. The surgical resection of troublesome foci of laryngeal amyloidosis may be warranted in selected cases.²

Such symptoms also occur in other connective tissue disorders, and I have one patient with severe ankylosing spondylitis and mild stridor that is due to unilateral cricoarytenoid fixation and supraglottic amyloid.

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Waist circumference remains useful predictor of coronary heart disease

EDITOR,—Margaret Ashwell and colleagues¹ have produced evidence from the 1992 health survey for England to support our suggestion that waist circumference is a good predictor of risk of cardiovascular disease.² We are puzzled, however, by their suggestion that the ratio of waist circumference to height may be a better predictor of risk than waist circumference alone. In fact, after adjustment for age their data show that correlations of the waist circumference with the calculated risk of coronary heart disease are, if anything, slightly higher than correlations of the ratio of the waist circumference to height with the calculated risk.

Table 1—Correlation coefficients between height and waist circumference in subjects identified by waist circumference action level 1 (94 cm in men and 80 cm in women)

Age (years)	Men				Women			
	Waist <94 cm		Waist ≥94 cm		Waist <80 cm		Waist ≥80 cm	
	No	r	No	r	No	r	No	r
20-	285	0.198**	48	-0.001	363	0.123*	85	-0.125
30-	386	0.202***	158	-0.043	442	0.153**	202	-0.038
40-	374	0.138**	298	0.092	417	0.120*	400	0.032
50-59	267	0.157*	367	0.120*	259	0.102	529	-0.022
All ages	1312	0.053	871	0.049	1481	0.056*	1217	-0.040
All ages after adjustment for age	1312	0.192***	871	0.080*	1481	0.131***	1217	-0.018

*P<0.05, **P<0.01, ***P<0.001.

Intuitively, it makes sense to correct associations with waist circumferences for height. Taller people would be expected to have larger waist circumferences than shorter people. We have therefore analysed the relation between height and waist circumference in our sample of 2183 men and 2698 women aged 20-59 according to their "waist action level" as previously proposed.² Table 1 shows that there are weak but significant associations between height and waist circumference in men and women with waist circumferences below the action level. The proportion of variance in the waist circumference explained by height in those with a waist circumference above the action level was 0.64% in men and 0.03% in women.

We therefore conclude that height does not have an important role in explaining variations in the waist circumference, especially in people who need weight management. This and the simplicity of using the waist circumference argue against the use of more complex measures such as the ratio of waist circumference to height, particularly in a public health or health promotion context.

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Shackling prisoners in hospital

Is unnecessary and avoidable

EDITOR,—Luisa Dillner's editorial and Beverley Beech's personal view, giving an account of the effects of shackling a pregnant woman from Holloway Prison, sickened me.

From 1962 to 1982 I was by the invitation of a succession of senior medical officers and governors of Holloway Prison the consultant to the prison for gynaecology and obstetrics.

During those 20 years about 800 women prisoners were delivered in the City of London Maternity Hospital and later the Whittington Hospital, in beds for which I had clinical responsibility. We lost only one baby—a remarkable perinatal mortality considering that the mothers were so disadvantaged. Much valuable research work was carried

out on the gynaecological cases, particularly on cancer and precancerous conditions.

Special attention was paid to avoiding prisoners on remand escaping—because the judiciary would rightly object if the prisoner was not available for trial at the correct time. Shackling was never contemplated. Women on remand were accompanied by two female warders, and violent women were very rarely accompanied by a male warder as well. It was sometimes a help to a prisoner in labour to have a sympathetic female warder with her. If she objected, or the warder was disturbing her, the warder was asked to wait outside the labour ward with her colleague.

When I retired I tried to persuade one of my colleagues at the Whittington to take over what I had been doing, which included conducting a weekly antenatal clinic in the prison where I saw, at the medical officers' request, especially worrying cases and each pregnant woman every month or so—only two or three at each visit. These women went to hospital only for special tests such as ultrasonography. If one consultant at the Whittington Hospital would take responsibility for these prisoners' care, much journeying with warders could be avoided and much money saved, especially if the consultant would go fairly regularly to Holloway.

Most women are in prison for crimes that do not pose a serious threat to society. The unknown ambulance driver who refused to drive a prisoner in shackles is the true hero of the story and gives a splendid example to the doctors and nursing staff at the Whittington Hospital. They are not stooges of the prison service; and never should be. Above all doctors should and can refuse to allow shackled patients into the hospital, and particularly into the labour wards.

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1 Diller L. Shackling prisoners in hospital. *BMJ* 1995;312:200. (27 January.)

2 Beech BAL. How could I breast feed with a man in the room? *BMJ* 1995;312:256. (27 January.)

Is a breach of human rights

EDITOR,—Luisa Dillner's proposed scheme for managing prisoners in hospital¹ can work well, at any rate for outpatients, given a clear policy by clinicians and enlightened prison staff. Before my retirement in 1993 from Ashford Hospital, Middlesex, I regularly saw detainees from the nearby Feltham Youth Custody Centre. After an episode when warders insisted on sitting in on both history and physical examination of a patient, I wrote to the principal medical officer, copied to the governor, that (as implied by the subtitle of the editorial: "contravenes international law") this was a breach of human rights and I would see referrals only if they were permitted exactly the same degree of confidentiality

that the custodians themselves would expect. As Dillner also notes, I accepted that exceptions might have to be made if there was thought to be a significant risk of escape (possible only by jumping through a double-glazed window) or violence to hospital staff.

With only the occasional quiet word to accompanying warders, this arrangement worked uneventfully for several years; the let out clause was never invoked, and no violence or escapes were attempted. As Dillner suggests, most prisoners can indeed be treated as presenting no threat.

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Causes problems for doctors

EDITOR,—Until I read Luisa Dillner's article on shackling prisoners in hospital¹ I was not aware that doctors have the right to request the removal of restraints for the purpose of treatment, and I wonder whether prison officers are aware of this.

At this hospital we often see and treat prisoners from the nearby prisons in Durham; recently one of them was a man with small cell lung cancer. I requested that I should be allowed to talk to him in private about his condition with his two warders outside the door (they were aware that he had cancer from what had already been said). This was refused. I insisted that they should ring up the duty governor for permission. They did this unwillingly because they felt it to be pointless; it was. I suggested that the patient should be chained to the examination couch so that I could talk to him alone but was told this would not be allowed under any circumstances. The best I could achieve for this man was that his short chain was replaced by a longer one so that the warders could wait outside an open door.

What can a doctor do in this situation? What is the legal position?

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1 Diller L. Shackling prisoners in hospital. *BMJ* 1995;312:200. (27 January.)

Nocturia may be due to growing old

EDITOR,—Minerva says that the most common reason why elderly men get up at night to urinate is benign prostatic enlargement, and she refers to sleep apnoea.¹ I hope she will not find me wanting in chivalry if I question why, after so many centuries, she seems not to be aware of another cause—that is, growing old. Older women have the same incidence of nocturia as men.² While nocturia may result from enlargement of the prostate, more often it results from an inability to concentrate urine at night, when old people secrete less pituitary antidiuretic hormone and more natriuretic peptide.³ A simple record of the time of urination and volume voided over 24 hours may avert a needless operation on the prostate.⁴

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