

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.

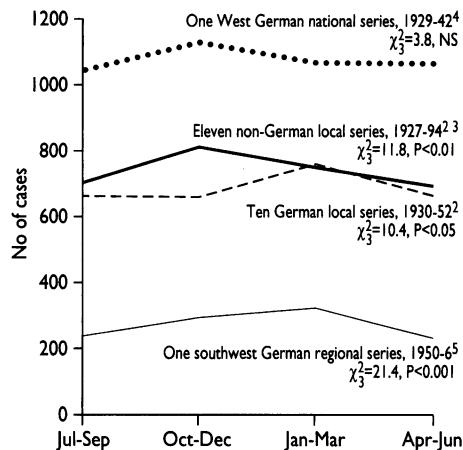


Fig 1—Number of cases of fatal pulmonary embolism in various series by quarter of occurrence, standardized to 31 day month

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.