Commentary

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Two Sides of the Same Coin?

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One purpose of publication is to correct erroneous or confusing information [1]. Sometimes it is difficult from a survey of the literature to find a consensus view. For example there are those who support the concept of screening for early detection of breast cancer and those who are vehemently opposed to it.

Nystrom et al. [2], in an overview of breast cancer screening data derived from five Swedish trials, report a maximum reduction in mortality from breast cancer of 29% in women invited for screening and aged 50-69 years at randomization.

Skrabanek [3], commenting upon the same paper concludes: 'although breast cancer mortality in four of five trials was not significantly reduced by mammography, one breast cancer death in very 12,000 women invited for screening yearly would be prevented or postponed'. He finds that the reduction is less than 1 % of all deaths, and questions the 'wisdom of offering mammography on a mass scale'. Additional information supplied by Nystrom emerged in Skrabanek's article, namely that total mortality (from all causes) in the invited and control groups was 15,695 and 11,887, respectively. On the basis of these figures, indicating that more women die in the screened group than in the controls (even after normalization for the size of the two groups), Skrabanek concluded that 'in this sense not one life was saved'. Any reader following the breast screening debate could be forgiven for failing to reconcile these points of view [1,2], and doctors might have genuine difficulty in advising women whether or not they should accept an invitation to be screened.

We attempt here to resolve the apparent conflict. Since the sizes of the control and screened groups are not identical, the difference in mortality from breast cancer must be calculated by normalizing the number of deaths to groups of equal size. There were 418 deaths in the invited group and 425 in the control group due to breast cancer. Normalization leads to the prediction of a mortality reduction of either 112 among 156,911 women or 90 among 125,866. This is equivalent to 1 less death among 1,400 women during the study period. To obtain Skrabanek's figure of 1 in 12,000 we must assume that the study period was 8.5 years. The minute benefit per year claimed by Skrabanek then is $1 \text{ in } 1,400 \times 8.5 = 12,000$. This device of presenting benefits on an annual basis makes the screening procedure appear to have an even more marginal effect and it is of doubtful value because the reduction in mortality is delayed by about 5 years from the start of screening. The data also implies a (relative) reduction of 21.2%, i.e. $(112/530 \times 100)$ in deaths from breast cancer. The higher figure of 29% given by Nystrom et

al. [2] arises because a subset of screenees aged 50-69 years is selected. However, in absolute terms, the reduction is 0.7%, or $[(112/15,695) \times 100]$, consistent with Skrabanek's conclusion of less than 1 %. Deaths from breast cancer were only 3.6% of all deaths in the control group and 2.6% in the invited group. Advocates of screening tend to stress relative reductions in mortality and opponents absolute values. The magnitude of the overall reduction in mortality (regarded as a major benefit) is undeniably small in the five screening trials. Skrabanek [4] has pointed out that if there is about one misdiagnosis of the cause of death in 20,000 women screened per year that would be enough to annul the alleged benefit. Another problem of screening sometimes overlooked is that of over-diagnosis. Lesions are detected by mammography which if left alone may never have become life threatening [5, 6]. Also, because of lead time bias, some cancers are detected by screening in women destined to die from other causes before the cancer would have become clinically apparent.

The financial cost of achieving a 0.7% reduction in mortality can be estimated. Taking a figure of \$60 per examination given by Dershaw et al. [7] and assuming 4 examinations on each of 156,911 women, the ball park figure for the cost of the Swedish trials would be \$38 million. The cost per death prevented, or postponed in the five Swedish trials would thus be about \$300,000. This figure does not take account of the costs of accelerated clinical activity consequent upon screening. For the Swedish Two County trial alone a lower figure of \$3,400 per year of life saved has been quoted by Tabar et al. [8]. The outcome of the Swedish Two County trial was a major factor influencing the conclusions of the Forrest Committee [9] and led to their recommendation to introduce a UK breast cancer screening service. The proposals were agreed by the UK government and have been implemented. The capital cost was E50 million. In 1991–1992, the running cost of that programme in which about 1.1 million women were screened, was E24 million [M. Bruce, M.P., private communication]. Therefore, the question that we wish to ask is, bearing in mind the high cost and the small reductions in mortality achieved by screening for breast cancer, would it not be better to redirect the resources to support instead targeted basic research into the fundamental biology and treatment of breast cancer? We would suggest that a group with the expertise of the Forrest Committee, but from a wide range of disciplines, should look at the benefits of spending these funds in a different way. There is a precedent for such a panel with a similar remit. The National Breast Cancer Coalition [10] organized its own research hearings and invited 'top' researchers to come and address the question of whether the field could use more funds. An increase of \$300 million was proposed and \$210 million was approved. Another expert panel will rule on the strategy for spending the money.

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Erratum

In the article by Dashti et al., entitled 'Gram-Negative Septicemia and Trace Element Alterations in Tissue and Plasma: Effects of Superoxide Dismutase and Allopurinol', published in Vol. 3, No. 2, pp. 77-83, 1992-93, reference 14 has an incorrect publication date. The reference should read as follows: Dashti H: Liver injury and liver cirrhosis: An experimental and clinical study. Bulletin No 64, Department of Surgery, Lund University, Lund, 1986.