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# LETTERS TO THE EDITORS

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## SCREENING WITH MAMMOGRAPHY

Dr. Skrabanek's recent article (2) contains serious inaccuracies. In his eagerness to disprove the value of breast cancer screening with mammography, he has misinterpreted some parts of the published work and overlooked others. In particular, his attack on the Swedish Two-County trial must not go unchallenged.

His first complaint is that randomization was by cluster rather than by individual. Randomization by cluster is a respectable technique, but should be reflected in the analysis. A statistical analysis which does not take into account the cluster-randomized design may underestimate the variance of an effect and, hence, overestimate significance. One solution is to model the mortality as negative binomial rather than Poisson (1). In the most recent report on the Two-County study, the relative mortality associated with screening was observed as 0.69 with 95% confidence interval (0.55, 0.88) using the Poisson analysis (5). With the negative binomial analysis, the relative mortality is 0.69 with 95% confidence interval (0.53, 0.91). Thus the effect of screening is still plain after accounting for the cluster randomization.

Skrabanek's second complaint is that "only a subset of the original sample was evaluated by statisticians." Presumably he refers to the fact that women aged 75 years or more were excluded from evaluation. In the first round of screening the response rate in this age group was less than 50%, as compared with 89% aged 40-74 (6). It would have been absurd to include this group as if it had the same qualitative treatment as women aged 40-74. We are happy to concede that a policy of offering screening to women aged 75 years or more is unlikely to be effective.

Third, Skrabanek claims that no lives were saved in the Two-County trial, since there was no decrease in mortality from all causes. The demand for a significant reduction in total mortality is naive for the following reasons. In women aged 40-74 years in Sweden, breast cancer accounts for approximately 7% of deaths. A 30% reduction in breast cancer mortality would therefore incur a 2% reduction in total mortality. At the 8-year follow-up, however, the death rate from breast cancer is only about 4%, since cause-specific death rates change as the population ages. Thus, the expected reduction in total mortality at eight years is 1%, exactly as observed, but not statistically significant. The numerical result may have been published after Skrabanek wrote his article, but we are surprised that he is not familiar with the above elementary facts about cause-specific mortality.

Skrabanek suggests that there was a potential bias in ascertainment of cause of death. Recent work has shown that there is no evidence of such a bias (5).

Skrabanek attributes the phrase "no reduction has yet been observed" (6) in description of mortality in aged 40-49 in the Two-County trial to wishful thinking. A reduction in breast cancer mortality was anticipated as in the HIP study and was subsequently observed (5).

In his discussion of overdiagnosis, Skrabanek enters the realm of pure fiction.

Citing a recent paper (4) on the Two-County trial, he claims that “the study group had an excess of 28% in tumors *larger than 2 cm* (13.1 v. 10.2/1000 women)” (the italics are Dr. Skrabanek’s). In fact, not only is this result absent from the paper cited, almost the opposite result was observed. There was a deficit of 26% in the study group in tumors at Stage II or worse, 5.9/1000 women in the study group, as compared to 8.0 in the control. Skrabanek is entitled to criticize the work of other researchers, but he would be well advised to read it first.

Finally, Skrabanek describes the Malmo trial as “perfect in design and execution.” Readers unfamiliar with this area of research should not conclude that this is established fact. Reservations have been expressed about the Malmo trial, notably the low response rate, the contamination of the control group by treatment and the optimistic power/sample size projections (3). It is not our purpose to attack the Malmo trial here. The point is that no study is perfect, and to categorize a trial as such because its results agree with one’s own opinions is evidence of the wishful thinking which Dr. Skrabanek is so quick to diagnose in others.

LASZLO TABAR  
*Mammography Department*  
*Central Hospital*  
*Falun 79182*  
*Sweden*

STEPHEN W. DUFFY AND NICHOLAS E. DAY  
*MRC Biostatistics Unit*  
*Cambridge CB2 2BW*  
*U.K.*

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### SKRABANEK’S RESPONSE

It is not surprising that Dr. Tabar and his colleagues, in view of their commitment to screening, did not like what I wrote (Skrabanek, P. Mass mammography: The time for reappraisal. *IJTAHC*, 1989, 5, 423–30). However, in their defense, they avoid answering the main points of my criticism: that is, the diminished impact of screening