Letters to the Editor

Misleading quotations and other errors persist in rejoinder on breast cancer screening

From PETER C GØTZSCHE

Sirs—Although I demonstrated that the review by Freedman, Petitti and Robins (FPR) of breast cancer screening[1] contains many errors,[2] they persist.[3]

In their review,[1] FPR noted that 434 more women with breast cancer prior to randomization were excluded from the study group than from the control group in the Health Insurance Plan (HIP) trial, quoting the principal investigator of this trial. However, in their rejoinder,[3] they claim this number originated with me. I wrote that many more women with breast cancer prior to randomization were excluded from the study group than from the control group[2] and FPR contradict themselves when they now say my statement is incorrect.[3] FPR also say that I withdrew a previous 'near-retraction'. There is no 'near-retraction' in the reference FPR citation[4] where I merely restated my concern that retrospective exclusion of women after 18 years of follow-up may not be reliable.

FPR disregard the large discrepancy in the Two-County study involving a benefit of 24% reported by the trialists versus only 10% reported in the Swedish overview.[1] Further, although they consulted extensively with the Two-County trialists, they avoid addressing the many discrepancies in numbers of women and deaths reported for this study.[1] FPR also turn a blind eye to overdiagnosis,[1] though the recent IARC/WHO report on screening acknowledges this is 'an obvious source of harm'.[5] The report notes that there was about a 50% increase in breast cancer during 5 years after introduction of screening in Finland and UK and that the rise may be persisting,[5] in agreement with our findings and those of others.[2]

FPR avoid discussing length bias[2] and do not acknowledge their error in comparing total mortality *among breast cancer cases* in the study group versus the control group and then concluding that a significant difference is evidence that screening is effective. It is well-known that case-survival is a highly misleading outcome,[6] particularly in screening trials.[2,7]

Finally, FPR reiterate that it is appropriate to exclude deaths in women who have been invited to screening but refused to get screened. This is not how trials should be analysed if we wish to avoid bias (www.consort-statement.org).

References

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3 Freedman DA, Petitti DB, Robins JM. Rejoinder. *Int J Epidemiol* 2004;**33:**69–73.

4 Gøtzsche PC. Screening for breast cancer with mammography. Author's reply. *Lancet* 2001;**358:**2167–68.

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6 Welch HG, Schwartz LM, Woloshin S. Do increased 5-year survival rates in prostate cancer indicate better outcomes? *JAMA* 2000;**284:**2053–55.

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In search of the best available version of the truth

From CORNELIA J BAINES

Sirs—In their qualitative assessment of screening trials Freedman *et al.*[1] demonstrate that impressive author affiliations and the peer review process do not guarantee accuracy. On page 50, the reader will find the following persuasive statement about the Canadian National Breast Screening Study (CNBSS). 'Centre radiologists only agreed with the reference radiologist 30–50% of the time.' What a dreadful study that must have been! Quite wrong.

The numbers they cite are clearly reported as kappa statistics, which indicate how much of the agreement observed was agreement beyond that which might occur by chance.[2] In fact, Table 2 (which they disregarded) from our publication reveals

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