UNICEF and the UN Security Council

David Woodward’s Comment “Vote buying in the UN Security Council” (Jan 6, p 12) is based on a Harvard University study that displays a profound lack of understanding of the process by which UNICEF funds programmes on behalf of children and families in the world’s poorest countries. Had Woodward contacted us to respond, we would have explained that UNICEF has very strict funding rules for countries, based on specific indicators of children’s health and wellbeing.

All contributions to UNICEF are divided into two categories. The first category consists of core resources, which are allocated to country programmes in accordance with a board-approved formula based on under-5 mortality rates, gross national income per head, and then absolute size of the child population. These resources are distributed as part of multiyear plans that are developed with host governments and approved by our 36-member Executive Board to ensure the best results for children.

The second category consists of earmarked resources generated for global development issues, such as malaria, polio, or HIV/AIDS, and for emergencies such as the 2004 tsunami.

Most funds contributed by the USA to UNICEF are core funds, and therefore cannot be earmarked for a particular country. None of the earmarked resources is earmarked in any way to whether or not a country has a seat on the UN Security Council. UNICEF has never been a channel through which any country can try to exercise influence over members of the Security Council. Our system of funding for children is as fair, equitable, and honest as our millions of generous supporters expect and deserve.

I declare that I have no conflict of interest.

Alan Court
jsedky@unicef.org
Director, Programme Division, UNICEF, 3 UN Plaza, New York, NY 10017, USA


Mammographic screening from age 40 years

Sue Moss and colleagues (Dec 9, p 2053) ought to be commended for a well balanced presentation of the findings of their breast cancer screening trial of women from age 40 years (the Age trial). However, a critical issue is the clinical relevance of breast cancer risk management of young women at low risk.

More than 1.7 million years of follow-up were not enough to show significance. However, statistical significance only tells us about numbers: it is enough to increase the denominator by a few more millions in a meta-analysis to show that the midpoint estimate of the Age trial is a fair representation of the effect. The 10-year risk of breast cancer death in the control population was 0.2%. The number needed to benefit from screening by saving one life in the next 10 years was 2500 (absolute rate reduction 0.00004 per year). If we compare this with cardiovascular risk management, a 10-year risk of cardiovascular death below 1% is regarded as very low, even among women. Unless being dead of breast cancer is far worse than being dead of stroke or myocardial infarction, there is a major inconsistency.

Cancer screening increases morbidity. The 10-year excess risk of a diagnosis in the intervention group was 0.24% (absolute rate increase 0.00024 per year), a number needed to harm of 420. If earlier diagnosis causes a later decrease in incidence, it will be in old age after stopping screening.

In treatment of eligible early breast cancer patients with trastuzumab, the number needed to treat to prevent disease recurrence within 1 year is 15—ie, orders of magnitude lower. Compared with effective treatment of patients, any cancer screening study will only show over and over again that screening healthy people at low risk is bound to be a poor solution to cancer.

I declare that I have no conflict of interest.

Luc Bonneux
bonneux@nidi.nl
Netherlands Interdisciplinary Demographic Institute (NIDI), PB 11650, 2502 AR Den Haag, Netherlands


Sue Moss and colleagues report a 17% decrease in breast cancer mortality after a mean follow-up of 10.7 years and note that this was consistent with the 3% reduction in all-cause mortality they also found. This result probably reflects biased reporting at a favourable time point.

In 1999, Moss and colleagues stated that the first mortality analysis was to be carried out after 7 years, and in 2005 they reported data after 8.1 years, but curiously, only on all-cause mortality excluding breast cancer. This odd outcome did not favour screening, since there was a mortality increase of 4% in the screened group. I estimated the missing number of breast cancer deaths after 8.1 years from a mortality curve, added them to the other deaths, and found a 1% increase in all-cause mortality with screening. I therefore