LETTERS TO THE EDITOR

RE: "BREAST CANCER AND LACTATION HISTORY IN MEXICAN WOMEN"

We read with interest the enlightening article by Romieu et al. (1) on breastfeeding in relation to risk of breast cancer among women in Mexico City. The widespread practice of long-duration lactation has given these researchers the ability to address questions that could not be easily answered using data from most industrialized countries. We were, however, puzzled by a few points made in the paper, particularly with regard to figure 1 and the conclusions drawn from it.

Romieu et al.'s figure 1 (1, p. 549) plots the probability of being a case in relation to duration of lactation, by menopausal status. From our examination of this plot, we are not convinced of the authors' conclusion regarding the interaction between menopausal status and the protection conferred by lactation. In the text, Romieu et al. state that "this protective effect was more evident for postmenopausal women. Among premenopausal women, a long duration of lactation did not substantially modify the results obtained for lactation up to 3 months in duration (table 4, figure 1)" (1, p. 545). Although the data from the table appear to show a stronger effect of lactation in the postmenopausal group, the trend is relatively strong in both groups, with less stability of estimates in the premenopausal group. Moreover, the fitted lines in figure 1 suggest practically identical protection in the two groups. For example, by 180 months of lactation, the risk appears to have dropped from 0.4 to 0.2 in the postmenopausal women, and from about 0.22 to about 0.11 in the premenopausal women. Thus, the fitted lines reflect a lack of deviation from the multiplicative model; i.e., they appear not to support effect modification by menopausal status. It seems that either these lines were based on a fitted model that did not contain an interaction term, or the model did contain an interaction term that made a negligible contribution to prediction of the risk (or, more accurately, odds) of breast cancer. In any case, we believe it important that the appropriate conclusion be drawn from this very rich data set.

Another concern is that the probabilities of disease that are plotted in the figure are misleading, since the study was case-control in design. Apparently, the authors did not use any correction for the sampling fractions in the study in order to obtain probabilities of disease. Thus, on an absolute scale, the data represented in this figure are conditioned on being in the study sample, and that is why there are such high probabilities (e.g., >40 percent). It would appear that the authors did have reasonable estimates of the sampling fractions and therefore could have provided true absolute risks.

We also wonder whether these two issues are linked: Perhaps the model did show an interaction using logits or odds, but the effect modification disappeared when the data were transformed to the risk scale. While the existence of effect modification on the logit and odds scales but not on the risk scale is possible, it seems rather unlikely for a rare disease. Again, calculation of population risks would help with interpretation.

Another minor point regards the reporting of duration of lactation. Two hundred months of lactation is more than 16 years, and a few women lactated for more than 250 months, or 20 years. While this length of lactation is not physically impossible, we wonder whether the women were asked about duration of lactation for each child. If so, and if a woman breastfed more than one child simultaneously, would those months have been counted multiple times?

Clarification of these points would enhance an otherwise very informative and important contribution to the literature on breastfeeding and breast cancer risk.

REFERENCE


Irva Hertz-Picciotto
Marilyn Tseng
Beth Newman
Jane Schroeder
Marilyn Vine
Department of Epidemiology
School of Public Health
University of North Carolina at Chapel Hill
Chapel Hill, NC 27599–7400

THREE OF THE AUTHORS REPLY

We appreciate the comments made by Hertz-Picciotto et al. (1) in relation to our recent paper (2). However, we feel that Hertz-Picciotto et al. overinterpreted our description of the effect of lactation among premenopausal women.

In the text, we stated that the effect seen was "more evident for postmenopausal women" (2, p. 545). However, we never mentioned an effect that was modified by menopausal status. We did explore this possibility, but the results were not included in the paper. We present here the results of the logistic regression analysis (table 1). The estimated effect of lactation was similar for pre- and postmenopausal women, and the interaction term, when included in the model, was not significant. This analysis suggested that the effect of lactation was similar for pre- and postmenopausal women.

Data for figure 1 in our paper (2, p. 549) were obtained by fitting separate models for pre- and postmenopausal women. We stressed this point because previously published papers had reported a protective effect only for premenopausal women. As for Hertz-Picciotto et al.'s concern about the probabilities of disease plotted in figure 1, we assumed that most readers would be aware of the fact that...