Letters to the Editor


Letter

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We would like to correct an inaccurate summary of our previous findings (1) on the relationship between self-reported soy intake and mammographic densities. In a recent study by Jakes et al. (2), our findings are described as follows: “Women with higher soy intake had lower percentage mammographic densities when compared with women with lower soy intake.” Our findings and conclusions were, in fact, the opposite. Contrary to our hypothesis, we found a significant positive association between self-reported soy intake and percentage of densities ($P_{\text{trend}} = 0.04$). Although it is worth noting that our findings differed by ethnic group and the Chinese/Japanese group had a nonsignificant trend in the opposite direction from the total study population, our results were nonetheless clearly mischaracterized.

We would also like to note three additional questions about the Jakes et al. study (2): (a) making inferences from our findings in support of theirs may be significantly complicated by the differences in mammographic measures used (the Tabar classification in their case, a quantitative method in ours); (b) the quartile boundary values for the main independent variable, soy consumption, are not given, and the raw mean is not given for any of the dietary variables; this leaves the range of intake values at which one might expect protective effects unclear; and (c) the finding that increased fat consumption still provided a protective effect even after adjusting for BMI, and energy intake is puzzling. Although the discussion addresses the inverse relation of BMI and energy intake with mammographic density, it implies that adjusting for BMI and energy intake should remove the confounding effects that make fat consumption appear to have a protective effect. Their results suggest the contrary view, and no explanation is offered for the continued protective effect of fat consumption after adjustment.

References


Reply

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We apologize to Dr. Maskarinec et al. for misinterpreting their work. We had originally intended to say that their study had observed a nonsignificant reduction in dense area and significant increase in percentage of density with higher intakes of soy foods. Unfortunately, while revising it in the light of referees’ reports, one of us (S.W.D.) misread a direction by the editor, and the error crept in.

As Dr. Maskarinec and Williams state, the results for a qualitative classification of breast patterns do not directly correspond to those for a quantitative scale, but Tabar patterns IV and V are typically >50% dense. As regards Dr. Maskarinec’s and Williams’ point about the absolute amounts, the quartile cutoffs for total soy protein intakes in gram/day were 2.9, 4.7, and 7.5. Thus, our “low risk” group has intakes that are relatively high in western terms but not particularly so in a Chinese population.

Contrary to the interpretation of Dr. Maskarinec and Williams, we did not suggest in our study that adjustment for BMI and energy intake would remove the confounding, which made fat intake appear to have a protective effect. The unadjusted effect of fat intake is of borderline significance, which is no longer seen when adjusted for soy intake. Our point about BMI was its negative association with breast density. Because of the negative confounding of BMI and breast density, we submit that the increased risk of breast cancer associated with breast density will be underestimated if not adjusted for BMI. Of course, if high BMI had a causal effect on breast density, which in turn produced the observed effect of high BMI on breast cancer risk, one could not assume the converse, because density would be part of the causal pathway of the effect of BMI on risk. However, since the two factors seem to act in opposite directions, at least in postmenopausal women, it seems likely that the effect of BMI on risk of breast cancer is operating by other pathways than breast density.

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