We read with interest and concern the commentary by Ranchor et al. (1) on a study that found no association between personality and cancer risk and mortality rates (2). Ranchor et al. (1) cited a previous study conducted by our group (3) that analyzed the effect of the interaction between stressful life events and social support on breast cancer risk among 83,334 postmenopausal women enrolled in the Women’s Health Initiative. Our study found no independent association between stressful life events and breast cancer incidence. We reported a marginally significant interaction between stressful life events and social support ($P = 0.07$).

Ranchor et al. (1) used our article as an example of publication bias that favored results that were positive over results that were negative or inconclusive. This was surprising because we clearly described the absence of an association in our analysis, stating, “This study found no independent association between stressful life events and breast cancer risk” (3, p. 137). In fact, a primary conclusion of our article was that results from the Women’s Health Initiative add to the evidence that a direct effect of stress on breast cancer risk is not likely. However, a complete causal mechanism for cancer may well include complex interactions of psychosocial factors. Evaluation of highly complex biologic interactions requires specially designed studies, and we feel uncomfortable using our analysis to rule out the role of stress as a component of a causal mechanism leading to breast cancer.

Although great weight should be given to results from high-quality registry data, such as those from the Finnish and Swedish twin cohorts (2) or a cohort study as well-designed as the Women’s Health Initiative (3), lack of a significant $P$ value merely means that the study lacked evidence to rule out the null, not that it proved the null hypothesis (4). Although Ranchor et al. made some important points in their commentary, it was disappointing to see our work taken out of context.

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REFERENCES


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