Response

Although we agree with most of the observations offered by Olsen et al., we have reservations regarding their interpretation of our clinical trial subgroup results.

Their letter implies that we advocate higher dose vitamin D administration for breast cancer risk reduction. In fact, we stated that “our findings provide some evidence against” a hypothesis that higher dosage would have changed the outcome of the study.

We agree with their suggestion that the association between serum 25-hydroxyvitamin D levels and body mass index and physical activity could be a confounding factor in observational studies relating 25-hydroxyvitamin D and breast cancer incidence.

We suggest caution in the interpretation offered by Olsen et al. of the subgroup analyses included in our report. They suggest that these analyses support a potential harmful effect of high vitamin D intake based on two subgroup analyses: one in the report of findings related to breast cancer and another in a report of findings related to colorectal cancer (1). However, in neither case did these analyses demonstrate a statistically significant interaction among baseline total vitamin D intake, randomization group, and cancer incidence. In addition, in the nested case–control analyses, no statistically significant interactions were observed between serum 25-hydroxyvitamin D levels, randomization group, and incidence of either breast cancer or colorectal cancer, even among women with the highest 25-hydroxyvitamin D levels evaluated (1). Taken together, these findings provide no reliable evidence regarding safety concerns for vitamin D dosage used in our trial and breast or colorectal cancer risk.
Although there may be reasons to continue to explore the potential relationship between total vitamin D intake and breast cancer risk, we concluded in our report that “current evidence does not support” calcium plus vitamin D supplement use in “any dose to reduce breast cancer risk.”

ROWAN T. CHLEBOWSKI
JEAN WACTAWSKI-WENDE
CHARLES KOOPERBERG
GARNET L. ANDERSON

Reference

Notes
Affiliations of authors: Los Angeles Biomedical Research Institute at Harbor-UCLA Medical Center, Torrance, CA (RTC); Department of Social and Preventive Medicine, The State University of New York, Buffalo, NY (JW-W); Division of Public Health Sciences, Fred Hutchinson Cancer Research Center, Seattle, WA (CK, GLA).

Correspondence to: Rowan T. Chlebowski, MD, PhD, Los Angeles Biomedical Research Institute at Harbor-UCLA Medical Center, Department of Medical Oncology and Hematology, 1124 W Carson St, Bldg J-3, Torrance, CA 90502-2064 (e-mail: rchlebowski@gmail.com).

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