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THE AUTHORS REPLY

Responding to our commentary on personality as a risk factor for cancer (1), Michael et al. (2) suggested that we unfairly cited their article (3) as an example of the kind of confirmatory bias permeating the literature relating psychosocial factors to onset, progression, and outcome of cancer. We welcome the opportunity to clarify our concerns about their article and why we chose to cite it in our commentary.

In the abstract of their original article, Michael et al. (3) claim that 1 stressful life event was associated with having an increased risk of breast cancer, but that having >1 event decreased the risk of breast cancer. This complex association is unprecedented in the literature and unlikely to be valid, particularly given the crudity of their 11-item measure of life stress that gave equal weight to death of a spouse and death of a pet. Moreover, the abstract further claimed that women who have had stressful life events and a lack of social support had a decreased risk of breast cancer. Again, this claim would not have been anticipated from the existing literature, and it was contradicted by the authors' own Table 3, which indicated a lack of interaction between stressful life events and social support. Essentially, Michael et al. obtained no readily interpretable positive findings in support of a role for stress-related factors in risk for cancer.

If these authors had simply reported these results in a straightforward fashion, we would not have cited their article (3) in our commentary. However, the authors' present letter (2) explains that they were "uncomfortable using [their] analysis to rule out the role of stress as a component of a causal mechanism leading to breast cancer." Consistent with this reluctance, they frame their study in terms of a speculative theoretical framework (4) that they claim is supported by a "significant body of human. . .research" (p. 143). This "significant body" depends heavily on the discredited work of Grossarth-Maticek et al. (5) concerning

personality as a risk factor for cancer that Michael et al. (3) also cite directly. We encourage readers to take a look at the article by Grossarth-Maticek et al (5). As reviewed in a special issue of *Psychological Inquiry* (e.g., 6–8), the research was widely condemned as poor epidemiologic practice and implausible in its exceptionally strong claims of high recruitment and retention rates, precise matching of patients and controls, and suspiciously strong strength of results, unmatched in the literature before or since.

Undoubtedly, it will take time for the essentially null associations in the high-quality data of Nakaya et al. (9) to overcome the strong confirmatory bias in interpreting poor quality data as support for the belief that personality and stress-related factors pose significant risk for the development of cancer. However, more immediately, authors should cease to cite the data of Grossarth-Maticek et al (5), without explicit indication that these data have been discredited.

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