Letter to the Editor

Clinical Cancer Research

Psychologic Intervention and Survival: Wishing Does Not Make It So – Letter

Steven C. Palmer[1], Michael E. Stefanek[2], Brett D. Thombs[3], and James C. Coyne[1]

Andersen et al. (1) claimed that breast cancer patients experiencing a recurrence survived longer when they had been exposed to a brief psychologic intervention an average of 11 years earlier. We are concerned that breast cancer patients, clinicians, and researchers might accept these claims at face value, leading patients to mistakenly seek and professionals to mistakenly recommend psychologic intervention to improve the quantity, rather than the quality of patient lives. Regardless of claims made by researchers invested in demonstrating the contrary, no study of psychologic intervention in which survival was an a priori end point and intervention was not confounded with improved medical surveillance and attention has demonstrated a survival benefit among cancer patients (2). The current study does not alter this conclusion. As stated previously (3) regarding the Andersen et al. parent study (4), claims about survival benefits are medical claims and should be evaluated using the same standards of evidence whether they arise from psychologic, pharmacologic, or medical interventions. Andersen et al. (1, 4) fail to meet these standards.

Results are based on an extremely small sample and an even smaller number of events. This report isolated 29 patients from the intervention group who subsequently recurred, 10 (34%) of whom survived, and 33 from the no treatment group, 8 of whom survived (25%). Although the authors present these data as representing a "59% reduction in the risks of dying from breast cancer" (1) the numbers are too small to have any real clinical significance when considered in absolute terms. Moreover, these numbers are too small to achieve statistical significance in simple analyses or be subjected to multivariate analyses appropriately. The authors do not provide unadjusted results, but report a significant multivariate analysis of study arm, adjusting for three of nine covariates examined using stepwise regression, a procedure that is well-known to overfit data and generate study-specific results that do not typically generalize to other samples (5). The strategy for selecting covariates was not explicit, and these included some variables which the authors claim the intervention affected (e.g., psychologic distress) but not others (e.g., health behaviors; ref. 6). Oddly, selected covariates did not include any of the minimization factors Andersen et al. previously stated should always be adjusted for "when analyzing data from a trial using this method (of randomization)" (ref. 4, pg. 3451). Regardless, there was no statistically significant difference between the groups in terms of proportion surviving and absent control variables.

The authors attribute their report of a survival benefit to a "biobehavioral advantage" for the intervention arm. This is somewhat puzzling, given that the intervention arm received a significantly higher chemotherapy dose intensity, and no significant study arm or study arm by time effect was found for health measures. Immunologic analyses lead to inconsistent results, with a significant arm by time effect for one of three reported analyses, NKCC, but no real discussion of the clinical utility of the size of the Natural killer cell cytotoxicity (NKCC) effect in the context of null findings for Concanavalin A (Con A) and Phytohaemagglutinin (PHA). Overfitting is again a likely issue, with the smaller of the two groups having only 18 cases and an unknown number of covariates entered into models used to assess group differences. In any case, it is difficult to make sense of any biobehavioral analysis based on a sample with 34% of the cases missing (21 of 62 patients). Such a large proportion of missing data in such a small sample invalidates any conclusions that the authors attempt to draw.

In sum, these findings do little to illuminate the complex relationships and pathways between stress, immunologic function, and tumor biology. Moreover, they risk motivating vulnerable patients to seek psychologic support for unproven reasons. Support and psychotherapeutic groups have much to offer patients, but improved survival has not been shown to be among them.

Disclosure of Potential Conflicts of Interest

No potential conflicts of interest were disclosed.

Published OnlineFirst 10/19/2010.

Authors' Affiliations: [1]University of Pennsylvania, Philadelphia, Pennsylvania; [2]Emory University, Atlanta, Georgia; and [3]McGill University, Montreal, Quebec, Canada

doi: 10.1158/1078-0432.CCR-10-1703

References

1. Andersen BL, Thornton LM, Shapiro CL, et al. Biobehavioral, immune, and health benefits following recurrence for psychological intervention participants. Clin Cancer Res 2010;16:3270–8.
2. Coyne JC, Stefanek M, Palmer SC. Psychotherapy and survival in cancer: the conflict between hope and evidence. Psychol Bull 2007; 133:367–94.
3. Stefanek ME, Palmer SC, Thombs BD, Coyne JC. Finding what is not there: unwarranted claims of an effect of psychosocial intervention on recurrence and survival. Cancer 2009;115:5612–6.
4. Anderson BL, Yang H, Farrar W, et al. Psychologic intervention improves survival for breast cancer patients. Cancer 2008;113:3450–8.
5. Peduzzi P, Concato J, Feinstein A, Holford T. Importance of events per variable in proportional hazards regression analysis. II. Accuracy and precision of regression estimates. J Clin Epidemiol 1995;48: 1503–10.
6. Andersen BL, Farrar WB, Golden-Kreutz DM, et al. Psychological, behavioral and immune changes after a psychological intervention: a clinical trial. J Clin Oncol 2004;22:3570–80.