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Letter

Response to Comments on: Does use of alternative medicine predict survival from cancer? *Eur J Cancer* 2003, **39,** 372–377☆

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Hamre and colleagues state that our study does not justify "speculations about causal links between AM use and cancer survival". We agree. As we explain in our paper [1]: "the association observed ... cannot establish a causative relationship". Indeed, we explicitly discount a causal link by stating "we do not believe that AM treatment directly influences survival" and instead we hypothesise that "shorter survival among AM users might be explained by patients' correct perception of the gravity of their disease".

In response to Hamre and colleagues' specific criticisms:

1. The flow of participants in the study is carefully explained in the initial paper [2]. The two main reasons for patients not being included in the final analysis were failure to return the questionnaire about AM use and being old or bedridden. The response rate to the questionnaire (75%) is well within the range considered acceptable in survey research. Exclusion of the very sick does not create a "selection bias". As regards missing data, as can be seen in Table 1 in the paper, we report few missing data in the data-set, except for the type of treatment (curative versus palliative treatment) where 10% of the data were missing. Since more users of AM received palliative treatment, we kept "type of treatment" in the main analysis ($n = 421$, $(421/515 = 82\%)$. Removing "type of treatment" from the model, but keeping all other disease- and demographic-related factors ($n = 480$, $(480/515 = 93\%$ of all patients) strengthened the association between the use of alternative treatment and survival ($n = 480$; hazard ratio 1.33; 95% CI 1.03, 1.72; $P = 0.026$).
2. Whilst it is true that more AM users had advanced disease, and that advanced disease is associated with a shorter survival, we controlled for this confounder in two separate ways: by using multivariate regression and by subgroup analyses that included only palliative care patients or those with metastatic disease. Results were extremely similar for the different analyses. The distribution of the different cancer forms between users and non-users was not provided in this paper, but was discussed in depth in the reference paper describing the patients [2].
3. Whilst Hamre and colleagues are technically correct to notice that P values of 0.056, 0.052 and 0.059 are, in fact, greater than a P of 0.05, it is clear that the preponderance of evidence supports an increase risk of death amongst AM users. We note, for example, the consistency of the findings between different analyses and that the relationship between AM and survival was strengthened by removal of non-predictive variables or examination of disease-specific survival.

We believe that our original methodology and findings are sound, indeed conservative, and support our original conclusion, which contrary to the implication of Hamre and colleagues, is that AM is associated with a shorter survival and not that it causes increased mortality.

References

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