Reviewer's report

Title: Psychophysiological effects of a web-based stress management system. A prospective, randomized controlled intervention study of IT and media workers.

Version: 2 Date: 8 March 2005

Reviewer: holger ursin

Reviewer's report:

General

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. The discussion is far too long, and requires substantial editing and language corrections. The discussion of methodological considerations is edited with a listing of arguments increasing from “firstly” up to “seventhly”, which at best is an unusual way of doing it. It did not help me, and the section is too long anyway. The conclusion refers to a “following” study, and acknowledgement is not written in the standard way. The abstract also requires language editing.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

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Discretionary Revisions (which the author can choose to ignore)

1. You assume that the markers are health related, and that their findings may be helpful in counteracting stress and health problems. You claim that stress related disorders are “major public health issues” and are expected to become increasingly common. You refer to WHO reports on global burdens in health. These deal mainly with depression and anxiety. These statements lack somewhat in precision.

2. In the introduction as well as in the discussion you refer to “stressful periods” in the Swedish society and to the election campaign, later also to seasonal variations. As far as I can read the method section, all experiments were done within the same period and with the same follow-up period. Since this is a randomized trial, some of this variance is controlled for. The conclusions become difficult and speculative when the changes are found in the control group. There is also no discussion of the possible variance in sampling time of the day. For many of these markers there is a considerable variance during the early morning hours.

3. I had a hard time following the arguments from the logistic regression studies. The odds ratios appear impressive at first, but then you go on to state that the predictions “remain significant even after adjustment for age, gender, annual income, education, marital status, etc”. There is also a referral to various “models” in the abstract, which is hard to interpret. Are the odds ratios reported the predictions that “remain” and what are the various models referred to in the abstract?

4. Some of the discussions of the biological markers, perhaps particularly for “NPY”, refer to findings
from pharmacological doses, which may not be relevant for variance within the physiological range.

5. It is surprising that in the discussion the sleep findings are suddenly the most important findings.

**What next?:** Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No

**Declaration of competing interests:**

'I declare that I have no competing interests'