Reviewer's report

Title: A systematic review of the evidence for direct health benefits of natural environments

Version: 2 Date: 18 June 2010

Reviewer: Terry Hartig

Reviewer's report:

1. Is the question posed by the authors well defined?
   Yes.

2. Are the methods appropriate and well described?
   Yes.

3. Are the data sound?
   They appear to be so.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
   Yes.

5. Are the discussion and conclusions well balanced and adequately supported by the data?
   Yes, with exception noted in my comments.

6. Are limitations of the work clearly stated?
   Yes.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
   Yes.

8. Do the title and abstract accurately convey what has been found?
   Yes.

9. Is the writing acceptable?
   Yes.

I appreciate the consideration given to the comments made in the initial review. The authors have satisfactorily addressed my major concerns.
- Discretionary Revisions

p. 6, lines 1-6: The authors seem to suggest that the evidence covered from the review comes from environmental management. It actually comes from multiple disciplines, including but not limited to exercise psychology, environmental psychology, landscape architecture.

p. 13, lines 3-4: “Eight studies investigated effects on attention/concentration (tests such as Backward digit span test but included ratings of ADD/ADHD symptoms).” Something seems wrong here ....

p. 20, lines 6-7: The authors write, “The clinical significance of any such short-lived effect is however, uncertain.” It seems strange to me that the authors make such a statement, as a similar statement could be made about most if not all of the effects considered in the meta-analysis. I would guess that few if any of the authors of the studies reviewed would argue that one brief instance of improved emotion, reduced physiological activation, or enhanced cognition has clinical significance. Rather, researchers in the field have assumed that such short-lived effects cumulatively can have significant implications for health. I think the authors adequately deal with this issue on p. 21 when they write, “The longer-term implications of repeated exposure to different environments cannot be fully assessed.” They might reinforce that statement by mentioning that it is assumed that repeated exposure will cumulatively prove beneficial.

Table 2. I didn’t catch this in the first review, but it looks like the authors have included only one of the two attention measures used in Hartig et al. (2003). Have the authors averaged across these measures? If so, they might indicate this in a note to the table or clarify it in the text.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.