Reviewer's report

Title: Bright Green Light Treatment of Depression for Older Adults

Version: 1 Date: 1 July 2005

Reviewer: Bjørn Bjorvatn

Reviewer's report:

General
This is a potentially interesting and important paper examining the effects of bright green light treatment on depressive mood in older adults. It is done in a natural setting (at home), making generalization of the result easier. 33 volunteers participated, making it a fairly small study. The paper is not strengthened by an appropriate baseline circadian assessment or melatonin assays as the companion paper examining bright white light by the same authors. Both subjective and objective data were gathered. The research group is well-known and has contributed significantly to this area of research. Although the results are negative, publication may be important. However, the paper needs major revision.

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

Similar to the companion paper on bright white light, the major problem with the present paper is the lack of a proper discussion of the results. Actually, the manuscript (especially the discussion) looks like a draft, and not like a complete paper (did the authors by mistake submit an earlier version of the paper??!). The discussion is surprisingly brief and incomplete, and the first paragraph seems misplaced (should come later in the discussion).

The reader wonders why the bright green light had no effects on mood, circadian rhythm etc, and the authors need to elaborate on this. Other studies have shown positive effects, and a discussion about differences in design etc is needed. In this study, participants were diagnosed with depression and were currently under treatment, either with antidepressants or psychotherapy. No information is given regarding how long they have been depressed, or how long they have been under treatment. A better description of the depression is thus needed. Bright white light has clear effects on SAD, and the authors need to better clarify whether these participants also suffered from SAD. Was SPAQ used, or are there any data on symptoms summer/winter? Bright green light may be effective especially during the winter months, and I wish the authors were able to differentiate between those being treated with green light/dim light during the summer and those during the winter. One may of course wonder if the effects of bright light may be minor in San Diego, being so far south, whereas the effects may be clearer in the more northern latitudes. One may also speculate that 4 weeks of treatment may be insufficient considering that these patients are old and probably suffer from chronic treatment-resistant non-SAD depression. The introduction of wake treatment prior to bright light is interesting, but not elaborated in the discussion. This was done to improve antidepressant effects, but did not have any effects in these older patients. Why? All these issues need to be discussed.

In the paper the authors write that they excluded patients that were outdoors so much that light treatment had little to add, but they fail to specify what the limit was. This needs to be specified. No sleep data (sleep length, sleep onset latency, sleep efficiency, etc) are presented. This is a major weakness. Bright light may change sleep, and actigraphy data are collected. I strongly advise for including sleep data in the paper.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the
1. Abstract: background: A little bit more here, please.
2. Conclusions: First sentence about adverse reactions belongs in the results.
3. Intro: There is a lot about different photon densities (too much in my opinion), but not much about depression and sleep in the elderly. Please add.
4. Methods. GDS: spell out first time the abbreviation is used.
5. End of methods: Remove last paragraph about what the other study focused on, or move to introduction.
6. Results, adverse reactions: The SAFTEE data are shown combining dim and bright light. This is confusing. This is a study examining the difference between dim and bright green light, and these data should be presented separately. All combined data should be deleted.
7. Discussion: Again, I find the discussion very brief and incomplete. First paragraph should be moved later in the discussion. Please elaborate on the findings and discuss the results in relation to other studies on the effects of bright light.
8. Discussion, 2. paragraph: First sentence about improvement by green light is misleading. There were no effects of green light, compared to red light, so remove sentence. Actually, follow-up data favor dim red light, but nothing is significant.
9. Discussion, end of third paragraph: “brighter green light might well demonstrate a substantial and significant benefit”. I suggest removing “substantial”.
10. Discussion, last paragraph: The study showed no clinical benefit of bright green light, so a more careful conclusion is warranted.
11. The tables are in general not very clear. They are for instance lacking explanations of the abbreviations. In table 3: the column with total data can be removed, as it adds nothing. The title should include Hamilton. Table 4: do not use combined bright and dim light. The purpose of the paper is to compare these two treatments. Table 5: Not easy to understand. Better explanations are needed.

Discretionary Revisions (which the author can choose to ignore)

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

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

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

Yes to first question: I am a consultant for a Norwegian company selling bright light boxes.
No to all the other questions.