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***Interactive comment on “Satellite-based estimates of light-use efficiency in a subtropical mangrove forest equipped with CO<sub>2</sub> eddy covariance” by J. G. Barr et al.***

**Anonymous Referee #2**

Received and published: 4 January 2013

General Comments:

This manuscript provides a detailed account of the development of a light use efficiency model specific to mangrove forests in the Everglades. The authors, well-versed in eddy covariance (EC) measurements within mangrove forests, combine EC data with satellite data to determine the ecosystem components important for estimating light use efficiency (LUE), and then the gross primary production of this ecosystem. Their use of a Bayesian framework allows flexibility in creating a model that is robust, yet that accounts for uncertainty in model inputs. The authors found that their LUE model, MVP-LUE, was able to capture much of the variability in the system, particularly with

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the inclusion of salinity, diffuse radiation, and a non-linear relationship between PAR and air temperature. This model out-performed the standard MODIS algorithm, which does not include any of these parameters. The manuscript is well-written, and should be considered for publication in Biogeosciences with due consideration of the comments below.

## Specific Comments:

- 1) The title does not seem to capture the key concept described in the paper. Much of the paper is about development of a mangrove-specific LUE model (e.g., page 14, line 454); this should be reflected in the title.
- 2) CO<sub>2</sub> storage in the canopy isn't mentioned here. Is this a significant part of the carbon balance in mangroves? If so, what is the potential impact of not including this part of the carbon balance?
- 3) The authors state that soil respiration is likely to be a minor contributor to ecosystem respiration (page 5, line 149 through page 6, line 151). However later, they include two sentences that seem to contradict this statement. On page 10, lines 303-305 refer to warmer soil temperatures leading to an increase in belowground respiration and lines 311-313 refer to work by Lovelock (2008) regarding increasing soil temperature and soil respiration. With the damage from Hurricane Wilma and subsequent increase in solar radiation at the surface, would the increased soil temperatures play a significant role in increasing ecosystem respiration? If so, should soil temperature then be included in equation 2? Also, the citation of Lovelock seems to be out of place.
- 4) In the Results and discussion section, there seems to be discussion in every section except section 3.3 (Physical drivers). Consider expanding a bit to include the impacts of the conditions described in this section to match what is covered in other sections.

## Technical Comments:

Page 6, Line 182 and Page 13, Line 433: Which versions of the MODIS EVI and GPP

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were used?

Page 9, Line 275: change “number or forcing terms” to “number of forcing terms”

Page 11, Line 327: change to “surface albedo and(?) the satellite-based greenness”

Page 13, lines 401-402: change “nearly as well during validation (...) compared to training (...)” to “nearly as well during validation (...) as during training (...)”

Figure 1: Site SH3 is mentioned on page 4, line 108 of the manuscript, but it is not shown on the map in figure 1. Is it co-located with LTER site SRS6? If so, consider changing the descriptor on the map to “Study Site” or something similar and include a statement in the caption that the 30-m EC tower, SRS6, and SH3 are co-located at the study site.

Figure 3: Change the caption to read “average albedo (raw) and albedo adjusted to include” since the figure itself refer to Raw and Adjusted values.

Figure 5: Consider changing panels A, B, C to have the same y-axis so that the reader can see not only the individual values, but also their relationship to each other. Or, include all of them in the same, larger, graph.

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**BGD**

9, C7034–C7036, 2013

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