Response to Anonymous Referee #2

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 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.

We thank the reviewer for these comments and for the thorough appraisal of our paper.

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.

The new title reads, "Modeling light-use efficiency in a subtropical mangrove forest equipped with CO_2 eddy covariance".

2) CO_2 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?

Storage of CO_2 is an important component of NEE, and therefore affects estimates of both nighttime R_E and GPP. We included CO_2 storage in our NEE estimates as described in Barr et al. (2010) and Barr et al. (2012). The following sentences were added to define our NEE estimates. "NEE represents the EC-derived CO_2 flux at a height of 27 m plus the amount of CO_2 stored in column of air below this height since the previous time step. This storage was estimated from the change in CO_2 mixing ratio at the infrared gas analyzer level of 27 m (Barr et al., 2010)." (p.5, lines 128-130)

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).

The text was modified as follows to show that soils are important but secondary contributors to R_E in mangrove forests. "Measurements of soil respiration in relatively undisturbed mangrove forests throughout the Caribbean, Australia, and New Zealand (Lovelock, 2008) suggest that soils contribute less respired CO_2 to R_E compared to that of above ground sources. However, the fractional contribution of the soil to R_E may increase as a result of hurricanes or other disturbances. Soil respiration increased by 18% in a dry tropical forest in Mexico one year following disturbance from Hurricane Wilma (Vargas and Allen, 2008)." (p.6, lines 151-156)

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.

The modified text (p.10, lines 311-315) reads, "Such processes contributed to increased nighttime soil-air temperature gradients of 1 to 3 ^{o}C one year following disturbance (Table 1, Barr et al., 2012). Warmer soils in this system are expected to lead to increased belowground respiration and fractional increases in the belowground contribution to total nighttime R_E. During both pre- and post-disturbance periods, the functional response of R_E to air temperature exhibited a better fit than that using soil temperature."

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.

The citation of Lovelock (2008) was removed. Also, we added the statement, "During both pre- and post-disturbance periods, the functional response of R_E to air temperature exhibited a better fit than that using soil temperature." (p. 10, lines 314-315) In excluding soil temperature in equation 2, we attempt to avoid over-fit as the model (equation 2) currently includes 5 fitted parameters. Also, air temperature at 27 m and soil temperature (at -5 cm) were somewhat correlated, even with inundation during high tide periods. Therefore, not much additional predictive ability could be gained by including soil temperature as a control on R_E. New text was added to provide some insight regarding the increase in R_E following disturbance and the increase in air temperature that defines peak R_E. "This increase in temperature, which defines peak respiratory response, also suggested an increased contribution of belowground respiration to R_E following disturbance. Quantifying the belowground contribution to R_E and the respiratory response to soil temperature require continuous measurements of belowground respiration, and such measurements were not made during this study." (p.11, lines 327-330)

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.

Text was added to the section (p. 11-12) discussing the controls of PAR, air temperature, and salinity on productivity.

Technical Comments:
Page 6, Line 182 and Page 13, Line 433: Which versions of the MODIS EVI and GPP were used?

Regarding MODIS EVI, these details were included in the following text. "For this study, the EVI data (Fig. 3b) were obtained from the MOD13A1 product (EOS; http://modis.gsfc.nasa.gov/). The mangrove flux tower site is included in grid h10v06, with a 500-m spatial resolution. Using GIS (Geographic Information System) software (Matlab Mapping Toolbox, The Mathworks, Inc., Natick, MA), the 16-day composite average EVI values for the pixel corresponding to the flux tower site and the 8 adjacent pixels were extracted for the period 2000 to 2011." (p.7, lines 189-194) Regarding MODIS GPP, new text was added in section 2.3 (p.7, lines 195-199).

Page 9, Line 275: change "number or forcing terms" to "number of forcing terms"
Done

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

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

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 1 was modified, and the study site is labeled in the figure and in the legend. The figure caption now reads, "Fig. 1. Map of Everglades National Park showing mangrove forest zones along the coast, the study site, and the Park boundaries, defined by the thick green line. 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.

Done

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.
In the new Fig. 5, panels A, B, and C have the same y-axis range of 0 to 0.8 mol C m^{-2} day^{-1}.