Response to Associate Professor, Dr. Lindsay Hutley

This paper presents an important data set describing mass and energy exchange from a tropical mangrove community in Florida, USA. There are few if any long-term flux data sets from mangrove forests given the technical difficulty and fetch constraints associated with operating eddy covariance systems in mangrove forests. The data span a good range of environmental conditions including recovery from a hurricane. Such observations provide data quantifying mangrove forest sink strength, important given the current interest in 'blue carbon' ecosystems with claims of large levels of carbon storage and high carbon sink potential. This is important as recently, mangrove forests can now be included in REDD+ schemes and knowledge of their response to environmental drivers and typical sink strength is increasingly important if they are to be included in emission offset and rehabilitation programs – we need to know how these systems work.

We thank Dr. Hutley for his review and for attesting to the importance of quantifying mangrove forest productivity and carbon fluxes.

The aim of this paper was to develop a light use efficiency model linked to MODIS derived EVI to predict productivity as a function of key variables such as salinity and temperature. Given many mangrove systems of the world are in tropical and/or difficult to access areas, developing a robust method of estimating GPP from satellites is highly valuable.

Thank you for affirming the importance of the second objective of our study, which was to test and validate a model of mangrove forest GPP using satellite reflectance data.

The methods look sound from this experienced team who are leaders in using EC methods in mangrove ecosystems. The development of an ecosystem respiration model for this ecosystem was also novel and useful. Modeled GPP using the calibrated LUE model performed well. I thought the paper was well written and well-structured and is of appropriate scope for Biogeosciences.

Thank you for the time invested in reviewing our paper and for providing feedback to improve the quality of our paper.

I had only minor comments, this is a very sound piece of work;
1) Why was soil/sediment temperature (assuming it was measured) not included in the ecosystem respiration modelling, in addition to or as well as air temperature?

New text was added to address this concern. For instance, "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." (p.10, lines 312-315) We also concede that some of the increases in R_E observed following disturbance were likely the result of higher soil temperatures and increased soil respiration. New text was added to explain the change in functional response of R_E to air temperature in equation 2 as follows. "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 326-330)

2) Use 'fPAR' instead of 'FAPAR', fPAR is the standard acronym.

FAPAR was changed to fPAR throughout the text.