Interactive comment on “Carbon exchange between the atmosphere and subtropical forested cypress and pine wetlands” by W. B. Shoemaker et al.

Anonymous Referee #1

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This article reports on the dynamics of Net Ecosystem Exchange of CO2 (NEE) and energy fluxes (sensible and latent heat, H and LE respectively) using one year of data from three distinct ecosystem types in or near the Florida everglades (a cypress swamp, a dwarf cypress wetland, and a pine upland forest). In one site (the dwarf cypress), methane flux is also measured. The dynamics were primarily linked to seasonal variation in meteorological drivers and the enhanced vegetation index (EVI). The data from the cypress sites have the potential to improve our understanding of ecosystem carbon cycling in warm subtropical wetlands, which are certainly underrepresented in the current network of flux monitoring sites. The upland pine site is not a wetland site; nonetheless, it represents a historic ecosystem type (i.e. open pine savannah experiencing frequent fire) that is the focus of many restoration efforts in the region. Thus, studies that help us to understand patterns and drivers of carbon and water cycling in such a forest should be viewed as valuable contributions to the literature. Finally, these studies are located in an important conservation area (i.e. the Florida everglades) reknown for its biodiversity and subject to much regional and national attention from the environmental community. Thus, while the significance of these results is potentially large, the current study suffers from a number of shortcomings, many of them methodological, which reduce the strength of the conclusions and the relevance of these results for predicting long-term patterns of carbon and water cycling in these sites.

Thank you for reviewing this paper. Author responses are in bold.

First, the authors present only one year of flux data from each of the three sites. Interannual variability in NEE can be quite large, and a snapshot based on just one year of data can provide a biased view of the carbon uptake capacity. Undoubtedly, the manuscript would be strengthened with the inclusion of an additional year of data. The study period for these results is December 2012 – November 2013. If the towers are still running, then an additional year of data should exist at this point.

Agree, another year of data was added to the manuscript. The paper now covers water, energy and carbon fluxes from 12/2012 to 11/2014.

Next, the authors take an unorthodox approach to filtering, gapfilling, and partitioning the NEE fluxes, which leads me to view the reported annual values with some skepticism, especially since they are among the highest values ever reported in the literature (see Baldocchi et al. 2008, Australian Journal of Botany for reference).First, while they apply a u* threshold to remove data collected under insufficiently turbulent conditions, this threshold is extremely low (< 0.05 m/s). If nocturnal data are retained that are collected under stable conditions, then vertical and horizontal advection fluxes may be important missing components of the flux balance, which could suppress inferred respiration model and thus lead to very high |NEE|.

Agree, the u* threshold was redefined based on plots of u* versus nighttime (9PM to 4AM) NEE normalized by air temperature and vapor pressure deficit, as described by Aubinet et al. (2012, pg. 147). NEE appeared to be considerably different as u* approached a 0.1 threshold at each site. u* filters were therefore increased to 0.1.

Second, daytime NEE data were gapfilled using a relationship between NEE and LE, which in my view is not appropriate as LE can represent a significant contribution from evaporation, which is not mechanistically coupled to carbon assimilation or respiration. This is particularly truein sites that support standing water!
Agree, the NEE(LE) gap-filler was replaced with a conventional look-up table from Reichstein et al. 2005.

While I do not believe that all flux data must use a uniform set of gapfilling & partitioning approaches, some justification for novel approaches should be provided, and evidence should be given that these approaches are more site-appropriate than the well-established procedures that are widely used by the community (see, for example, Reichstein et al. 2005 or Lasslop et al. 2010). In the case of this particular study, I believe the results would be much stronger if the site-specific fluxes were presented alongside flux estimates derived from standardized approaches. Towards that end, the authors may find this Online Flux Partitioning and Gapfilling tool helpful: http://www.bgc-jena.mpg.de/_MDIwork/eddyproc/. The Reichstein et al. (2005) and Lasslop et al. (2010) approaches are discussed thoroughly on that website.
Agree to use well-established procedures as documented by Reichstein et al. 2005.

Third, as the authors acknowledge, export of carbon through surface water flow can be an important component of the carbon balance in wetland ecosystems. Unfortunately this was not measured in these sites; this is okay, but the authors should do a better job of discussing the relevance of this missing term, and also make its absence clear in the abstract.
Agree, "unmeasured" overland flow was added to abstract. This issue is mentioned in the text beneath equation 1 as "Technical difficulties inherent in measuring "sheet flow" and the dissolved/particulate organic/inorganic C concentrations within surface water did not allow quantification of this term."

Finally, the discussion of the drivers of these ecosystem fluxes is largely focused on seasonal patterns in meteorological conditions and EVI (or leaf area). Their principle conclusions seem to be: a) NEE and LE will be more decoupled in sites with open water, and b) replacing green leaf area with open water will decrease the magnitude of carbon uptake. Neither of these are particularly surprising and both could have been predicted a priori.
Disagree, the relative magnitudes of transpiration and evaporation are uncertain and debated - albeit verbally in science discussions of ET in greater Everglades wetlands. This paper demonstrates the magnitude of transpiration (and C uptake) is resolvable with EC methods and sufficient to alter bulk ET over cypress and pine forested wetlands with standing water. Furthermore, periphyton production at open water sites has been suggested as a mechanism for C uptake. A priori, it was unclear whether growth and decay of periphyton would produce similar C uptake rates as cypress, pine or sawgrass productivity. This study provides some clarity on these issues.

I wish the authors had focused more closely on the unique physical and physiological features of the site (i.e. variation in water table depth, the effect of burning in the pine upland site, the exceptionally warm and mesic climate, etc), as in doing so their results may have represented a more novel and meaningful contribution to our understanding of carbon and water cycling in these ecosystems, and the sensitivity of these fluxes to ongoing changes in climate and management regime.
Agree, another year of data was added to the analysis, creating an opportunity to discuss soil respiration responses to water levels dropping below land surface for an extended period of time at all three sites. Soil oxidation is of keen interest in south Florida. We are

able to link enhanced respiration to dry conditions using the 2nd year of data. We are unable to address the effects of fire and controlled burns on C cycling, as there were no fires on record during our study time period. A controlled burn recently occurred (during Jan 2015) near the Dwarf Cypress site. Preliminary analysis indicates the burn areas were outside of our C flux footprint.

Some minor comments follow: Section 2.1: Can the authors report on leaf area index for the study sites, rather than using a qualitative approach to describing canopy cover (i.e. open vs dense?).
Our attempts to collect LAI data during field runs have failed, either due to bad equipment or safety issues associated taking measurements while hanging from tall towers. We feel panoramic photos in Figure 2 provide readers with an understanding of LAI at each site.

Section 2.3, page 15760, lines 1 - 10: These threshold filters seem to be too limiting. How were they chosen, and what is the effect of using thresholds that have a higher absolute magnitude?
Agree, the thresholds seem somewhat subjective and biased by expert judgment of unrealistic limits. After advanced processing with EdiPro, our 30-minute fluxes were somewhat "spikey", especially at the Cypress Swamp site (see Figure 6B). Higher thresholds created "spiker" daily results. Some of the "spikes" could be real ebullition or large uptake events; however, it is difficult to know this for sure as processes such as ebullition are active areas of research. Furthermore, rapid events are difficult to model for gap-filling procedures. We acknowledge this issue as limitation of our work.

Section 2.3, page 15760, lines 12-17: A very large amount of data from the pine upland (60%) is removed in an effort to avoid contamination from fossil fuel burning occurring to the east of the tower. More details need to be provided about when the data originates from the east (for example, is it principally at night or during the day)? Also, it is possible a footprint model could be used to more carefully exclude questionable data, and thereby improve data availability?
Agree, more details were added regarding seasonality in wind-direction, mean day/night wind directions, and data availability after the wind direction filter. We also reduced the filter to exclude data originating from 15 to 130^{o}, which increased data availability for trend identification and C flux modeling.

Section 2.3, page 15761, lines 1-10: The authors correct the energy balance fluxes in order to force energy balance closure. This is not an approach that I recommend, as there are many reasons why energy balance closure may be low at any given site, and the synthesis of Foken (2008) and Stoy (2013) suggest that macro-scale heterogeneities will likely affect the observation of sensible heat for than latent heat flux, which invalidates the assumption of accurate measurement of the Bowen Ratio. If the authors insist on this approach, at some point the pre-corrected energy balance closure should be reported. It would also be helpful to report the estimates of annual sensible and latent heat flux before and after the correction.
Agree, thank you for this clarification of the literature. We removed the energy budget closure correction for H and LE.

Section 2.3, page 15762, Lines 11-22: Was the methane data missing evenly over the course of the year, and over the course of a representative day?
Agree, we clarified that CH4 was missing mostly during the months of 12/2012 to 5/2013; 10/2013 to 1/2014; 4/2014 to 5/2014; and 11/2014. Missing data also was identified on Figure 5C.

Section 3.3., page 15766, lines 24 - 26: Can these correlations between NEE & LE be placed into context by including previously reported values at other sites.
We were unable to find published correlations of NEE and LE at other sites. These correlations may become more available in the literature, as deployment time increases for the relatively new LICOR 7500 gas analyzers.