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

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This manuscript reports on one year of eddy covariance observations of carbon, water, and heat flux at a trio of sites in Florida, USA that present fluxes at a topographic gradient across pine upland, cypress swamp, and dwarf cypress vegetation within and near a preserve. These are understudied ecosystems and its C sequestration has important implications for wetland restoration. It's good to see more of these kinds of sites reported in the literature and I believe this paper should be published. However, there are some methodological shortcomings and areas for additional analysis that will require major revision before I believe it should be accepted in BG.

**Thank you for taking time to review our paper, author responses are in bold.**

Major:

The sites were certainly challenging to measure and a result a number of gaps occurred. The presence of oil drilling nearby complicated one site, while methane sensor window cleanliness affected another. Further, the GEE/RE partitioning method is somewhat novel and motivated mainly from the perspective of poor correlations with standard variables (T, maybe PAR?). I think if the authors wish to present annual sums of various flux components, much more work should be done on estimating uncertainty in the flux estimates due to sampling error, gaps, and partitioning and these should be propagated through to estimate uncertainty of annual NEE, GEE, RE, which will help later put into context any inter annual variability in future analyses and also make the sites more comparable to the published literature. For sampling error, this could be done with existing published methods. I believe EddyPro or other flux processing software outputs at least an estimate of error. Generally, this is small.

**Agree, random error for measured fluxes was estimated with EdiPro based on Finkelstein and Sims (2001).**

Gap filling error can be estimated either by a) propagating the uncertainty of the least-squares regression parameters using a Monte Carlo approach or b) creating artificial gaps and estimating the reliability of estimating fluxes using the gap filling model (i.e., within site cross validation) - Partitioning error can be estimated in similar way to above.

**Agree, artificial gaps (1, 5, 10 and 20% of available NEE) were created in observed NEE for LUT gap-filling based on Reichstein (2005). The standard error of residuals between observed NEE and LUT NEE was used as an approximation of uncertainty. The maximum standard error of the artificial gap scenarios was summed with random errors into root mean square monthly uncertainty estimates.**

In particular, the use of latent heat flux (and the inherent uncertainty of that value which is greater than for temperature or other state variables) to fill NEE requires some more discussion on mechanism and reliability. What about shortwave radiation?

**Agree, the latent heat gap-filling function was replaced with a LUT approach from Reichstein (2005). This LUT approach performed better than the latent heat correlation for gap-filling NEE.**

The authors might also consider comparing the NEE, GEE and Reco estimates from their method to standard published methods, such as the MDS or temperature based non-linear regression. Even though the fit is bad to temperature, it is worth showing what the standard gap filling models produce. MDS relies on sampling across measured observations and there are pre-compiled R packages and online gap filling tools at http://www.bgc-jena.mpg.de/bgc-mdi/html/eddyproc/index.html

Moffat, A.M., Papale, D., Reichstein, M., Hollinger, D.Y., Richardson, A.D., Barr, A.G., Beckstein, C., Braswell, B.H., Churkina, G., Desai, A.R., Falge, E., Gove, J.H., Heimann, M., Hui, D., Jarvis, A.J., Kattge, J., Noormets, A., and Stauch, V.J., 2007. Comprehensive comparison of gap-filling techniques for eddy covariance net carbon fluxes, Agricultural and Forest Meteorology, 147(3-4): 209-232, doi:10.1016/j.agrformet.2007.08.011.

Desai, A.R., Richardson, A.D., Moffat, A.M., Kattge, J., Hollinger, D.Y., Barr, A., Falge, E., Noormets, A., Papale, D., Reichstein, M., and Stauch, V.J., 2008. Cross site evaluation of eddy covariance GPP and RE decomposition techniques, Agricultural and Forest Meteorology, 148(6-7): 821-838, doi:10.1016/j.agrformet.2007.11.012.

Reichstein, M., Falge, E., Baldocchi, D., Papale, D., Aubinet, M., Berbigier, P., Bernhofer, C., Buchmann, N., Gilmanov, T., Granier, A., Grünwald, T., Havrankova, K., Ilvesniemi, H., Janous, D., Knohl, A., Laurila, T., Lohila, A., Loustau, D., Matteucci, G., Meyers, T., Miglietta, F., Ourcival, J.M., Pumpanen, J., Rambal, S., Rotenberg, E., Sanz, M., Tenhunen, J., Seufert, G., Vaccari, F., Vesala, T., Yakir, D., Valentini, R., 2005. On the separation of net ecosystem exchange into assimilation and ecosystem respiration: review and improved algorithm. Global Change Biol. 11, 1424-1439. 2.

**The LUT approach from Reichstein (2005) was applied in this paper. Comparisons between the latent heat gap-filler and LUT approaches were not presented, as the goal of this study is to present approximate magnitudes and trends in C fluxes for cypress and pine-forested wetlands.  A separate paper could be written about the relative performance of various gap-filling procedures.**

In particular, with 80% of CH4 missing, the authors just chose to average across existing data to create monthly means, if I understood the manuscript correctly. This seems problematic if the distribution of missing data is biased from month to month. Either this needs to be disproven or the authors should consider MDS or similar gap filling method, and certainly an uncertainty budget is required. Further, this provides another motivation for uncertainty analysis.

**Agree, the distribution of missing data was biased from month to month.  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 was identified on Figure 5C.  CH4 averaging was replaced with a CH4 molar flux model driven by air temperature and water levels at daily resolution.  The standard error of the molar flux model was used for uncertainty analysis.**

3. Lateral flows are neglected and that seems appropriate at this stage. However, there are published estimates from other sites of the % of C in lateral flow in marshes, wetlands, and so forth. Some discussion on what that value might be for these sites and what that implies for the NECB would be useful. See, for example (for northern sites):

Chu, H., Gottgens, J., Chen, J., Sun, G., Desai, A.R., Ouyang, Z., Shao, C., and Cza-jkowski, K., 2014. Climatic variability, hydrologic anomaly, and methane emission can turn productive

freshwater marshes into net carbon sources. Global Change Biology, in press, doi: 10.1111/gcb.12760.

Buffam, I., Turner, M.G., Desai, A.R., Hanson, P., Rusak, J., Lottig, N.R., Stanley, E.H., and Carpenter, S.R., 2011. Integrating aquatic and terrestrial components to construct a complete carbon budget for a north temperate lake district. Global Change Biology, 17(2): 1193-1211, doi:10.1111/j.1365-2486.2010.02313.x. 4.
**Disagree, the lead author is not comfortable presenting lateral C flux results from other studies as a proxy for the greater Everglades.  Lateral C fluxes are currently being measured near our stations as part of another study; however, the results are preliminary and evolving.  Combining lateral C fluxes in the Everglades with our atmospheric fluxes is the focus of future work.**

There is a lot of confusion in various parts on methane. There is a report value of 12 gC/m-2/yr. Yes, many of the global warming potential estimates use a value of 20 gC (in the abstract) and 15 gC (in the manuscript). Why not use 12? It is almost as if these were written before the final value was inserted into the text! Further, there is good literature showing that IPCC based GWP estimates for CH4 emission from wetland is not appropriate for two reasons: 1) GWP is based on an instantaneous mass pulse of CH4 and CO2 and relative contributions to radiative forcing that are time-scale dependent (hence the 100-yr vs 25-yr values) whereas wetlands have continuous emissions, which alters the net GWP - as most of the past wetland CH4 emissions are already CO2 in the atmosphere - why use 100? and 2) wetlands have been emitting methane and sequestering carbon likely for the past thousands of years whereas GWP is an expected radiative forcing change for a perturbation to the atmosphere. This wetland is not perturbing the background state unless it is converted to something else. For more discussion, I recommend this paper by Frolking et al: Frolking, S., N. Roulet, and J. Fuglestvedt (2006), How northern peatlands influence the Earth's radiative budget: Sustained methane emission versus sustained carbon sequestration, J. Geophys. Res., 111, G01008, doi:10.1029/2005JG000091. So I don't object to including GWP estimates, but caution on their use for wetlands must be mentioned.
**Agree, we clarified the CH4 and C-CH4 fluxes.  We stated some of the limitations you mention regarding GWP multipliers - specifically, "We recognize GWP multipliers are controversial due to assumptions such as instantaneous CH4 and CO2 release, and time-scale dependence of the radiative forcing contributions.  Careful use of GWP multipliers for wetlands is suggested".  We emphasized our conclusion regarding methane is its relative insignificance in the C budget for altering land surface topography.**

5. Some more justification of various aspects of data processing are required. The u* cutoff are quite low and at least in water covered surfaces, diffusive fluxes scale directly with u*.
**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* decreased approximately below 0.1 threshold at each site.**

Correcting latent heat flux by Bowen ratio energy balance closure has published in Twine, but future papers all recommend against this as a standard practice.It puts certain assumptions on where the underestimation occurs, which do not have justification.
**Agree, we removed the energy-budget closure correction for H and LE.**

Finally, strict screening criteria are applied to NEE observations which require at least some discussion of the fraction of large fluxes screened. I worry that "real" events of flushing or large uptake are being missed if too conservative in the screening. Why not apply a 3-sigma type local despike filter?
**Agree, we removed half-hour fluxes that fall outside 3 standard deviations within a moving 7 day window. This local de-spike filter significantly improved trend identification.**

Further, the authors do not measure storage flux, only the turbulent flux. For analysis of half-hourly fluxes in tall canopies (such as the upland and the forested wetland), this adds an additional source of uncertainty (in some cases, previously screened turbulent fluxes may be actually brought back into the threshold by consideration of storage) - at least 1-point storage flux could be computed for the canopy towers.
**Disagree, our stations are over 16 m tall. We decided against including 1-point storage changes in light of guidance in EdiPro: "In eddy covariance applications where profile concentration data are not available, storage fluxes are approximated by using one-point time derivatives, assuming that all gradients nullify at ground level and that the profile is linear from the measurement point to the ground. Evidently, this is an extreme assumption that has little relation to the actual situation. This is why we encourage users to consider storage fluxes provided by EddyPro as purely indicative. For the same reason, storage fluxes are not summed to turbulent fluxes to provide Net Ecosystem Exchange (NEE) estimations, that would be affected by unacceptable inaccuracy."**

For the wind direction screening for the oil drilling, perhaps a supplemental figure of wind direction versus CO2 flux might be useful to see, or a footprint model.
**Agree, more details were added regarding wind-direction seasonality, mean day/night directions and data availability. We reduced the filter to the edge of our comfort zone (15 to 130), prior to re-submitting this paper. Over ten-thousand NEE fluxes remained for trend identification and gap-filling after the contamination filter at the Pine Upland site. Seasonal trends were apparent and diurnal NEE variations were resolvable into surrogates for respiration and photosynthesis.**

6. It appears water depth by pressure transducer was measured and the introduction cites literature on the importance of water level on fluxes at other nearby sites. Yes, there is very little discussion of this other than to mention that dry/wet season differences in fluxes cannot be estimated with only one year of data. However, regression at the half-hourly or daily scale of NEE to water depth within each season might be useful to do for evaluating mechanisms and comparing to other papers - certainly some variability occurs. I wonder to what extent water level may be a better variable for gap-filling and partitioning instead of LE too?
**Agree, a year of data was added to the analysis. This data extension created 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 suppressed respiration to flooding using the 2nd year of data.**

7. Given the importance of the ecosystems locally, it would be nice for the paper to attempt to scale these fluxes across the region. How important are they for the BCNP? Can a simple upscaling be accomplish to discuss total area C sink capacity and current uncertainty?
**Disagree, upscaling is planned for future study. The lead author would like to improve maps of the spatial distribution of plant communities, before upscaling and distributing results.**

8. I recognize that USGS cannot make policy statements. However, the introduction hints at the importance of this study for wetland restoration locally. However, the discussion or conclusion does not fully discuss these implications. Without making a policy statement, I think the paper could make some stronger statements on what restoration might imply for C sink capacity of the area and the impact of changes to the landscape. Perhaps the scaling in the previous comment might help with that.

**Thank you for acknowledging USGS sensitivity regarding policy statements. We added statements regarding the possibility of suppressing soil respiration with hydro-period management. We also state that a redistribution of plant communities toward more open-water ecosystems could create less C uptake and greater evaporative losses. The lead author feels the paper may need to be reapproved by the USGS for publication, if we add further managerial guidance.**

9. Finally, I'd like to encourage the authors to share their flux data, perhaps by submitting to the Fluxnet archive upon publication of the paper. Open data goes hand in hand with open access publication.

**Agree, we've made our flux and met data (with DOI#'s) available on the following Federal public repository:**
**http://sofia.usgs.gov/exchange/carbon_exchange/**
**We hope to eventually share our data with Ameriflux and Fluxnet, as permitted by time, funding and Federal priorities.**

Minor:
I recommend ending the introduction with a set of hypotheses or questions motivated by the Jimenez and other papers and the objectives. –
**Agree.**

Page 15764, line 8 - do you mean transpiration was limited by tree physiology?
**Agree.**

Page 15769 line 20 - the sentence links: were revealed, seems like it could be expanded to make a stronger statement. What links?
**Agree, "such as photosynthetic water-use efficiencies" was added to the sentence**