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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.

Major:

1. 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. - 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. 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? 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. 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.

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. 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.

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*. 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. 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? 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. 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.

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?

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?

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.

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.

Minor:

- I recommend ending the introduction with a set of hypotheses or questions motivated by the Jimenez and other papers and the objectives.

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

- Page 15769 line 20 - the sentence links... were revealed, seems like it could be expanded to make a stronger statement. What links?

Interactive comment on Biogeosciences Discuss., 11, 15753, 2014.