Reviewer's report
MS# bg-2017-526

This manuscript describes three years of ecosystem carbon (C) and water flux observations in a relatively dry temperate evergreen forest. Interesting temporal dynamics of C fluxes are described whereby uptake was highest during the winter months and largely driven by variations in ecosystem respiration (ER). Analyses of controls on fluxes are presented and demonstrate the important role of elevated air temperatures and vapor pressure deficits in restricting surface conductance and limiting gross primary production (GPP) during summer months. Although this was an evergreen forest, phenology and structural dynamics of the canopy were important determinants of photosynthesis.

This manuscript presents material that would be appropriate for the readership of Biogeosciences, provided that the points of concern and comments outlined below can be adequately addressed.

Comments

Please eliminate the use of the diffusely defined term "atmospheric drought". A term like "atmospheric demand" is more precise and appropriate.

Is it really true that the canopy height is ~25 m (L105) and the top of the profile and eddy covariance system is 29 m? This is rather close to a tall canopy for an eddy covariance application for observing ecosystem fluxes. A check of the site description is needed, and if indeed these numbers correct, a clearer presentation and discussion of the implications on representativeness of reported fluxes and analyses is in order.

Was NDVI measured at the site or was a satellite product used? What was the rationale for using only NDVI versus EVI (or checking both)?

A careful read and editing of the methods (and entire manuscript) is needed to ensure better consistency in the use of terminology and symbols be used. Furthermore, where possible, the use of more common symbols/abbreviations would be helpful. A non-exhaustive list of examples includes:

- F_{CT} and F_{CS} are used to represent the eddy flux and storage flux, respectively (Eq. 1). Then Fc and Sc are used to represent the eddy flux and storage flux, respectively (L136).
- F_N (e.g., L183-184) and Rn (L196) are used alternately to represent net radiation.
- The profile system measured CO2 "mixing ratios" (L115), then "concentration" is used later (L160-170) with a symbol similar to the "concentrations" referred to in relation to the high frequency density measurements made by the open path IRGA.
- Use Δ or *s* for the slope of the saturation vapor pressure curve instead of ε
- Use *LE* or λ*E* for latent heat flux

The sign conventions regarding the directions of fluxes are mixed up in places. For example, in the abstract C sinks carry a positive sign for uptake (L18-19), but later in the text (L264-265) "C sinks" are reported with negative signs. Please carefully review the entire manuscript and ensure consistency throughout regarding sign conventions for fluxes, sinks, and sources.

L111…The LI190SB quantum sensor is calibrated to report PPFD as umol/m^2/s. Was this then converted to W/m^2? Check the units throughout the manuscript because the reported values for incident PAR in W/m^2 (e.g., L230-231 and 242, Figs. 1 and 3) are not physically possible. Also check to ensure that there was no effect on analyses and it is only an error in the manuscript text.

L129-137: If the net ecosystem exchange reduces to the sum of the eddy flux and the storage flux, then don't worry about including advection in the equation. Just state the simplifying assumptions clearly in full in the text. Note that more than just well-developed turbulence (L133-134) is needed to simplify the mass balance on the control volume (e.g., horizontal homogeneity). Please be more complete in this description in the text. Were there any concerns regarding the validity of the simplifying assumptions because of proximity of the EC system to the canopy (as mentioned previously)?

On the calculation of the eddy flux:

- It is more accurate to state that the IRGA measures the (number) densities of CO2 and water vapor (L142).
- Eq. 2 is not necessary with an adequate description in the text (but if you keep it define primes and the overbar). It's not the most elegant presentation in the current Eq 2…especially since the equation doesn't include the WPL terms, which are needed. Given the maturity of the EC method a text description is fine.
- L148-149. Rephrase: "Fluxes were rotated into the natural wind coordinate system using the double rotation method". Wilczak et al. isn't the best reference for the double rotation. The original is Tanner, C. B. and Thurtell, G. W.: 1969, Anemoclinometer Measurements of Reynolds Stress and Heat Transport in the Atmospheric Surface Layer, University of Wisconsin Tech. Rep., ECOM-66-G22-F, 82 pp. [Available from US Army Electronic Command, Atmospheric Sciences Laboratory, Ft. Huachuca, AZ 85613.] or referencing the chapter in the Handbook of Micrometeorology.
- L149…what time lags? Between the sonic and IRGA?
- L150-151. Block averaging is not a detrending operation.
- Check to make sure that the order of the steps in the description of the flux calculation matches what was actually done (e.g., one of the last items in the description concerns the removal of spikes in raw data, L153).

On the calculation of the storage flux:

- A complete description of Eq (4) is lacking (definition of all symbols etc.)
- Why were storage fluxes of water vapor not estimated? Is this a significant source of bias in LE measurements?

L183. What exactly is specific heat density (SHD)?

L188-194. It would be worth presenting a footprint climatology in the supplementary information.

L195-199. Clarify whether closure was forced on the fluxes reported in the results.

L200-210. Why were the turbulent fluxes not substituted for available energy when calculating surface conductance? The spatial representativeness would be better.

Eq 7. Check the 2nd term in the denominator on the RHS…the exponent should be -0.67. Add a citation and be sure all terms are defined.

L241-244. The description of leaf-level sampling needs more detail. Since there is a reference that describes these measurements in more detail, the description in this manuscript can be abbreviated, however, a more thorough description of the basics.

- The instrument used
- More details on leaf chamber conditions…were the temps, humidity etc matched to ambient?
- Were sunlit or shaded leaves (or both) measured?
- What species were targeted?

L266-267. "Summer GPP was higher (-460 ± 112 g C m-2) compared to winter GPP (-291 ± 28 g C m-2)": -291 is higher (>) than -460. Check the manuscript for any other discrepancies when comparing magnitude and direction and ensure that the wording is correct.

L272-278. It looks like there is still hysteresis during winter (albeit less severe than in summer, Figs. 2 and 3). It might be useful to add 2 panels to Fig. 3 and show surface conductance light responses to help with underscoring the importance of stomatal regulation of C fluxes.

L336-337. Is soil respiration in the subsoil really that important to the integral over the whole profile?

L345-356. The paragraph starts out by rather definitively stating that "strong stomatal regulation" was the driver of diurnal hysteresis in NEE during summer and then becomes less clear and murkier. Seems odd to take this approach if it is was found that there was strong stomatal regulation of GPP and NEE. Revise.

L357-366. Hard to follow…especially the first sentence. Revise and clarify.

L367. "Canopy dynamics"…be more specific about what you are referring to.

L390-393. Is the temperature/moisture regime at Cumberland Plain the only difference versus the other sites in eucalyptus forests? Are all the sites at similar ages and stages of succession?

Figs. 2 &. It still looks like there is hysteresis in winter…is it that it is not statistically significant?

Fig. 4. Check units for apparent quantum yields (α). These values seem high.