

Anonymous Referee #2

Koerber et al. propose that they are validating eddy covariance flux measurements with other measurements including leaf area index and ecosystem respiration by using a new – and rather confusing – approach where they adjust the calculated light response curve.

I found the manuscript to be poorly organized and somewhat confusing. I don't agree that the approach helps any overestimate or underestimate of respiration or assimilation if the true value, or at least a value constrained by multiple other observations, isn't known. A visual schematic (more than a flowchart) of the light response adjustments that they advocate would be helpful; at the moment I can find little justification by the particular approach chosen. A formal uncertainty analysis is certainly needed. The discussion is short and poorly organized, perhaps a bit rushed. Quite a few statements in the Results would make for a compelling Discussion section, although other examples from the literature seem to have been picked almost at random. Altogether an analysis that is important or interesting as we learn that the Kok effect shouldn't be ignored, but ultimately unconvincing. The following minor points are designed to help the authors re-write the manuscript for resubmission.

The statement on line 19 is important but jumped out of the blue a bit. I'm assuming the authors mean the y-intercept of the light response curve?

The revised manuscript will be better organised because my understanding is now complete. The revised manuscript will walk readers through to my ultimate aim of calculating a correct ecosystem respiration R_{eco} for regressing against leaf area index (LAI). The y-intercept will be the heterotrophic soil respiration (R_h). Hence if I can derive a correct R_{eco} then we can use LAI to partition R_{eco} into heterotrophic and autotrophic respiration.

The end of the abstract seems to suggest that, because the numbers are kind of close, the approach must be good. A formal uncertainty analysis is needed. Also, with respect to significant digit reporting, 0.01 g per m² per year is 10 mg per m² per year. The eddy covariance method, and all other methods that I know of, are not that certain.

A new manuscript by Wehr et al.

(<http://www.nature.com/nature/journal/v534/n7609/full/nature17966.html>) is critical to cite in any new analysis of daytime respiration suppression.

The revised manuscript will contain a formal uncertainty analysis. Thank you for drawing my attention to the new paper by Wehr et al., 2016.

The first sentence of the intro on line 37 is already a bit of a mouthful. Try to remove every unnecessary word from the manuscript. It could be made much easier to read. (The second sentence beginning line 39 is not a step in the right direction.)

The revised manuscript will pay better attention to removing unnecessary words.

The net ecosystem exchange occurs over the time scale defined, not necessarily a day (line 40). Keep in mind on line 46 that this statement excludes CAM species.

Thank you for making sure the NEE timescale will be correctly defined and that we are not considering CAM species at night is an important oversight that will be stated in the revised manuscript.

On line 50, there are (valid) concerns about flux divergence in large-statured forests like tropical forests where the canopy is often warmer than the sub-canopy air space, which creates an inversion and suppresses mixing. On line 52 'decouples' is a word with strong meaning in the atmospheric sciences and the decoupling is – for lack of a better word – a bit dynamic. I'd reword these passages to note that the decoupling is common.

We will reword decoupling as you suggest in the revised manuscript.

On what basis is the van Gorsel et al. (2007) approach only valid for undulating terrain?

By this statement we meant we don't have to use the van Gorsel et al. (2007) approach because our site is not undulating. In the revised manuscript we will clarify.

With respect to the Heskel et al. (2013) paper, please also note the new paper by Wehr et al. (2016). With this in mind, the approach developed by the present manuscript is important. I just wish it was better-written.

I understand and I thank both of the reviewers for their constructive comments. My knowledge and understanding of eddy covariance, photosynthesis and carbon budgeting is now complete and the revised manuscript will be better-written.

There are other reasons why NEE and NEP diverge (noting that the authors did not adequately introduce this discrepancy above). Carbon released by, for example, soil could be taken up by leaves on its way out of the canopy. These fluxes beneath the sensors may not be adequately captured depending on the profiling system, causing further NEE and NEP divergence (I think that Goulden et al. 1996 delves into this issue.)

I think that the approach to use variable LAI is interesting but worry that pre and post-fire ecosystems have rather different heterotrophic respiration rates, making me question the validity of this approach (see line 89).

In the revised manuscript, pre and postfire will be analysed separately and together.

Hypothesis 1 is a straw-man hypothesis, not a null hypothesis.

Thank you, seen as hypothesis 1 is self evident, it will be removed.

Spaces between, for example, 20 and m on line 114 I know will be corrected during the copyrighting process but no harm in starting things off on the right foot. I like the description of mallee, but because this is a habitat unique to Australia please explain briefly for a global audience.

The revised manuscript will describe mallee in a global context.

On what basis was the ustarr threshold set to those values for different years? These seem reasonable but there are different ways of computing the threshold, which I personally feel is best applied seasonally instead of annually. Please justify the choice of the annual ustarr threshold.

The OzFlux processing calculates ustarr on an annual basis. I will incorporate seasonal ustarr in the revised manuscript if possible.

On line 171 what are the 'Kormann-Meixner constraints'?

The revised manuscript will now contain a better description like Kljun et al, 2003 as follows:

To calculate the effective sampling footprint of the tower we used the Kormann-Meixner method (Kormann and Meixner, 2001), employing a modified version of the ART Footprint Tool of Neftel et al. (2008). The Kormann-Meixner footprint determines the two-dimensional density function for an ellipse upwind from the tower. "The model is based on the assumption of a homogeneous and stationary flow over a homogeneous terrain. It can be applied to a large range of atmospheric stability conditions, stable and unstable. By assuming an independent vertical and crosswind dispersion the continuity equation is reduced to a two dimensional advection-diffusion equation, which are solved utilising power law profiles for the horizontal wind velocity and the eddy diffusivity. The predominant wind direction here is from the south-westerly quarter. For every 30 minute measurement of wind speed and direction, mixing and buoyancy parameters a "footprint" is calculated. Analysis of the seasonal effects exhibited a smaller footprint in summer which reflected the increased mixing in summer as well as the influence of more frequent winds from the northerly quarter. The annual average of the footprint area for 2014 displayed a distance from the tower of 500 m for at least 10% of the maximum contribution (1300 m for at least 1%).

Surprised in Fig. 1 that 10 W/m^2 was chosen as a nighttime threshold. Zenith angle is so much less-prone to uncertainty. Please explain the soil moisture adjustment on line 189 in a bit more detail; the flux response can go both ways in droughted vs. waterlogged conditions.

Thank you, soil moisture will be described in more detail and you are correct the adjustment can go both ways.

There is no justification for the approach on line 202 that PAR decreases respiration only above the compensation point.

Thank you, I will correct this incorrect part of the sentence.

If nonlinear weighted least squares was used, what was the weighting? It's been argued (e.g. Richardson et al. 2005) that least absolute deviation should be used for parameter fitting.

We will take the same approach as Stoy et al., (2006), where they noted that the error distribution of EC measurements may be better approximated as double exponential (Laplacian), rather than normal (Gaussian), and thus least absolute regression maybe preferred for estimating model parameters. For the same reasons as Stoy et al., (2006) we employed least-squares optimization here for comparisons with previous studies.

Furthermore, some months had PAR thresholds less than $1500 \mu\text{mol m}^{-2}$ and therefore the majority of our data will be in the bottom of a Gaussian distribution not requiring a Laplacian, peaked distribution.

On line 272 and elsewhere this is far too many significant digits for an eddy covariance measurement of net ecosystem exchange.

Thank you, the revised manuscript will carry out calculations correctly retaining the initial decimal places.

Lots of discussion in the results section, i.e. lines 278-281.

Lines 278-281 will be saved for the discussion.

There is no justification for saying that the OzFluxQC is underestimating ER on line 322. Or 358 for that matter. (also 404! On what basis do the authors call any of these estimates an over or underestimate when they don't know the true value! This is biased thinking.) The statement on line 325 is also vague.

Researchers measuring leaf respiration directly always refer to not incorporating the "Kok" effect will lead to overestimates of ecosystem respiration and gross primary productivity, see Heskel et al., (2013). I can understand AR#2's concerns and will clarify in the revised manuscript.

The statement beginning line 367 makes no sense in context. Why compare these ecosystems only to one in the UK? To add another (self) citation? The same goes for the comparison to Xu et al. 2004. Not incorrect but puzzling given the hundreds of flux studies. The discussion as a consequence is poorly-organized.

I will add more references besides mine.

The statement on line 426 also makes little sense (much of the manuscript makes little sense). Using nighttime flux observations from eddy covariance is also non-destructive. Table 2 would be more comprehensible as a figure or two.

Thank you, you are correct and I will remove this.