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Interactive comment on "Under a new light: validation of eddy covariance flux with light response functions of assimilation and estimates of heterotrophic soil respiration" *by* Georgia R. Koerber et al.

Anonymous Referee #2

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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

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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 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 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 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.

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 subcanopy air space, which creates an inversion and suppresses mixing.

On line 52 'decouples' is a word with strong meaning in the atmospheric sciences and

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the decoupling is – for lack of a better word – a bit dynamic. I'd reword these passages to note that the decoupling is common.

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

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.

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).

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

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.

On what basis was the ustar 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 ustar threshold.

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

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Surprised in Fig. 1 that 10 W/m2 was chosen as a nighttime threshold. Zenith angle is so much less-prone to uncertainty.

Please explain the soil moisture adjustment on line189 in a bit more detail; the flux response can go both ways in droughted vs. waterlogged conditions.

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

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.

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

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

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 extimates an over or underestimate when they don't know the true value! This is biased thinking.) The statement on line 325 is also vague.

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-organize.

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.

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