Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-466-RC1, 2018

Interactive comment on "Global, Satellite-Driven Estimates of Heterotrophic Respiration" *by* Alexandra G. Konings et al.

Anonymous Referee #1

Received and published: 5 December 2018

Konings et al. provide globally distributed estimates of heterotrophic respiration (Rh), both from satellite based observations and from a bottom-up scaling of an empirical model. The satellite based estimates in particular are very novel, and are obtained by combining atmospheric inverse estimates of net ecosystem production (NEP) with global photosynthesis (GPP) estimates informed by solar induced fluorescence data, multiple vegetation models, and empirically upscaled estimates. To combine NEP and GPP to get an estimate of Rh, the authors need to estimate global variations in carbon use efficiency (CUE). The use estimates provided by CARDAMOM, a simple empirical model of carbon fluxes with parameters constrained by global observations. The final estimate of Rh is then taken as ðĬŚĚh =ðĬŘžðĬŚČðĬŚČ−ðĬŚĄðĬŘÿðĬŚČ−(1−ðĬŘűðĬŚĹðĬŘÿ)ðĬŘžðĬŚČðĬŚČ.

The authors are to be commended for their heavily data-informed approach, which highlights the potential to use disparate observations to inform global estimates, not just test predictions. The results are interesting and the manuscript is very clearly written and no doubt will be of interest to the readers of Biogeosciences.

There are several limitations to the approach, however, and the estimates of Rh should be taken as a first pass of a promising approach rather than a reliable and informative quantification of the global distribution of Rh. As the authors note, the Rh estimates should not be used as a benchmark for other estimates, as their global quantified uncertainty is $\sim$50% of the mean flux. There are simply too many uncertainties, some quantified in this manuscript, and some not. Great caution should also be taken in using the approach to quantify trends over time.

The uncertainties stem primarily from the fact that both GPP and CUE are not known, but must be estimated themselves. Global GPP estimates vary a lot between approaches, and although the authors use an approach that combines SIF with DGVMs and upscaled GPP estimates, the relationship between SIF and GPP is poorly understood, and even the magnitude and spatial distribution of GPP has considerable uncertainty. The cited paper on which the GPP estimates are based, Parazoo et al. (2014) does a good job of assessing some of those uncertainties, but important sources of bias persist. For example, Parazoo et al. (2014) used the empirically upscaled GPP from Jung et al. 2012 to constrain the magnitude of GPP to roughly 120 PgC, but recent results of more updated empirical upscaling approaches from the FLUXCOM project (https://www.bgc-jena.mpg.de/geodb/projects/Data.php) show global GPP estimates vary from 108PgC (neural net based) to 125 PgC (Random forest based), each with an associated uncertainty of $\sim$8 PgC (standard deviation). It would be worth including an assessment of the contribution of this uncertainty to the global estimates reported here. The global estimates of CUE are also subject to large uncertainty, though the authors do a great job of assessing the impact on their results. In the absence of a global database of CUE and its seasonal variability, however, the uncertainty is difficult to quantify accurately. It would be worth highlighting in the abstract what these uncertainties indicate regard research needs to improve this approach. Detailed comments:

The annual totals for GPP and NEP should be given in the methods section to allow the reader to assess their relationship with the annual total Rh. Page 2, line 20 Although the authors are correct that heterotrophic respiration is relatively unconstrained, the same cannot be said for ecosystem respiration, particularly at night. Eddy-covariance observations provide a direct observation of ecosystem respiration at night at 100's of sites around the world. Consider rewording.

Page 3, line 20: 'is calculated as'

Page 6, line 16: 'To reduce error, all visual maps are presented after applying a 3 pixel by 3 pixel moving average smoother.' This does not reduce error, unless the error is randomly distributed around zero. Do you have evidence that there is no systematic bias in spatial distribution of the CMS-Flux predictions?

Page 6, line 17: "The NEP is a small number that is the balance of many larger components, so small errors in NEP could lead to large compensating errors in Rh." This is not clear. A small error in NEP should have little effect on the derived Rh, as NEP itself has a small role in the calculation especially relative to GPP, which is a very large number, and CUE.

Page 6, line 25: the uncertainty in total annual GPP from the Parazoo et al (2014) paper does not consider methodological uncertainty (see differences between methods in FLUXCOM). How would this affect the results presented here.

Page 12, line 24: Most plant traits can not be estimated from space, and it is difficult if not impossible to properly characterize the uncertainty associated with estimates of photosynthesis from space as there are no observations of ecosystem photosynthesis. The authors should show some restraint when trying to argue that estimates of photosynthesis, plant traits and Rh from space contain significantly lower sampling errors

than bottom-up estimates. Also please clarify what you mean by sampling errors here and how sampling errors relate to total uncertainty.

Page 14, line 10: measurements of SIF and estimates of GPP. GPP is not measured by TROPOMI.

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