Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-512-RC1, 2018

Interactive comment on "Deriving Photosynthetically Active Radiation at ground level in cloud-free conditions from Copernicus Atmospheric Monitoring Service (CAMS) products" *by* William Wandji Nyamsi et al.

Anonymous Referee #1

Received and published: 5 February 2018

General comments: The manuscript "Deriving Photosynthetically 1 Active Radiation at ground level in cloud-free conditions from Copernicus Atmospheric Monitoring Service (CAMS) products" describes a method of calculating the photosynthetically active radiation (PAR) during cloud-free conditions from widely available CAMS data using the libRadtran radiative transfer numerical model in combination with a spectral resampling procedure. Since PAR is an important variable to assess the carbon and water cycle of the terrestrial biosphere and is used in most process models and statistical models, this research paper and the research question described within is of high interest for

the research community. However, overall the manuscript suffers from a lack of novelty regarding the presented method. The actual new portion of the work is not much. While most of the method such as the spectral resampling has been developed and published already before, and the authors also refer to that appropriately. Yet, the new part is so little that it is in fact summarized in only 4 lines by the authors, in particular lines 190 to 193 in the manuscript. The rest is simple interpolation and regression analysis as well as an application of the libRadtran library functions in order to successfully relate PAR to other measurements which is not new at all even though, a specific dataset was used for the predictor variables. The new contribution seems rather minor for a publication in Biogeoscience and for that matter for a research paper. I suggest enhancing your study to non-cloudy conditions as alluded to at the end of the manuscript (lines 281 to 289), for instance. Since the amount of PAR at the surface is clearly related to the total irradiation, cloudiness and atmospheric variables such as aerosol optical depth and other meteorological conditions this seems feasible and would more of an advance in this field of research over previous studies. I therefore suggest a major review/overhaul of the manuscript.

Specific comments: 1. In line 44 to 54 the authors give only few references. Due to the importance of PAR and the sparsity of its measurements similar research has been going on for many years. Please include more studies that also aim to model PAR by using atmospheric measurements as predictor variables. Also, the performance of the approaches presented in those different studies should be compared to your findings to put your results into context of this ongoing research in his field.

2. In the paragraph starting in line 44 the authors also discuss the quality of the PAR measurements and also address the error of the PAR devices. This is a bit too short given the complexity of this issue. The error of the ground measurements, when used a reference, are of outstanding importance for the model presented. There are studies already published that address the errors of PAR measurements in practice at the ground level. Schmidt et al. (2012) ,for instance, give an average error of 3% for PAR

measurements among a large network that is, however, affected by a large standard deviation of errors observed between PAR sensors at different sites and a reference system. A PAR sensor's response to the natural spectrum needs to be addressed briefly. Although the authors describe the quality control of the measurements used, the fact that measurements might always show some deviance from the real value should be addressed on a certain level in a manuscript that focusses on PAR. The response of the very common PAR sensor (LICOR quantum sensor) that is also used in this study is not perfect for the natural spectrum but adheres to an optimized spectral curve. Please address this issue if the performance of your approach is measured by the correlation with those ground measurements.

3. The sentence in lines 75 to 78 is not quite clear, neither is the approach described. Although a reference is given, please rephrase and add a brief description about the method. How many narrow bands are used in your case? The authors mention "one or more". I would expect the model performs better the more band bins are correlated and used. How strong do the correlations have to be? The summary is too simple to understand the process. Later in section 3.2 the method is explained in more detail, yet the statement "A total of 19 NBi is sufficient" comes out of nowhere. Please give information as to why 19 can be considered sufficient. Please also merge these two sections and explain this part of the method only once but thoroughly.

Details about the origin of the function parameters (slope and intercept) are not given although it seems to be the central point of the method. How many comparisons at which stations were used to get these parameters of the affine functions? A statement like "The choice of these NBi has been made on an empirical basis." As give in lines 188 and 189 is not a sufficient description of the process. Please elaborate.

4. Figure 3 gives the differences between the measured (at the sites) and CAMS data. While the upper panel gives a ratio, the lower panel gives the absolute difference divided by 100. It is not clear why the difference given in the original data units should be divided by 100 (unless one wants to make them appear much small than they are).

Also, please provide unit labels on the vertical axes showing the differences of the various variables in the lower panel.

Technical corrections: The sentence in line 75 starts with a dot. Please correct (delete). In line 163 there is a dot missing at the end of the sentence. "...any location and any time." Line 167: Please spell out AFGL data set. The acronym appears for the first time here and not any reader might be familiar with that dataset and its origin. Line 137 and 138: The sentence reads "...cloud-free instants instances." Please correct.

Reference used in comments:

Schmidt A., C. Hanson, W.S. Chan, B.E. Law (2012): Empirical assessment of uncertainties of meteorological parameters and turbulent fluxes in the AmeriFlux network. Journal of Geophysical Research 117, G04014, doi:10.1029/2012JG002100.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-512, 2018.