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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.

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First of all, we thank Referee #3 for these constructive remarks on this topic. The authors believe that they have understood the concerns of the referee. All remarks have been taken into account for revising a part of the text following recommendations of the referee.

The manuscript submitted to Biogeosciences titled, "Deriving Photosynthetically Active Radiation at ground level in cloud-free conditions from Copernicus Atmospheric Mon-

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itoring Service (CAMS) products" by Wandji Nyamsi et al. presents one aspect of a larger project developing radiation modeling for the bottom of the atmosphere. This includes a sequence of recent articles lead by the current lead author. The specific objective of this study appears to be testing the use of CAMS atmospheric products for inputs to an existing numerical radiation transfer model. The model itself is cited, as is the spectral resampling technique used here, referred to as the 'new method'. What is new in THIS study seems to be the use of the CAMS products; columnar aerosol optical depth (AOD), ozone (O3) and water vapor (H2O) inputs to the radiation model. The general accuracy of the simulated cloud-free PAR is impressively high at 7 locations with strong regional differences likely in AOD and H2O. However, a number of questions seem to remain that are central to the research objective. These include, what causes the tendency of the model to perform well at lower PAR levels and over-estimate at higher values (e.g. Fig. 2), and cause slope>1 in general? Did this appear in previous evaluations of the model? Can the differences in accuracy found between the seven stations be used to inform the cause of inaccuracy? The model over-estimate appears to be correlated with a tendency of the model to overestimate under low zenith angles and/or under low AOD. Why is this? Is the cause of this inaccuracy related to the CAMS data set or is it the radiation model itself? The answers to these questions seem important to the objectives of this study, because they should provide leads towards future model improvements. As presented, I find the manuscript needs to overcome two major obstacles currently inhibiting its potentially useful contribution to Biogeosciences.

Answer: Thank you for this comment. We feel that apparently we were not able to emphasize clearly enough those parts of the method that have been already published from those that are discussed and published first time. We appreciate this comment and we have tried to clarify these issues in the revised manuscript. Moreover, we want to stress that the core objective was to validate this approach (at PAR range) against ground-based measurements. This is done first time in this manuscript and is entirely new contribution.

The method we described is a combination of three parts: (1) use of CAMS products to describe the atmospheric state, (2) irradiances of correlated–k approach over only eleven bands covering the PAR wavelengths by the means of libRadtran and (3) the resampling technique for computing PAR estimates. Only the third part has been previously published by ourselves. The goal of this current manuscript is to focus on the entire approach (e.g. also including the other two parts) and to present the ground-based validation.

Since estimation of PAR under cloud-free conditions at any time and place is an important first step in calculating PAR in all-sky conditions, in this paper, we concentrated first on these conditions.

Based on the above referee comment, we have re-written several parts of the text accordingly. For instance, a part of introduction is re-written as follows:

"This resampling technique has not been validated in operational conditions, i.e. using available inputs to describe the atmosphere in cloud-free conditions and the properties of the ground, and tested against ground-based measurements. This paper is making this step forward and aims at describing and evaluating the entire method when tested against measured PAR in cloud-free conditions."

Comment 1. Surprisingly, little information is provided about the CAMS product, especially accuracy assessments of AOD, O3 and H20 vapor products from past studies. This context is necessary to assess the efficacy of using it for inputs to radiation modeling. It may also provide some useful insight into the cause of the overestimate of PAR in simulations found here.

Answer: Thank you for this comment. We have included a discussion part, newly added in the revised manuscript where we discussed about the accuracy of CAMS products as inputs of the method. We have re-written the relevant part of the text accordingly.

Comment 2. Analysis doesn't seem to test the CAMS input specially, but rather the

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CAMS driven radiation simulations against observations. This could be done by comparing these simulations to those using standard (e.g. monthly average) AOD, H20 and O3 for a site. Sensitivity simulations for variations in these input variables would also help shed light on the sensitivity of model accuracy to each of these inputs (or discussed, if reported elsewhere). My guess is that this shows AOD is key. I believe AOD is also measured at the observation sites? Could the CAMS product be evaluated to determine if, for example, the overestimate could be related to a bias in CAMS AOD?

Answer: Thank for these constructive comments. The remarks have been taken into account for revising a part of the text following recommendations and suggestions especially in the Discussion part which has been newly added in the revised manuscript.

Minor comments:

Specific objective of this study is not really clear. Nor is it clearly separated from the other model developments in this sequence. There seems to be significant overlap as written.

Answer: Thank you for this remark. We have re-written the relevant part of the text accordingly.

It would be helpful to the readership of this journal for the authors to articulate more fully in the introduction and discussion sections, the implications of these methods and results to biogeoscience research in general.

Answer: Thank you for you remark. It has been done as requested in the introduction.

Methods, why not include the Modeling Efficiency Coefficient (Nash and Sutcliff 1970)? It's a very direct test of model performance, including bias and random error. There is no discussion section. This would be a useful place to investigate answers to the questions above based on the evidence presented in results.

Answer: Thank you for this valuable suggestion. The Modeling Efficiency Coefficient is very nice test mostly used to evaluate the performance of hydrological models. While

we want here to evaluate performance of a solar radiation model, the statistical indicators, namely the bias (mean of the differences), the root mean square of the differences, these same quantities but in relative values, the mean value and the correlation coefficient, the ratio and absolute differences are vastly used in many studies in the literature for such model. For this study, we think these statistical indicators are more appropriate.

The manuscript needs to be carefully edited for small but frequent lapses in grammar, or clarity in expression.

Answer: Thank you. Done as requested.

L128: What is "fine rock"?

Answer: Thank you. We have replaced these words by “small rock”.

L137: End of sentence unclear-‘instant instances’ is confusing alliteration. Any way to rewrite using other words such as ‘periods’, ‘frequency’. . .?

Answer: Thank you for this remark. We fully agree with you. We have used the word “periods”.

In discussing Fig’s 2 and 4, What is an ‘identity line’?

Answer: The idendity line is also called line of equality or the 1:1 line. In accordance with the relevant figures, we replaced “identity line” by “1:1 line” in the text.

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