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Comment on essd-2021-209

Anonymous Referee #1

Referee comment on "VODCA2GPP – a new, global, long-term (1988–2020) gross primary production dataset from microwave remote sensing" by Benjamin Wild et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2021-209-RC1, 2021

This study produced a global GPP product (VODCA2GPP) using satellite-based vegetation optical depth (VOD) datasets from 1988-2020. Basically, the authors used the method proposed by Teubner et al. (2019), and evaluated the product accuracy based on the measurements of multiple eddy covariance towers globally. In addition, the authors compared the spatial and temporal differences of VODCA2GPP with MODIS and FLUXCOM GPP products. There are several main concerns in my mind to make me suggest rejecting the manuscript because of the important and inherent flaws of VOD data.

First, VOD indicates the vegetation biomass, a large carbon pool, but the GPP indicates one of the carbon fluxes, much smaller compared to biomass. Teubner et al. (2019) and this study used the changes of VOD to simulate GPP, which requires the high performance of VOD to indicate biomass changes, a huge challenge. GPP is sensitive to leaf area index or leaf biomass, not wood and root biomass. However, leaf biomass is quite small fraction compared to other two components. It is obviously using VOD to estimate GPP has much large uncertainty. From the validation results, we found low model performance of VODCA2GPP shown as fig. 3a. VODCA2GPP only reproduce spatial variations of GPP about 25% (pearson r = 0.515), and I can’t trust the information provided by VODCA2GPP as such low performance. In addition, limited by VOD products, the spatial resolution of VODCA2GPP is only 0.25°, which is not far enough for scientific applications, as MODIS Landsat GPP has reached to 30 meter spatial resolution.

Second, I am confused to model algorithms proposed by this study. The algorithms of this study look different a little bit with those of Teubner et al. (2019). Equ. 3.2 represents the model algorithms, the authors added the third term the temporal median of VOD (mdn(VOD)) derived from the complete time series which serves as a proxy for the landcover. Unfortunately, the authors did not explain how mdn(VOD) can indicate landcover. And I also concern why the authors need this term for simulating GPP. Besides, the authors separated GPP into Ra and NPP components, so the model can provide the simulations of Ra (as least maintenance respiration) and NPP respectively, which may help us to judge if the structure of model is reliable. However, the manuscript also missed this kind important information. Another important model flaw, if the changes of biomass is equal to GPP. Obviously, the answer is NO! When the biomass lost partly at the 0.25 degree pixel, the GPP should be positive assuming the rest vegetation in this pixel still live, but lost biomass may larger than actual GPP contributed by the rest vegetation. Then, VODCA2GPP product may produce negative GPP.

Third, as the dataset description paper, I majorly pay much more attention to validation of dataset. The authors actually conducted very weak model validations, only validate the performance for reproducing spatial variations of GPP (low performance as the above comment). I would like to see if the product can reproduce the seasonal and interannual variations of GPP against the measurements of eddy covariance towers. In addition, the authors seem to hide the magnitude of global VODCA2GPP GPP. From fig. 6, VODCA2GPP estimates for global mean GPP intensity is almost twice than that of MODIS. If I am correct, the global GPP derived from MODIS is 108 Pg C yr-1, thus the global magnitude derived from VODCA2GPP may reach to 200 Pg C or larger? This is a new and the maximum estimates of global GPP that I ever knew. The authors need provide enough robust evidences to prove this number is correct, else I can't trust the VODCA2GPP, which will lead us to a wrong way.

Forth, I felt disappointed and the writing is very poor. There are some low-level mistakes in writing and format. All sections of the manuscript look very rough. Especially, the introduction and discussion are meaningless mostly, and the authors kept some sentences to repeat rather than discussing important scientific questions. Several paragraphs are very short consisting of only 2-3 sentences, which failed to provide useful information. Feeling so bad when I was reading.

Some minor comments.

line 49-54: this paragraph is not necessary.

it seems the subtitle of "4. Results" missed.

There are no GPP values over the desert area in fig. 1c, but there are uncertainties of GPP in fig. 2. These two maps should keep constant for non-vegetated area.

Fig. 3 shows the poor performance of VODCA2GPP compared to fluxcom and modis products. So why do we need VODCA2GPP product?

4.2 section. The authors only compared temporal variability of VODCA2GPP with modis and fluxcom datasets. How about VODCA2GPP to reproduce the interannual variability of GPP compared to fluxnet observations. Actually, the comparison with fluxnet observations is more important for this data description paper to let the readers and potential users to know if VODCA2GPP can reproduce the long-term or interannual variability of GPP.

4.3 section. Why did the authors add the trendy simulations here, not compared previously?

4.4 section. Totally confused. Sometimes, the authors made comparison from 2002 to 2016, and otherwise 2003-2015? Can not understand the purpose of the authors.

Line 396-399: can't accept this explanation for poor model performance. The sparse observations in arid and semi-arid regions will reduce the performance of modis and fluxcom, but VODCA2GPP showed the obvious bias.

5.2 section. What is the objective of this section? I did not get any meaning points. The manuscript seems to be rough at the initial stage, and the authors need much more work to improve the writing.

5.5 section. I can't agree to this point. There are many factors may enhance the global gpp, how the author can claim the increasing trend imply the CO2 fertilization? There are many conclusions that the authors reached without robust evidences. By the way, this section is basically repeat of results.

5.6 section. Again, a meaningless paragraph and section. It looks like a conclusion or summary.