Open Access Earth System Science Data Discussions

Interactive comment on "The Global Long-term Microwave Vegetation Optical Depth Climate Archive VODCA" *by* Leander Moesinger et al.

Anonymous Referee #1

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This manuscript presents the VOD Climate Archive (VODCA), a set of combined VOD records from several spaceborne sensors that are statistically matched to each other to cover a longer period. As such, the VODCA is very similar to the statistically matched dataset from Liu et al. (2011), as acknowledged in the paper. The VODCA differs from the Liu et al dataset in only two ways: a) it uses a slightly different form of cdf-matching to bias-correct records from different sensors and b) different versions of the data are produced for Ku-band, X-band, and C-band. Another key improvement over the Liu et al (2011) dataset, not mentioned in the paper is that this new VODCA dataset is publicly available, which should greatly increase its utility to the scientific community. While I applaud that this group is sharing this data, and while statistically matched, long records of VOD can have many applications, the authors need to do more to clarify how

this dataset is different from the Liu et al dataset. Aside from this, I have several major concerns with the underlying dataset. My specific concerns are detailed below.

Major concerns:

1) The use of regression-based cdf-matching is argued to be an important component of accurate bias-correcting. This may well be, but as currently written the paper is not convincing. The analysis in Figure 2 is not clear on this front without a label to the color map. Please include a scale bar on this figure. Even if the numbers are normalized, how much of a difference does it make? 0.5% 1%? 10% 50%? Also, it would be useful to include an additional figure that shows the exact difference between the: piece-wise CDF-matching and least-squares methods, either by replacing one of the panels or adding it as a third panel. More important, though, is the fact that no comparison between the dataset of Liu et al and this new dataset is created. In the actual practice of the VOD dataset, how closely related are the two datasets? Are there any changes induced to say, the trends? What about other statistics, or simply some sense of say, how often the VOD differs by more than some small threshold (0.05 or so) as a result of this change? Such information needs to be included in several figures and is crucial not only to judge the improvement created by this new dataset, but also towards understanding the quality of the large number of papers that have been written analyzing the Liu et al (2011) dataset.

2) Figure 7/text on page 14: The values mentioned in the text here are pretty low Spearman correlations so it is difficult to test if the LAI anomalies line up with the VOD anomalies or not. Furthermore, this analysis in and of itself does not indicate successful bias removal. It would be more useful to focus instead on whether there are any changes at the breakpoints in when different datasets are available (which are known a priori) rather than comparing across the entire record. Please use the methods developed for soil moisture in Su et al, Geophysical Research Letters, 2016 ("Homogeneity of a global multisatellite soil moisture climate data record") to test for breakpoints.

3) I strongly urge the authors to reconsider the choice not to include daytime retrievals in the VODCA (page 4, line 20). While the idea that daytime retrievals are more error-prone because of greater differences between soil and canopy temperature is common in the microwave radiometry community, few studies have been done document the extent of this error. Recent results suggest, for example, that PM soil moisture retrievals are not always more error-prone than AM ones (particularly under densely vegetated conditions), see Fan et al, Remote Sensing, 2015 ("The Impact of Local Acquisition Time on the Accuracy of Microwave Surface Soil Moisture Retrievals over the Contiguous United States"). Given the significant potential for diurnal changes in VOD to be useful for studying vegetation water stress (see Konings and Gentine, Global Change Biology, 2017 ("Global variations in ecosystem-scale isohydricity"), such a dataset could be quite useful. A flag could still be included for the nighttime data to suggest greater uncertainty.

4) Relatedly (page 7, line 14), the above-cited Konings and Gentine paper has shown there is a significant expected diurnal cycle in VOD (see also Konings et al, Geophysical Research Letters, 2017 "Active microwave observations of diurnal and seasonal variations of canopy water content across the humid African tropical forests" for the active equivalent of this, with more complete diurnal measurements). As such, presenting the data to be "resampled to a specific time" as in page 7, line 14, is misleading. At the very least, the data should be presented as averaged over a certain period. If that is the case, and if the authors really insist on not using daytime data, it would still be cleaner to just present it as a day-long average.

5) Little literature exists on whether sensor differences are really more significant than algorithmic differences. This may explain why SMOS and SMAP baseline retrievals were found by the authors to have little consistency; those retrieval algorithms are fairly different – not something specific about L-band frequency. L-band VOD has been shown to have significant utility over X-band and likely C-band (see Brandt et al, Nature Ecology and Evolution, 2018 "Satellite passive microwaves reveal recent climate-

induced carbon losses in African drylands"). While I recognize that it may genuinely be impossible to create an L-band product using the methodology employed here (without re-running the LPRM on SMOS and SMAP so that a common algorithm is present), a more detailed treatment should be provided than just lines 21-23 on page 3. Please make it clear that it is not possible for you to create the L-band product, not "does not warrant a product for it", which suggests L-band data is not useful. Also, can you include the low temporal correlation in a supplemental plot?

Other presentational issues:

1) Section 3.1 Regarding the 2 AMSR2 C-band channels: was any statistical evaluation done to see how different the retrievals from the two channels were, when taken in isolation?

2) Page 9 lines 2-3: Please provide more information on how the bin sizes are chosen

3) Page 10 line 11: How often does removal of such unphysical values happen? This is important information as these values are made unphysical as a direct consequence of the cdf-matching variant employed here.

4) Page 10, line 1-2: How many observations is deemed enough? Please include in text (not just Figure 3 for clarity. Relatedly, Figure 3 is not consistent with the text in Section 3.2.4 (since for example, the figure pseudo-code does not mention the use of the first and last two years). This makes it actively confusing – please make sure Figure 3 is fully complete

Minor textual issues:

The abstract would be cleaner if it was all one paragraph

Page 2 line 9: please fix the capitalization of how VOD is written out

Page 6 line 9 "us" instead of "use"

Page 7 line 4: please reword

Page 7 line 22: please include a comma

Figure 4b: How are different VOD datasets combined in this figure if this is "the original VOD data" as stated in the legend?

Page 12, line 2: Please define TC, SV, and BG

Page 17, line 10: Contributing

Page 19, lines 29-31: Is a flag included to warn the user of this?