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

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General comments

The authors present alongside this paper a new long-term data set of vegetation optical depth (VOD) at C, X and Ku bands constructed based on the statistical merging of available products spanning the last three decades. They first describe the motivation and their proposed approach, and then detail some aspects of the data set such as its spatio-temporal coverage and its error characteristics with respect to the individual products. Since direct validation of VOD data is not possible due to the lack of in situ measurements, they provide an indirect validation by means of a spatio-temporal comparison to two optically-derived measurements: Leaf Area Index and Vegetation

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Discussion paper

Continuous Fields data.

I would like to first thank the authors for their initiative of creating a new data set of multi-frequency long-term VOD and making it freely accessible. I read the manuscript with interest. The presented data set has enormous potential to monitor global changes in canopy water and contribute to a wide variety of ecological studies. However, I found some major aspects need to be further detailed or analyzed in the manuscript to support the usability and robustness of the product. My main concern is that the manuscript does not contain a full characterization of their matching methodology and, as a consequence, it is not clear how the flags provided correspond to the quality of the retrievals and how the final spatio-temporal resolution of the products is impacted. This is key information for potential users. Also, some design criteria need to be further justified or discussed in the text and their validation approach, while convincing, does not show the value of having multi-frequency VOD. The novelty and potential applications of the provided data set need to be further supported by references to previous related works.

My main recommendations follow:

1. Their approach for the merging builds from the one used for the ESA CCI Soil Moisture product and the previous long-term VOD product from Liu et al., 2011, with improvements to make it more robust to the presence of outliers. The improvements shown with respect to the previous version is not convincing. What is the numerical range of the colorbar in Fig. 2? Can the authors also show results with real data? Also, the authors say (page 8, line 31) they dynamically increase the step size of the percentiles “if only a few” observations are available. It would be important to be more specific here and show how the method is sensitive to the choice of this parameter. In general, an improved characterization of their matching approach is needed.

2. The authors report there is a flag indicating the matching method (page 10, line 8) and a flag indicating which sensors contributed to a measurement (page 11, line

2). It would be very useful if they could relate those flags to the quality of the final product and make recommendations to the user. Perhaps the authors could consider dedicating a specific section of the paper to their quality flags and assessment.

3. I would strongly recommend the authors to consider including the daytime observations to the data set. Although it is well-known that daytime retrievals are expected to have a higher error than nighttime ones due to the thermal equilibrium assumed in the inversion, the difference between day and night canopy water have been shown useful for certain science studies (e.g. see Konings & Gentine, "Global variations in ecosystemâĂŘscale isohydricity", Global Change Biology, 2016). Also, their combination could be potentially useful for some applications to enhance the temporal coverage.

4. The validation does not show the value of the multi-frequency retrievals, nor discusses in detail their differences with respect to the optical indicators they selected. The authors should elaborate more on their results with focus on the different bands and perhaps consider a comparison of the sensitivity of the different VOD to biomass (e.g. see Nemesio-Rodríguez et al., biogeosciences, 2018).

Specific comments and recommendations:

1. Page 1, line 10. The authors should introduce in the abstract the previous long-term VOD data set and clarify the novelties of their newly presented data set, i.e. frequency-specific VOD, extended period, improved matching.

2. Page 1, line 24. Is the trend measured by all frequencies? Are there any differences? It would be nice to complement the validation and include the value of having frequency-specific VOD here.

3. Page 2, line 2. The authors could (at least) indicate how the multi-frequency VOD could actually complement optical measurements (e.g. canopy water vs. greenness)

4. Page 2, line 14. Additional references are needed in the intro and the discussion re-

garding multi-frequency VOD estimates and sensitivity to different parts of the canopy. I point out two articles hereafter, but recommend nonetheless the authors to do a bibliography search:

F. Tian et al., Coupling of ecosystem-scale plant water storage and leaf phenology observed by satellite, nature ecology and evolution, 2018

N. Rodríguez-Fernández et al., An evaluation of SMOS L-band vegetation optical depth (L-VOD) data sets: high sensitivity of L-VOD to above-ground biomass in Africa, biogeosciences, 2018

5. Page 3, line 23. Do the authors mean there is a low temporal correlation of SMAP and SMOS VOD products? Which products? Please, provide appropriate references or supporting material for this statement. Perhaps the addition of L-band could be directly included as future work, latest products from the two missions (SMAP MTDCA and SMOS-IC for instance) seem to agree well.

6. Page 4, line 10. A reference to the tau-omega model is needed. Please include: T. Mo, B. Choudhury, T. Schmugge, and T. Jackson, "A model for microwave emission from vegetation-covered fields," J. Hydrol., vol. 184, no. C13, pp. 101–129, Dec. 1982.

7. Table 1: It would be interesting to add ascending and descending times for each sensor as well as their incidence angles, spatial and temporal resolutions. The authors could perhaps add a little discussion on the impacts of mixing the different times and observation geometries (spatial resolution, incidence angle, etc).

8. Page 4, line 24: it is unclear how the different data sets can be accessed (webpage?). Please, specify which ones are available and which ones are not (perhaps on Table 2 also).

9. Page 9, Line 25. I understand AMSR-E is used as a reference for having the highest overlap. But perhaps AMSR-2 could also be chosen for being a more advanced instrument with improved capabilities, or also a modeled VOD could potentially be used.

Please, include a discussion for this choice (or provide a reference) and why it was chosen over the alternatives.

10. Page 6, line 1. Please, indicate how to access the ancillary data used in the corresponding subsection (LAI and VCF).

11. Page 6, line 4. What do the authors expect from the comparison of VOD and LAI? A rationale of why they chose LAI over other indices (e.g. NDVI, EVI) and whether they expect a higher correlation with any of the specific VOD products is needed.

12. Page 7, line 8: How is the VOD climatology from AMSR-E derived? Please provide details.

13. Page 7, line 26. I agree with the authors that negative VOD retrievals are physically impossible. However, they are most probably linked to uncertainties/simplifications on the physical model used in the inversion, and their direct truncation may lead to erroneous trends for specific areas. One alternative could be to let the user truncate the values after temporally averaging the data set according to the needs of their study. This is the procedure followed for instance in the SMOS-IC product (Fernández-Moran et al., remote sensing, 2017). I would ask the authors to consider this option or at least, mention it in the discussion.

14. Page 8, line 2. How different are the retrievals from the two C-band channels? Again the authors include a flag but this flag is not useful if it is not related to a quality indicator or any further recommendation is given.

15. Page 8, line 22. I infer from the text that there is a need to a new cdf-matching technique due to the presence of outliers in the VOD data set. Could this cdf-matching also improve the VOD data in Liu et al 2009, 2011? Could this cdf-matching improve the soil moisture merging within ESA CCI? The authors could perhaps elaborate on this, to better motivate the approach.

16. Page 10, line 11. Does this happen very often? Could this be one aspect to

improve to increase coverage?

17. Page 10, line 18. Have the authors tried with the median statistic? It is less sensitive to ouliers.

18. Figure 3. I do not think this figure is necessary.

19. Figure 4. What is the dominant vegetation in the chosen pixel? Perhaps the author could also include an example of time series in which TMI is also used, for completeness.

20. Page 12, line 3. The authors could also perhaps refer to the L-band VOD spatial patterns, which are consistent and correlate well with canopy height (e.g. Konings et al., L-band vegetation optical depth and effective scattering albedo estimation from SMAP, Remote Sensing of Environment, 2017).

21. Figure 6. It is hard to see the seasonal patterns. The authors could perhaps consider showing only the period 2002-2017 (or even shorter)

22. Page 15, line 1. It is unclear how the authors measure the spatio-temporal coverage. Is Fig.8 showing the fraction of days each month as stated in the label? The final temporal resolution shown in the figure and referenced in the text above is unclear.

23. Page 15, line 14. What do the authors understand by a “CDF-matching failure”? could it be for one specific reason (e.g. see comment #15 above), or several? please, be more specific.

24. Page 19, line 29. This advice is helpful but it could really be useful and applicable if converted into a criteria that contributes to a quality flag. There is clearly a need for a quality flag.

25. Fig. 12. I would suggest to include subfigures f and g into a separate figure, for clarity.

26. Page 21, line 17. Only Ku band spans three decades, this sentence is a bit

misleading.

27. Page 21, line 21. From section 4.2., it is unclear that the resulting VOD data sets provide observations “on a daily basis”.

28. Page 21, line 24. The authors could perhaps consider mentioning at some point in the manuscript that their work is particularly relevant in the context of the prospect launch of the multi-frequency candidate mission Copernicus Microwave Imaging Radiometer (CIMR, www.cimr.eu).

A few technical corrections:

Page 6, line 9: should be “Use”

Page 6, line 11: typo “short vegetation all shorter vegetation”

Page 7, line 4: typo “for the product of each product”

Page 7, lin 21. Reference to Nijs et al (2015) seems to be missing.

Page 17, line 10: typo “is still positively contribute”

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