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Interactive comment on "The high sensitivity of SMOS L-Band vegetation optical depth to biomass" *by* Nemesio J. Rodríguez-Fernández et al.

Anonymous Referee #1

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The authors present an interesting work addressing the sensitivity of SMOS L-band vegetation optical depth (VOD) to biomass. The study is centered in the African continent and employs the three available data sets of SMOS VOD data currently available (L2,L3 and IC). As independent data sets, the authors chose four above-ground biomass sources (AGB), lidar-based tree height, MODIS vegetation indices and cumulated precipitation. The differences of the three SMOS products are clearly detailed and discussed. The analyses relating VOD to the independent data sets are performed in a scientifically sound manner. However, sometimes the pretensions of the authors with respect to the obtained results are too high, specially taking into account that they are only using one year of SMOS observations. Also, they report a higher sensitivity

to AGB at L-band than at higher frequencies (K/X/C-bands), but they do not present a clear comparison of the different data sets. Therefore, I recommend this manuscript for publication after addressing the following issues:

1. The results should be re-organized in a more clear and structured way to facilitate readability and comprehension. There are many general references to relevant results in supplementary material that should directly point to a specific figure or table and be commented in the main text. Some choices made in the analysis and presentation of results are unclear (e.g. the stratification per land cover in two biomes should be further justified) and it is hard to follow the results presented in the main and the supp. material.

2. It is unclear how the authors obtain the results plotted in Fig. 4. It seems they do not use K/X/C-VOD data from year 2011 for a fair comparison to the presented results with L-band and NDVI. Instead, they show the results from Liu et al 2015, which are based on VOD time series from 1993 to 2012 and a significantly different approach. I believe the data is not directly comparable and the result presented in the figure is therefore misleading. I strongly suggest the authors to either a) include the K/X/C-VOD data from the same study period (yearly average) and detail in the methods or b) focus on the comparison of L-band VOD and NDVI and AGB. I would particularly encourage the latter. Also, the results on Fig 4 could be shown for the four different AGB data sets used in the study, for completeness.

3. The title is too ambitious and general. The focus is clearly on SMOS L-band VOD and biomass, but the results presented (using 1 year of observations over Africa) do not support the use of the words "high sensitivity". I would recommend the authors to provide a more specific title, more representative of its contents.

4. Section 5 "Discussion" is too short. Results are already discussed in Section 4, and Section 5 adds a brief overview and a comparison to literature studies. I would recommend the authors to re-organize the manuscript and include the content of Section 5

either in the results or in the conclusions as "Discussion and Conclusion".

Here is a list of more specific comments and recommendations:

1. Abstract, last sentence. Consider changing "index" by "indicator"

2. Page 2, lines 9-11. In presence of vegetation, part of the soil emission is absorbed and scattered. There are two microwave vegetation parameters that are used in the physical model to account for the effect of vegetation: the vegetation optical depth and the single effective scattering albedo. The authors should introduce here the albedo parameter, or at least mention it.

3. Page 2, line 16. Specify how "thick" is the vegetation layer that microwaves penetrate, and introduce here a comparison between frequencies (this is later briefly discussed in line 30). Is the soil emission from tropical and boreal forests reaching the satellites operating at C/X/L bands? Add references and a brief discussion to support and clarify how the different frequencies are complementary.

4. Page 3, first paragraph. Literature on SMAP L-band VOD is totally missing and should be added. For instance, a global comparison of SMAP VOD to lidar-based vegetation height is reported in Konings et al. 2017. A.G. Konings, M. Piles, N. Das, D. Entekhabi, L-Band Vegetation Optical Depth and Effective Scattering Albedo Estimation from SMAP, Remote Sensing of the Environment, Vol. 198, pp 460-470, 2017.

5. Page 3, line 17. It should be relevant to (at least) mention briefly the difference between active and passive microwave sensing of vegetation.

6. Page 3, line 25. Please, add a reference to support that the quality of the ascending data is better than the descending. I would "a priori" recommend to use both to increase coverage.

7. Page 4, line 7. SMOS is first introduced as a full-polarization radiometer but here it is stated that only dual-polarization measurements are used in the retrievals. Why? Too much information to constrain retrievals? Consider including a reference here.

8. Page 4, line 14. The authors mention that previous L-VOD retrievals are used to constrain new retrievals. How many closest retrievals? Please, be more specific.

9. Table S1. It would be relevant to include how albedo and soil roughness are computed in the different products. Also, please detail previous retrievals. ISEA should be ISEA4h9.

10. Page 5, line 6. Mention how SMOS-IC is initialized and refer to Table S1.

11. Page 5, line17. A reference is needed for Worldclim data.

12. Page 5, line 21. change "sential" by "essential" (?)

13. Page 5, line 24. Consider adding a refernce for EVI and its main differences to NDVI.

14. Page 5, line 7. Words "In a second step" are used in lines 5 and 7.

15. Page7, lines 15-16. It seems here that the authors hypothesize the AGB data set derived from L-band SAR is probably the more appropriate and therefore they restrict the study to its coverage (i.e. Africa). However, best results are obtained with Saatchi. The authors should better elaborate on why it is important to use this data set and reformulate this sentence.

16. Page 7, line 24. Please, specify which parameter is used to select the lower values of the cost function (chi-square?)

17. Page 7, line 28. There are different criteria to filter out the quality of SMOS observations. As a common practice, the DQX parameter is used. However, the authors here propose to use the Chi2 parameter larger than 3. A reference should be added to support this criteria.

18. Page 7, line 30. It would be important to show a map with the final number of samples used per pixel, after the filtering criteria is applied. It would also be relevant to show a map of the standard deviation of the estimates (apart from the average on Fig.

1). This is critical, since the study is based on a final comparison of spatial maps.

19. Page 8, line 3. The authors average on a yearly basis since they chose only one year of observations. A seasonal study would be interesting, but of coarse more years would be needed. The choice of using only one year of SMOS observations should be further justified. Also, the impact of using one year in the results should be (at least) discussed later in the manuscript.

20. Page 8, line 13. Please be specific as to the contents you are referring to in the supplementary information.

21. Page 4, line 15. It would be relevant to detail the function used for the fitting in the main manuscript.

22. Page 8, line 22. Please, detail "the remaining static data sets" and comment on Figure 1 (e.g. main visual differences between the VOD products and the AGB ones)

23. Figure 1. The reference to Mermoz is missing.

24. Page 9, line 1. Comment on Spearman and Kendall results, which confirm the results obtained with Pearson.

25. Page 9, line 20. It is interesting that only with Saatchi and Baccini there is a single AGB peak corresponding to the higher VOD values. Why do the authors believe this peak is not appearing as clearly with the other two data sets? Is it consistent that the peak is higher for Baccini than for Saatchi? The authors should elaborate on the results presented.

26. Page 9, line 22. It seems to me that also Saatchi shows a very low dispersion for low AGB values, but the plot is too small. Please, address.

27. Page 9, line 29. The authors aggregate the data sets in two groups of biomes. This separation should be further justified. Also, there are many results shown in the supplementary material that are relevant and should at least be discussed in the text.

28. Section 4.9. I would suggest the authors to include a box plot with the SMOS IC VOD results per land cover. It will give a general idea of the dispersion and the mean values of VOD per land cover. Perhaps it would also be good to show the box plots for the AGB data sets.

29. Section 5. It would be nice to add a discussion on the consistency of the four AGB data sets and on why best correspondence is found between L-VOD and the approach of Saatchi (and not the one of L-band SAR).

30. Figure 3. It would be interesting to know the number of pixels in the two groups of biomes, and whether they are balanced. Are all the correlation significant? To what level? This is important information that should be included either in the figure or the text.

31. Page 10, line 3. The authors should comment on the slope of NDVI per land cover and most relevant aspects shown in the supplementary information.

32. Page 10, line 17. Please, specify which part of the supplemtary information is being referred to here.

33. Figure 4. Legend reads "C/X VOD" but caption reads "K/X/C VOD". Please, correct.

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