Referee # 1

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:

We thank the referee for his/her constructive comments. We will add a discussion on how results would change if a different year is used for the study (see plots and table in the answer to comment 3). The results remain unchanged, but we agree with the referee, that the presentation and discussion would look more robust adding other years. With respect to VOD from higher frequencies, we develop below in comment 2.

In addition below, we give more details on these comments and we address all the other comments made by the referee.

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.

Taking into account the three reviewers comments (Referee #2 thinks that "too much importance is given to the inter-comparison of different VOD retrieval algorithms), we reckon that the best trade off is to leave in the supplement the results for SMOS L2 and L3 but to move to the main body of the analysis per biomes currently in supplementary (text and Fig. S6). All land cover classes will be shown separately and Fig. 3 will be removed (see also answer to comment 29). Following this and other related comments, all references to supplementary information in the main body of the manuscript have been checked and they will be developed more explicitly in a new revised version.

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.

This manuscript is devoted to SMOS L-Band VOD. That's the reason why we did not attempt to perform a complementary study with data from other radiometers at the present stage. However, we do think that it is interesting to discuss the new results by comparing to previous results reported in the literature for other frequency bands. Figure 4 does not contain any new result. It is a Figure for the discussion, where results presented earlier are compared to published results by Liu et al. 2015. However, we realized that the normalization used to plot L-VOD, K/X/C-VOD and NDVI in the same plot could be misleading. The normalization is not needed to compare with other VOD, only for NDVI. Therefore, we will present the results in a new figure with two panels as the figure below. In the left panel L-VOD and NDVI were normalized to 1 using their maximum values. This is needed to plot the two quantities in the same figure. In the right panel, L-VOD and K/X/C-VOD relationship to Saatchi AGB are shown without using any normalization. The curves plotted here for the K/X/C-VOD are just those of Figure S4 from Liu et al. 2015, which were computed using Saatchi AGB and the same method that we used in the current study. Liu et al fitted their relationship using K/X/C-VOD data in the period 1998-2002 and Saatchi data acquired from 1995 to 2005 (page 6 of their supplementary information document). This will be reminded explicitly in the discussion section of a revised version of the manuscript. However, the non-linearity of the curve and the difference sensitivity to high AGB from different frequencies is driven by the high AGB values in the dense equatorial forest, which is not supposed to vary strongly in a few years time.

The curves for the other AGB dataset with respect to L-VOD are already shown in Fig S3. They will not add much information to this discussion and we tried to show them in the figure below but it becomes unreadable.

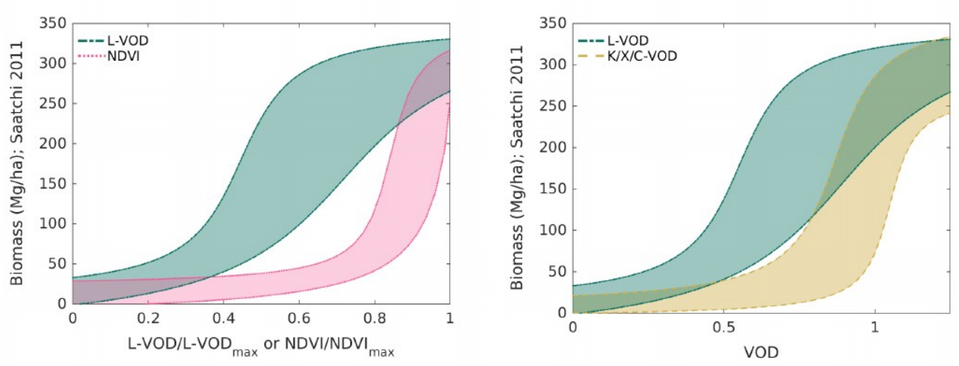


Figure 5. Left: Fits of the 5th and 95th percentile curves of the Saatchi et al. (2011) AGB with respect to SMOS-IC L-VOD (green) and NDVI (pink). To plot both distributions with the same scale, VOD and NDVI were normalized from 0 to 1 using their respective maxima (0.83 for NDVI and 1.24 for L-VOD). Right: Fits of the 5th and 95th percentile curves of the Saatchi et al. (2011) AGB with respect to SMOS-IC L-VOD (green) overlaid in the K/X/C-VOD versus Saatchi et al. (2011) AGB curves of Fig. S4 from Liu et al. (2015) (brown). No normalization is needed in this case as both VODs span a similar range of values.

Following reviewers comments, the text on Sect. 4.4 discussing this figure will be moved to Sect. 5 “Discussion” and will add the more detailed explanations provided here-above.

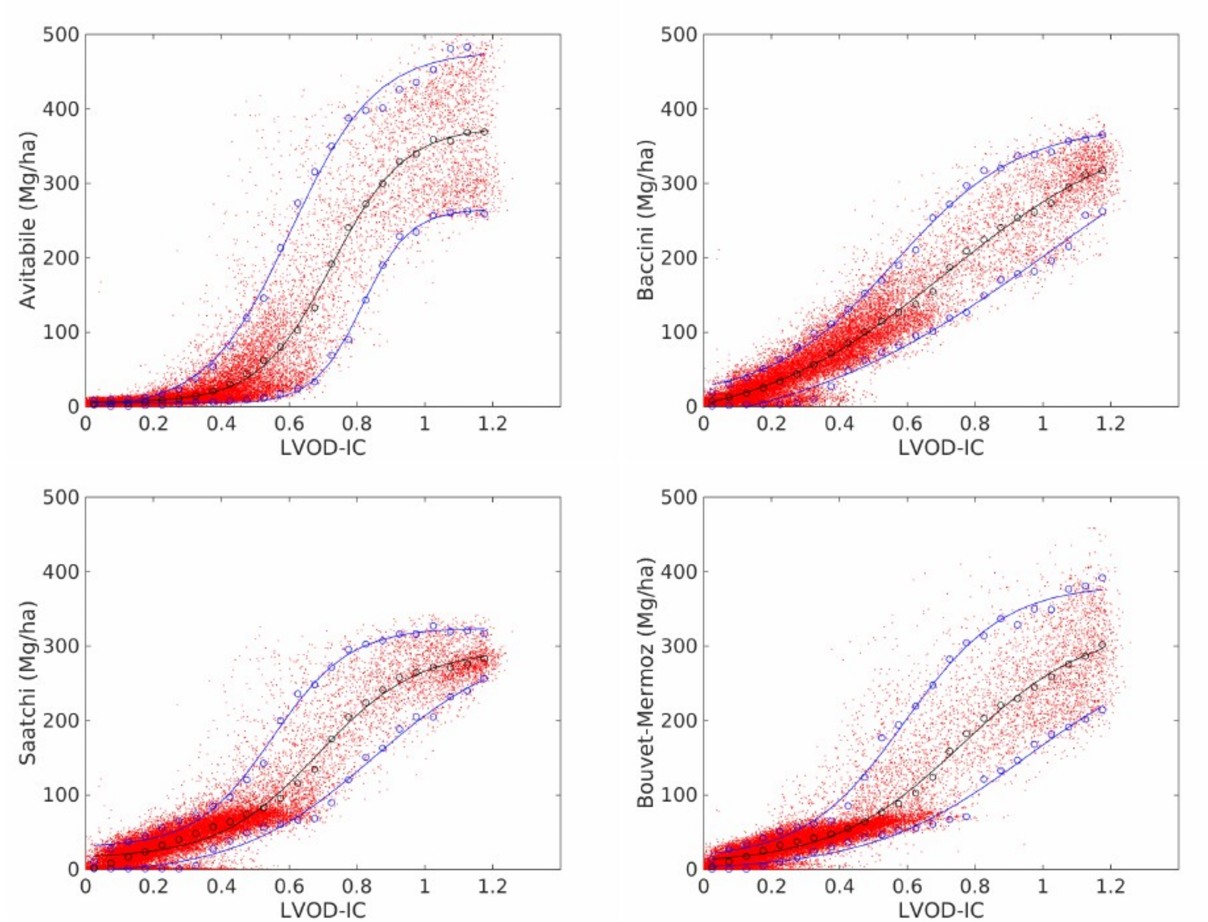
3. The title is too ambitious and general. The focus is clearly on SMOS L-band VOD and biomass, but the results presented (using 1 year of observations over Africa) do not support the use of the words “high sensitivity”. I would recommend the authors to provide a more specific title, more representative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

Taking into account comments from Referee #2 as well, we decided to change the title to:

An evaluation of SMOS L-band vegetation optical depth (L-VOD) data sets: a high sensitivity of L-VOD to above-ground biomass in Africa

Otherwise, by “high sensitivity” we meant that the AGB and L-VOD relationship is smooth and with a moderately low slope. This is not related to the number of years used for the study. However, as already mentioned, in the corrected version we will show that using more years do not change this result. For instance, the figure below will replace Fig. S3. Both are almost indistinguishable, however the figure below has been computed using data from two years (2011 and 2012) and both ascending and descending orbits (taking into account the comment on the orbits below).

See also answer to comment 19 below.



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”.

The referee is right that there are a few comments on results from the literature already in Section 4 “Results”. They will be moved to section 5 “Discussion”. In addition section 4 will be enlarged with the discussion of the results by biomes following the suggestions (here below) of the reviewer.

Here is a list of more specific comments and recommendations:

1. Abstract, last sentence. Consider changing “index” by “indicator”

We agree. This will be changed.

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.

Thanks for pointing this out. We agree that the best would be to introduce the tau-omega model already here. This will be done as follows. Instead of (lines 10-14):

In the presence of vegetation, part of the soil emission is absorbed and scattered. This extinction effect is parameterized by the vegetation optical depth (VOD) that can be estimated using radiative transfer theory […] Wigneron et al. 2007).

It will be rephrased to:

In the presence of vegetation, part of the soil emission is absorbed and scattered. These effects can be parameterized using radiative transfer models such as the so-called tau-omega model (Refs), were tau is the optical depth and omega is the single scattering albedo. Tau was shown to be linked […] Wigneron et al. 2007). Therefore, tau is commonly known as Vegetation Optical Depth (VOD).

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.

"Thick" will be removed (*VOD samples the vegetation including the woody vegetation under the green canopy*) as it is difficult to quantify it. We prefer to say that it is thicker than the layer sampled at higher frequencies as done in Line 30. Otherwise, the goal of this paragraph was to cite some examples of studies of the vegetation with VOD. The actual comparison of frequencies is done in the next paragraph (line 23 onwards) and in the first paragraph of page 3.

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.

This is a pertinent paper that will be added to the introduction, together with Konings, A. G.; Piles, M.; Rötzer, K.; McColl, K. A.; Chan, S. K. & Entekhabi, D. Vegetation optical depth and scattering albedo retrieval using time series of dualpolarized L-band radiometer observations Remote Sensing of Environment, 2016, 172, 178-189

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

Line 18 will be continued as follows:

[…] observations. In contrast to passive measurements, for which the goal is study how the thermal emission arising from the Earth is affected by the vegetation layer, active measurements allow to study how the radiation emitted by a human-made radiation source is backscattered by the vegetation, which depend mainly in vegetation water content and the vegetation structure.

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.

Ascending orbits data have been shown to give somewhat better results than descending orbits to retrieve soil moisture in Europe, North America and the Sahel (see Kerr et al. 2016, RSE, and references therein). The reason is that in some regions they can be less affected by radio frequency interference and that at 6 AM (ascending orbits) the soil and canopy are closer to thermal equilibrium and there are less problems of convective precipitations than for descending orbits (6 PM). However, for a sensitivity study of VOD to vegetation and in particular biomass this does not necessarily apply.

See also answer to general comment 3 above and comment 19 below. We show that the results obtained using descending orbits are same as those obtained using ascending orbits.

In a revised version of the manuscript we will use two years of data (2011-2012 and both ascending and descending orbits) as mentioned in the answer to general comment 3.

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.

The parameters Stokes 3 and 4 are actually used for filtering the SMOS brightness temperatures, for instance to detect RFI (Kerr et al. 2012, TGRS). This will be added to the text of the revised version.

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.

Due to the specificities of the SMOS geometry of observation, the profiles of brightness temperatures observed at the middle part of the field of view (~600 km centered on the satellite sub-track) have larger ranges of incidence angle than the outer pat of the field of view. For such observations, the retrieval system has more information content to discriminate the vegetation emission from the ground emission leading to more accurate retrieved soil moisture and VOD. The retrieved VODs and associated uncertainties for such grid points are used as prior first guess and uncertainties for the VOD retrieval of the next overpass of these grid-points (3 days later max) that will be observed, this time, at the outer part of the field of view with reduced range of incidence angle instead of using auxiliary data LAI, LAImax as first-guess values and fixed large prior uncertainties (see Kerr et al 2012).

This information will be added to page 4.

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.

A) **Albedo and roughness:**

For SMOS IC the roughness and single scattering parameters are assigned per IBGP classes, based on Parrens et al. (2017a, b), and are averaged per pixel according to the fraction of classes present in the pixel (Fernandez-Moran et al. 2017).

For SMOS L2 and L3 algorithms, single scattering albedo and roughness values depend on the surface type and are taken from literature and/or specific SMOS studies. For low vegetated area the single scattering albedo is set to 0 and roughness set to 0.1. For forested areas the single scattering albedo is set to 0.06 for tropical and subtropical forest and 0.08 for Boreal forest and roughness set to 0.3 (Rahmoune et al. 2013,2014, Al Bitar et al. 2017).

Marie Parrens, Jean-Pierre Wigneron, Philippe Richaume, Ahmad Al Bitar, Arnaud Mialon, Roberto Fernandez-Moran, Amen Al-Yaari, Peggy O'Neill, and Yann Kerr, 2017. Considering Combined or Separated Roughness and Vegetation Effects in Soil Moisture Retrievals, International Journal of Applied Earth Observation and Geoinformation 55, 73-86.

M. Parrens, A. Al Bitar, A. Mialon, R. Fernandez-Moran, J.-P. Wigneron, P. Ferrazzoli, and Y. Kerr, 2017. Estimation of the L-band Effective Scattering Albedo of Tropical Forests using SMOS observations, IEEE GRS Letters 2017, 14, 1223-1227

b) **Previous retrievals:** see answer to the next comment #10.

c) **Grid name:** The name of the grid will be corrected to reflect the exact name.

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

In a first run the minimization is initialized with SM 0.2 m3/m3 and L-VOD 0.5. This allowed to compute a mean L-VOD map per each grid point. In a second run SM was initialized at 0.2 m3/m3 while the mean L-VOD for each grid point was used to initialize the L-VOD. This information will be added to page 5 referring the Table S1 where it was already given but with a typo as the value quoted for the initialization of VOD in the first run was 0.2 instead of 0.5.

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

Absolutely, thanks for pointing this out. The reference is: Fick, S. E. & Hijmans, R. J. WorldClim 2: new 1-km spatial resolution climate surfaces for global land areas *International Journal of Climatology, Wiley Online Library,* 2017

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

Thanks for the typo correction.

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

The following text will be added with an explicit reference. First, Huete et al. 2002, cited in the first line of the subsection for NDVI will be replaced by:

Tucker, C. J. Red and photographic infrared linear combinations for monitoring vegetation *Remote sensing of Environment, Elsevier,* **1979**, *8*, 127-150

Second, the EVI description text will be enlarged with:

The enhanced vegetation index (EVI) was designed to have a higher sensitivity in high biomass regions than NDVI by allowing to distinguish the vegetation and the atmosphere contributions to the signal (Huete et al. 2002). Whereas the (NDVI) is chlorophyll sensitive, *the EVI is more responsive to the canopy type and structure (including LAI) and, for example, it has allowed to study the Amazon green-up season (where other vegetation indexes such as NDVI do not show any particular pattern, Huete et al. 2006).*

Huete, A. R.; Didan, K.; Shimabukuro, Y. E.; Ratana, P.; Saleska, S. R.; Hutyra, L. R.; Yang, W.; Nemani, R. R. & Myneni, R. Amazon rainforests green-up with sunlight in dry season *Geophysical research letters, Wiley Online Library,* **2006**, *33*

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

We assume it is page 6. The second “in a second step” will be replaced by “In the third step” and “in the third step” (line 8) will be replaced by “Finally”.

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.

One cannot summarize the results of this study saying that the best agreement of L-VOD is found with respect to Saatchi AGB dataset. Please, see the answers to comments 26 and 29 here below. Regarding the sentence referred to here, it will be reformulated as : […] *because one of the reference data-set is available only in Africa, in addition this dataset is particularly interesting because it has been obtained using radar observations at a lower frequency than other datasets, namely, in L-band, which is also the frequency of SMOS. The African continent contains […]*

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

Yes, Chi2.

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.

The DQX is actually a standard deviation which informs only about the uncertainty of the retrieved solution which is driven by the forward model sensitivity at the solution point. It is the retrieved parameter post uncertainty computed using the inverse linear tangent model (Jacobian) at the solution used to translate the observation uncertainty (radiometric accuracy) into the parameter space uncertainty. It does not inform by itself about the correctness of the solution with is based on a quality of a fit. In other words, we can have a very wrong modeling (bad fit) with a retrieved parameters solution where the forward model is very sensitive resulting to low DQX. Moreover, the DQX values are not homoscedastic as our forward models are much more sensitive for lower values of the (SM,VOD) parameter space (leading to low DQX) than for higher values (leading to high DQX). By filtering the DQX too strictly there is a serious risk to bias statistics toward lower retrieved SM and VOD, which would bias our results for tropical forest where both SM and VOD are high.

The DQX should be used as a weight in the parameters use e.g. as it is done by assimilation system. See for instance: A. Tarantola; Inverse Problem Theory and Methods for Model Parameter Estimation, SIAM, 2005.

In contrast, the Chi2, or alternatively its probability, which is naturally used in the retrieval procedure is currently the preferred option to filter out the retrieved solution; it is the classical goodness-of-fit test. See for instance Román-Gascon et al 2017 (using Chi2 < 3.5) or Bircher et al. (2013), who used Chi2 probability.

Román-Cascón, Carlos, et al. "Correcting satellite-based precipitation products through SMOS soil moisture data assimilation in two land-surface models of different complexity: API and SURFEX." *Remote Sensing of Environment* 200 (2017): 295-310.

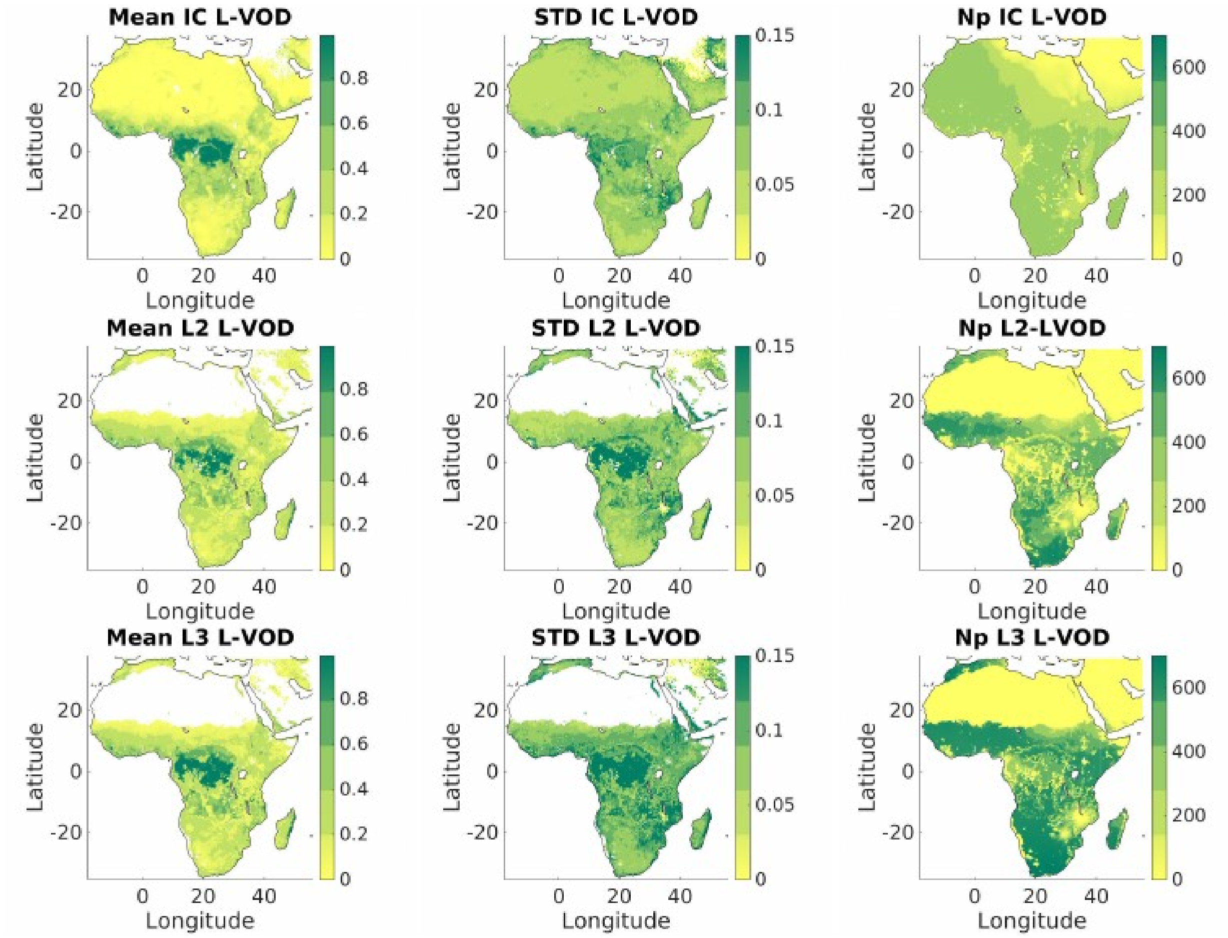
Validation of SMOS L1C and L2 Products and Important Parameters of the Retrieval Algorithm in the Skjern River Catchment, Western Denmark, IEEE Transactions on Geoscience and Remote Sensing, pp 2969 – 2985. S. Bircher, N. Skou, Y. H. Kerr, 2013, DOI - 10.1109/TGRS.2012.2215041, Vol 51, Issue 5, ssn 0196-2892

The manuscript text will be modified as follows:

Several quality indicators are present in the SMOS products. The DQX parameter uses the inverse linear tangent model (Jacobian) to translate the observation uncertainty (radiometric accuracy) into the parameter space uncertainty. The forward models are much more sensitive for lower values of the (SM,VOD) parameter space (leading to low DQX) than for higher values (leading to high DQX). Therefore, filtering using DQX implies a risk to bias our results for tropical forest where both SM and VOD are high. In addition, the DQX parameter does not give information about the correctness of the solution, which is based on a quality of a fit. Therefore, the Chi2 parameter (goodness of the fit) was used to filter out the retrieved solutions. Several tests were done and a value of 3, corresponding approximately to the peak of the Chi2 probability distribution was found to be a good threshold. This is in agreement with the values used in other studies (see for instance, Roman-Cascon et al. 2017).

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.

In a corrected version of the manuscript we will split Fig 1 in two different figures: one for maps that have been averaged on time and another one for AGB and cumulated precipitations datasets. The first one will add the STD and the number of points in the times series for each grid point as follows.



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.

Actually, the shape of the scatter plots is very similar using data for other years. See for instance the next figure and compare to Fig S3.

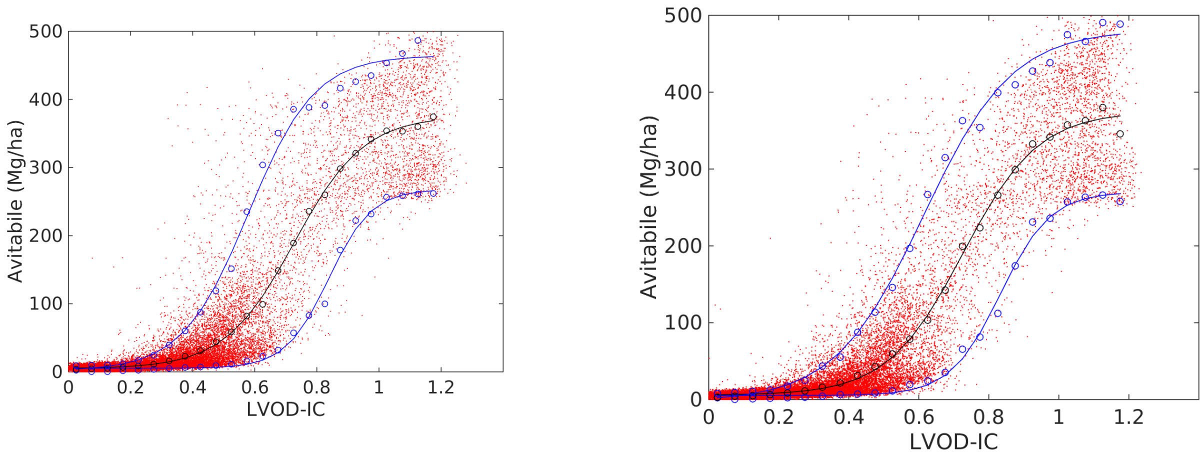


Fig. Left : 2012 Ascending orbits. Right: 2012 Descending orbits

The next table shows the parameters of the fits using ascending or descending orbits in 2012. The values can be compared to those of Table S2. They have almost the same numeric values.

AGB	Year	Orbit	curve	a	b	c	d	R^2
Avitabile	2012	A	05th	262.697	14.406	0.838	5.347	0.99548
Avitabile	2012	A	Mean	370.615	8.920	0.727	4.915	0.99917
Avitabile	2012	A	95th	462.962	9.398	0.580	1.613	0.99243
Saatchi	2012	A	05th	333.496	4.642	0.906	-3.338	0.99080
Saatchi	2012	A	Mean	280.675	6.686	0.684	14.439	0.99334
Saatchi	2012	A	95th	291.444	9.661	0.545	32.436	0.99372
Baccini	2012	A	05th	497.748	2.673	1.024	-41.748	0.98810
Baccini	2012	A	Mean	414.033	3.503	0.717	-27.346	0.99882
Baccini	2012	A	95th	383.823	4.950	0.550	-1.911	0.99757
Bouvet-Mermoz	2012	A	05th	292.291	4.800	0.963	4.717	0.97445
Bouvet-Mermoz	2012	A	Mean	319.123	5.261	0.760	7.879	0.99616
Bouvet-Mermoz	2012	A	95th	358.172	7.290	0.581	17.621	0.99226
Avitabile	2012	D	05th	264.867	13.984	0.833	5.353	0.99548
Avitabile	2012	D	Mean	368.991	9.271	0.726	5.795	0.99765
Avitabile	2012	D	95th	483.476	7.893	0.614	-2.245	0.99585
Saatchi	2012	D	05th	332.540	4.976	0.890	-5.476	0.99111
Saatchi	2012	D	Mean	287.547	6.436	0.691	12.981	0.99499
Saatchi	2012	D	95th	291.493	8.979	0.543	30.449	0.99324
Baccini	2012	D	05th	606.345	2.250	1.168	-53.195	0.98742
Baccini	2012	D	Mean	398.354	3.688	0.706	-21.945	0.99806
Baccini	2012	D	95th	379.008	5.076	0.567	6.162	0.99538
Bouvet-Mermoz	2012	D	05th	308.430	4.441	0.965	0.250	0.98165
Bouvet-Mermoz	2012	D	Mean	317.532	5.425	0.764	10.022	0.99729
Bouvet-Mermoz	2012	D	95th	375.791	6.420	0.604	12.619	0.99451

Therefore, there is no real impact of using just one year. However, in a revised version of the manuscript we will use two years of data both for ascending and descending orbits. The results will not change but they would look more robust to the reader. This will be discussed in a revised version of the manuscript. See also answer to General Comment 3.

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

We think that the reviewer meant “Page 8” and his/her comment refers to Liu et al. 2015 function.

$$AGB = a \times \frac{\arctan(b(\mathrm{VOD} - c)) - \arctan(-bc))}{(\arctan(\infty) - \arctan(-bc)} + d,$$

And the equation of the logistic function. Eq. S3. Of course, both can be shown 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)

“Remaining static data sets” will be changed by “other evaluation data sets”. The following description will be added to a revised version of the manuscript. See the figure in the answer to comment 18, which will be the new Fig. 1, the remaining panels (evaluation datasets) of former Fig. 1 will become Fig. 2 in the corrected manuscript.

Figure 1 shows the average L-VOD computed over 2011 and 2012 using both ascending and descending orbits for the three SMOS L-VOD products. In addition, it also shows the standard deviation (STD) and the number of points of the local time series after applying the filters discussed in Sect 3. The three SMOS L-VOD products show a similar spatial distribution but the SMOS-IC L-VOD shows a smoother spatial distribution than the L2 and L3 datasets. The highest values are found in equatorial forest regions and L-VOD decreases monotonically with distance to the equatorial forest in the tropical area and beyond. The STD of the L-VOD time series also increases towards the equatorial forest, in particular for the L2 and L3 datasets. The number of points in the time series is lower for the IC dataset due to the lower revisit frequency arising from the requirement of having brightness temperature measurements spanning an incidence angle range of at least 20º (Fernandez-Moran et al. 2017).

Figure 2 shows the evaluation data *after resampling to a 25 km EASEv2 grid*: 2011-2012 average *of the MODIS NDVI and EVI indices, tree height, cumulated precipitations and AGB datasets. EVI and NDVI also decrease with increasing distance to the equator but more slowly than L-VOD. The tree height map shows two main populations: the equatorial forest, with heights larger than 20 meters, and the rest of the continent, where most of the vegetation is lower than 4 meters. In contrast to the previous quantities, AGB can vary in two orders of magnitude, therefore AGB maps are shown in logarithmic units in Figs 1. The Baccini, Saatchi and Bouvet-Mermoz maps show a similar AGB distribution. In contrast, the Avitabile* *map shows a much sharper decrease of AGB from the equatorial forest region to the rest of the continent.*

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

Thanks, it will be added to a corrected version.

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

The referee is right that the table contains all values while the text commented only on Pearson. As he/she says, the Spearman and Kendall results confirm the Pearson results, which also means that the lower values obtained for the L3 dataset are not due to a correlation that could be good but more non-linear than those of the IC and the L2 dataset. Thus, we fully agree that the results should be commented. We propose the following rewriting:

A quantitative assessment of the correlation and the dispersion of the different scatter plots can be found in Table 1, where Pearson, Spearman and Kendall correlation coefficients are given for the three L-VOD data sets with respect to the evaluation data sets. The lowest Pearson correlation coefficient values were obtained for L3 L-VOD (R = 0.65−0.87). The Pearson correlation coefficients obtained for L2 L-VOD are similar (R = 0.67−0.87) than those obtained for L3 L-VOD but systematically higher by up to 4%, while the values obtained for IC L-VOD are the highest (R = 0.77−0.94) with respect to all the evaluation data sets. The correlation increase is in the range of 5%-10% with respect to L2 L-VOD and up to 15 % with respect to L3 L-VOD. The rank correlation values with respect to all the evaluation datasets are also higher for IC L-VOD (rho 0.78-0.91, tau 0.61-0.75), followed by L2 L-VOD (rho 0.67-0.83, tau 0.50-0.65) and L3 L-VOD (rho 0.66-0.80, tau 0.49-0.62). These results are in agreement with those obtained with the Pearson correlation and imply that the lower Pearson correlation values obtained for the L3 and L2 datasets are not due to a correlation that could be better but more non-linear than that of the IC dataset. Therefore, using eight vegetation-related evaluation data sets and three different metrics, the most consistent SMOS L-VOD data set is SMOS-IC. This result implies that, currently, the SMOS-IC dataset is the best SMOS L-VOD product to perform vegetation studies, and the rest of the current study will focus on SMOS-IC L-VOD.

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.

We have analyzed the high AGB blobs of the scatter plots as follows:

Avitabile blob 1: VOD > 1 ; 230 < AGB < 330

Avitabile blob 2: VOD > 1 ; AGB > 330

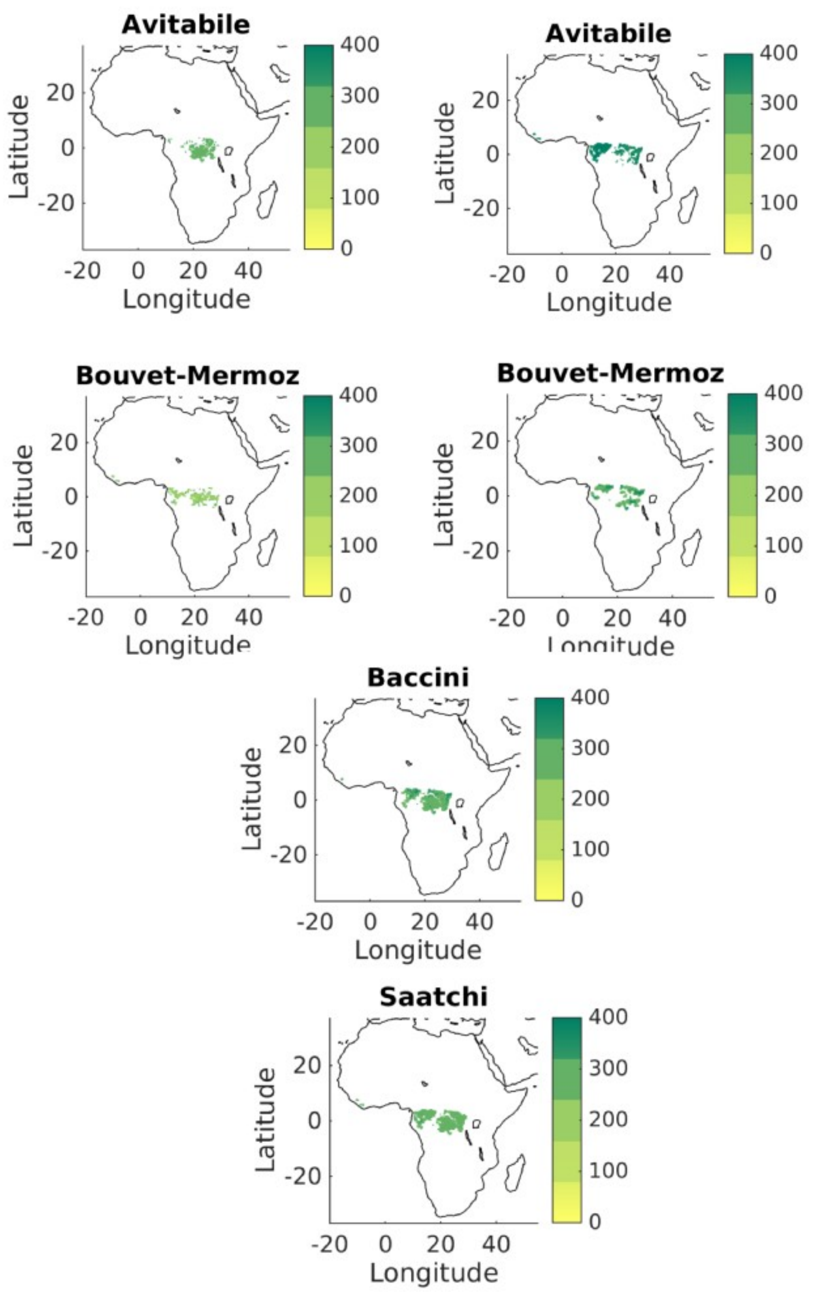
Bouvet-Mermoz blob 1: VOD > 1 ; 170 < AGB < 260

Bouvet-Mermoz blob 2: VOD > 1 ; AGB > 270

Saatchi : VOD > 1; AGB > 240;

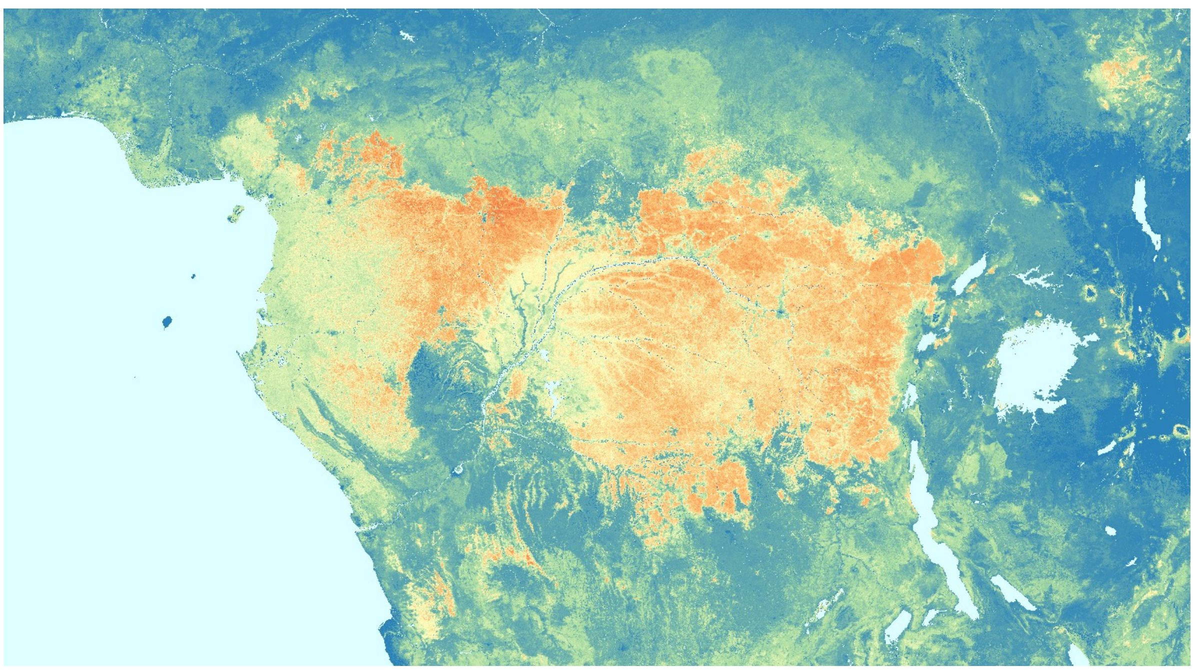
Baccini: VOD > 1; AGB > 240;

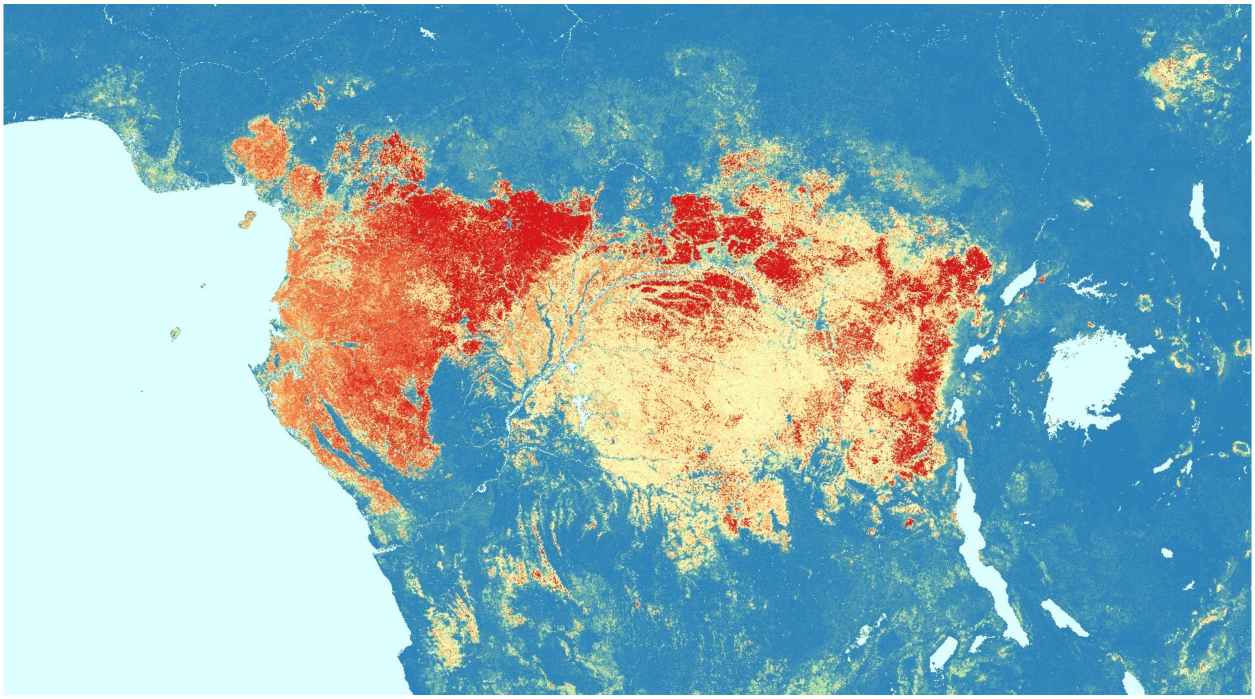
The next figure shows the spatial distribution of those peaks:



In the two upper rows, one sees that consistently for Bouvet-Mermoz and Avitabile the first blob (slightly lower AGB) is in the center of the equatorial region around the Congo river basin while the spatial distribution for the highest AGB blob surrounds the first one. This bi-modal behavior is not seen for the high AGB values in the Saatchi and Baccini datasets, where the whole equatorial forest shows more homogeneous distribution with similar values in the two regions. Definitely, L-VOD seems to be in more agreement with the two latter datasets, unless the high AGB blobs in Bouvet-Mermoz and Avitabile are more realistic and in this case, L-VOD would show signs of saturation since, it remains basically constant. This is the same discussion already done when commenting the scatter plots. Thus, we reckon that it is not necessary to add anything to a revised version of the manuscript. In any case, to our knowledge, it is not easy to say which of the four AGB datasets is more realistic in the densest parts of the equatorial forest.

Note that the spatial distributions shown in the previous figures are not an artifact arising from the spatial averaging of the AGB to the SMOS resolution. If one plots the AGB data at the original resolution the differences are clear. See for instance in the next two figures that the Baccini original data (upper figure) is much more homogeneous than the Avitabile data (lower figure).





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.

That's true. Together with Bouvet-Mermoz, the Saatchi dataset show the highest correlation values with respect to SMOS L-VOD. In Fig. S6, one can see that correlation coefficients obtained with Saatchi's data are somewhat higher than those of Bouvet-Mermoz for low vegetation but lower for Savannahs. For woody savannah the situation is more complex for the mixed nature of this biome and because in the Bouvet-Mermoz datasets uses the Mermoz law for pixels classified as dense forest on the ESA CCI land cover dataset. Pixels classified as woody savannah using IGBP at the SMOS resolution can contain both woody savannnah and dense forest in the ESA CCI dataset. The scatterplot for woody savannah using the Bouvet-Mermoz dataset show these two populations for high L-VOD values, which decreases the Pearson correlation (which is lower than that obtained for Saatchi in woody Savannah) but still, Kendall and Spearman correlations are higher for the Bouvet-Mermoz dataset. This discussion will be added to a corrected version of the manuscript. See also answer to comment 29.

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.

We understand that the referee is suggesting to move Fig. S6 and the text in page 2 Lines 16-31 to the main body of the manuscript. This seems a good idea for a revised version of the manuscript. Of course, the additional discussion suggested in the previous point will also be added.

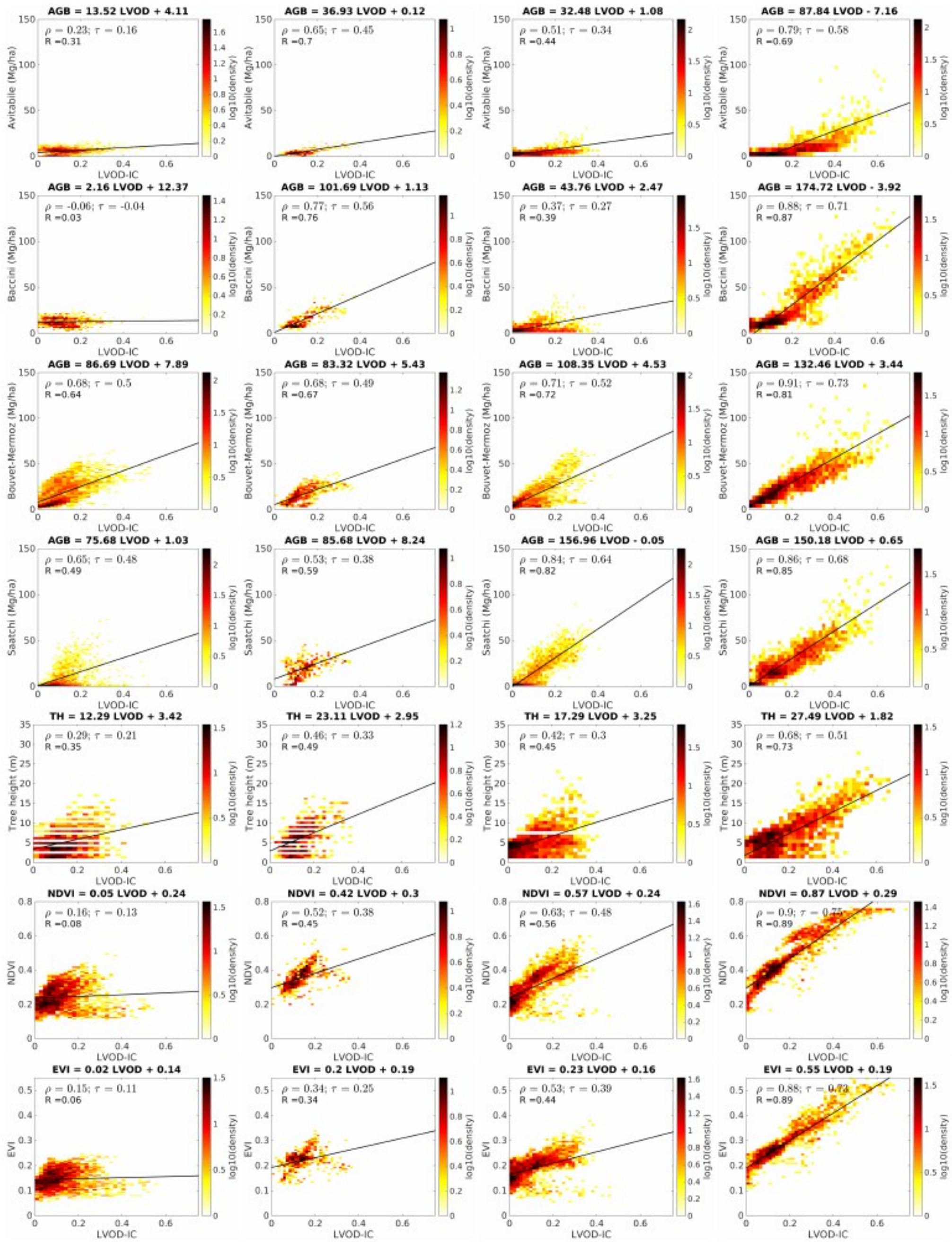
28. Section 4.9. I would suggest the authors to include a box plot with the SMOS IC VOD results per land cover. It will give a general idea of the dispersion and the mean values of VOD per land cover. Perhaps it would also be good to show the box plots for the AGB data sets.

There is not section 4.9 and it is not fully clear to us what the reviewer is suggestion. Making box plots showing the distribution of LVOD for different land cover classes? We recknon that this will not add much information as the distribution can be seen also in the scatterplots per land cover class. Maybe he/she is proposing something else ?

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).

We do not think that one can summarize the results of this study saying that the correspondence of L-VOD and AGB is better with Saatchi AGB. Figure S6 clearly shows this. For instance for woody savannah the Pearson correlation is higher for Baccini and all three correlation coefficients are also higher for Baccini AGB in dense forest (see also the answer to comment 26). In any case, we will proceed as indicated in the answer to comment 27 and we will move Fig. S6 to the main body showing separately all the classes (separating shrublands, crops, grasslands and natural vegetation, see the two figures below). The best correlations of AGB and L-VOD are found with (i) Bouvet-Mermoz for Shrublands and Savannahs (ii) Baccini for croplands and equatorial forest (iii) Saatchi for grasslands. Regarding natural vegetation and woody savannah the correlation values obtained with Saatchi and Baccini are very similar. One should note that correlation values obtained with Bouvet-Mermoz for woody savannah are degraded to the fact that for the highest values of AGB found in this class at the SMOS resolution, the AGB estimation is a mix of Bouvet and Mermoz approaches. Therefore, all AGB datasets except that of Avitabile performs the best for a few land cover classes.

L-Band radar observations are thought to be very sensitive to biomass variations, in spite of a significant sensitivity to soil moisture as well. The high correlation with SMOS L-VOD, also at L-band, would confirm this fact. The strange behavior of Avitabile AGB probably comes from the fact that it is pure data driven method and that it is therefore very sensitive to the data used to train the method. In their training database, high AGB plots could be over-represented.



gure 4. SMOS IC L-VOD relationships to the AGB and tree heigh evaluation datasets for different land cover classes: From left to right: Open shrublands. *(ii)* Croplands, *(iii)* Grasslands, and *(iv)* Cropland/natural vegetation mosaics. See Table S2 for more details on these ıd cover classes.

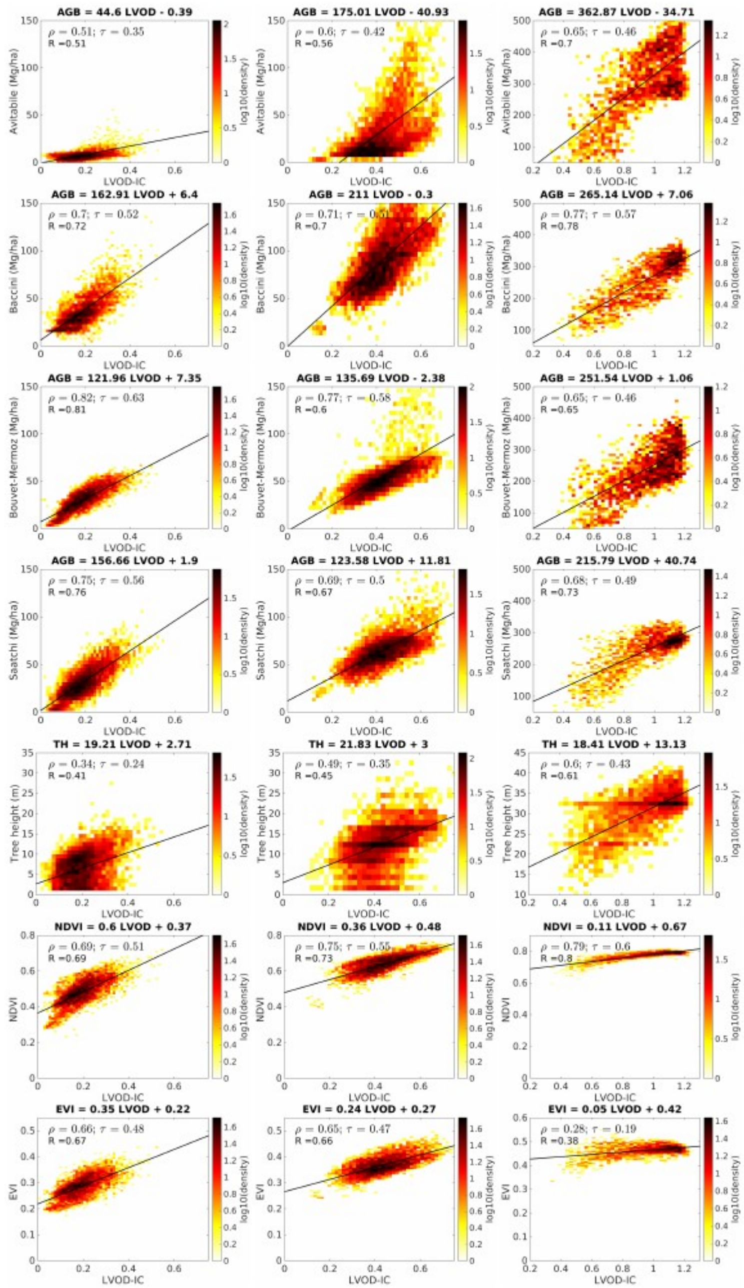


Figure 5. SMOS IC L-VOD relationships to the AGB and tree heigh evaluation datasets for different land cover classes: From left to right: *(i)* Savannah *(iii)* Woody savannah and *(iv)* Evergreen broadleaf. See Table S2 for more details on these land cover classes.

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.

The number of grid points in the two groups of biomes it is, of course, not the same. That is not the point here, the point is that groups from grassland, croplands, savannah and woody savannah show a similar slope, much lower than that of the equatorial forest (Fig. S6, which will be moved to the main text) therefore they can be grouped in two groups because they show two “regimes” of the AGB vs L-VOD relationship. All correlations are significant with very low P-values (<0.05).

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.

NDVI and EVI will be added to current Fig. S6 and moved to the main body. Former Fig. S6 will become two new figures to show all land cover classes and Fig. 3 will be removed. The new Figures are shown in the answer to comment 29.

Regarding the slopes of the relationship, the following text will be added to the manuscript.

Regarding the L-VOD and NDVI/EVI relationships in different biomes, it is worth noting that, in contrast to AGB, the slope of the relationship decreases from low vegetation types to savannahs and dense forest as the optical/infrared indices saturates. It is noteworthy that no significant difference is seen on the behavior of EVI and NDVI for high L-VOD values.

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

The sentence will be removed as Fig. S6 will be moved to the main document and discussed in detail there.

33. Figure 4. Legend reads “C/X VOD” but caption reads “K/X/C VOD”. Please correct.

Thanks for pointing this out. The legend should actually show “K/X/C VOD”. See answer to general comment 2 above.