General modifications:

We took the opportunity of this second round of reviews to update our paper with the latest released version of the CCI Biomass maps. The version 5.01 of the CCI was released in July 2024 and distributes global biomass maps for 2010 and 2015-2021. Besides the usual 100m resolution maps, aggregated maps at 1 km, 10 km, 25 km and 50 km are distributed and we are now working with the 25 km aggregated maps. Using v5 of the CCI rather than v4 also ensures a longer (2015-2021 rather than 2017-2020) and a state-of-the-art time series comparison to contextualize the results presented in our article without changing its general conclusions. The philosophy, principle and main results are not altered by this update. The statistics have been updated with the new values but beyond the values of the results, their interpretation remains the same.
Moreover, we have slightly strengthened the filtering of our input VOD. On top of the Chi2P<0.05 or RFI>20% filtering we have set up the following:

- The footprints which median RFI value over the year is above 20% are entirely dropped for that year
- The footprints for which there are fewer than 10 available quality measures over the year are entirely discarded for that year
- The VOD acquired when the air temperature is below 273K are also discarded.

This more restrictive filtering increases the quality of the input VOD but does not change the philosophy, principle and main results of the article. More pixels are discarded in the process but the remaining ones are of higher quality. The time series of estimated AGB maps are not significantly changed by this update. All of the above-mentioned changes are described in the revised version of the paper.

Responses to Anonymous Referee #1

We thank referee #1 for providing a second feedback on our revised manuscript. The valuable feedback was carefully taken into account to improve the paper.

Below, Referee #1 comments are marked in red.
Responses to the comments are marked in blue.
Changes made in the manuscript are marked in *italic*.

Minor comments:

Thanks a lot to the authors for making the effort to review the manuscript! Most of my concerns were addressed. However, I still believe that the following issues should be clarified:

1. Regarding the aggregation of the uncertainty map by using a quadratic mean of all non-nodata contributing pixels - this automatically reduces the uncertainty estimate obtained for the SMOS resolution. The uncertainty of an average is usually lower than the uncertainty of the individual pixels. How much the uncertainty is reduced depends on how strongly the errors are correlated between pixels. I understand that the authors did not really take this into consideration, so I think that you should clearly explain this in the manuscript.

Indeed, thank you for pointing it out. Following this round of review, we have decided to update our manuscript with the just-released version 5 of the Biomass CCI. In the latest version, the CCI AGB maps and their associated uncertainties are distributed at several ground resolutions (100 m and aggregated at 1 km, 10 km, 25 km, 50 km). We then directly worked with the aggregated resolution of 25km. We added a sentence to clarify this, and also to specify that the aggregated uncertainty is lower than the original uncertainty.
Modifications:

lines 232-234: The standard deviation of the aggregated maps at 25 km is significantly lower than the standard deviation of the original resolution (100 m) maps and lies below 15% of the AGB value for most pixels

2. I completely agree that validating AGB or L-VOD datasets in the temporal domain is tricky and that one cannot expect agreement with optical data in most of the biomes. However, given that this dataset is one of the only AGB datasets with multiple time steps, I think it would be important to include time series of your AGB estimate compared to other AGB estimates (Xu).

We agree with this comment, and this is the reason why such comparisons are included in the paper, see Figures 8 and 9. As there is no benchmark AGB map, the time series comparison was performed with the two datasets available over multiple years: Santoro et al. (CCI biomass) and Xu et al.

Responses to Anonymous Referee #3

We thank the referee #3 for reviewing and commenting on the manuscript. This valuable feedback was carefully taken into account to improve the quality of the paper. It seems like a new review to us as such concerns were not raised during the first round. This is why they were not previously addressed.

Below, Referee #2 comments are marked in red.
Responses to the comments are marked in blue.
Changes that have been made in the manuscript are marked in *italic*.

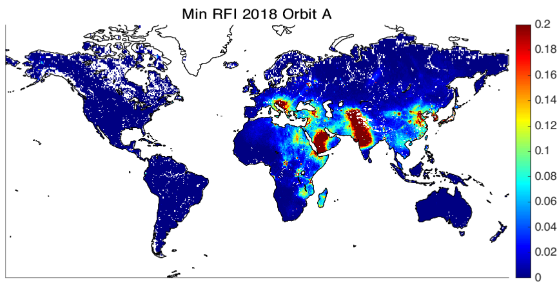
This study derives yearly Above ground biomass (AGB) from the vegetation optical depth of SMOS, taking advantage of the L band of SMOS. This leads to a new set of data readily available for use in scientific research and other potential areas.
The product is demonstrated to be reliable, and its uncertainties are assessed to properly understand its performance.
There is a lot of clarity about how the data is developed, and the study also aims at answering some questions that lead to decisions made in the development of the product. I do have some concerns that I hope will make some issues clearer and perhaps, the data more helpful to users.

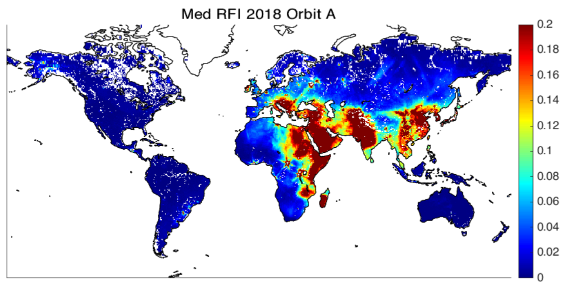
General comments
1. The two hypotheses chosen here are nice but might need proper reframing. The first one, looking at the impact of orbit time is already well known. Additionally, I don't see it's relevance at the timescale the AGB product is developed. Finer timescales will benefit more from this. Furthermore, it doesn't really inform/optimize the Asc+Desc merging routine, to perhaps, provide weights to pixels or time points of higher qualities. At this time scale, I would be very surprised if the difference between the uncertainties of the orbit times are anything worth taking seriously. Studies of Thomas Holmes (NASA Goddard), for instance, have shown that the SMOS overpass times are the cremes for thermal equilibrium. I would be more interested in this hypothesis if one of the overpass times were somewhere close to something like AMSR2's, close to midday. I think a proper reframing may be required.

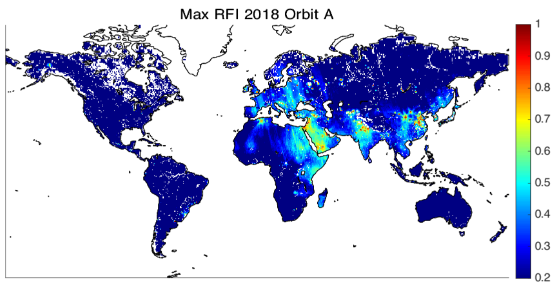
Indeed, the SMOS orbital configuration was set up at 6am specially for the mentioned reasons (before T. Holmes' studies). By construction, the counterpart of the orbit is 6:00 PM. The reviewer is correct to say that more differences are expected between the orbits (Ascending vs Descending) for daily/season variability studies (see 3rd and 4th images below for a comparison of the yearly and monthly difference between Asc. and Desc. orbits). Ideally, we would only use ascending orbits if we could. Unfortunately, RFI (Radiometric Frequency Interference) prevents SMOS from acquiring data everywhere (China, Eastern Siberia, Africa may be impacted). Ascending (A) and descending (D) orbits are impacted differently by the RFI (see the first two

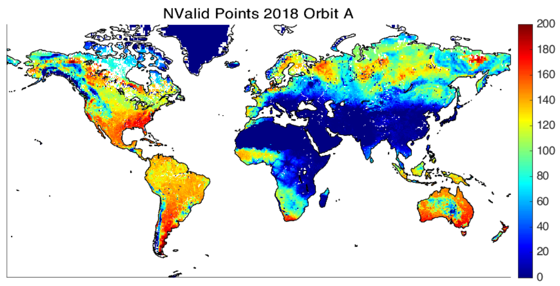
example images below for 2018) and including descending orbits in the processing leads to an increased number of observations and an extended coverage of the Earth. As shown by the third figure below, ascending and descending yearly averaged VOD still present non negligible differences even though these differences are smoother than when averaging over a month (4th and last image). According to us, testing how the calibration may be impacted by merging both orbit types remains relevant but, as you suggested, needs to be specified and reframed.
Indeed, our hypothesis is more about checking that both ascending and descending orbits can be merged properly rather than checking whether an orbit type is better than the other (we know that ascending orbits are better by design for this mission). When conducting this work, we were expecting that including descending orbits would not impact the estimation significantly but we still wanted to check it out, especially in the areas where ascending orbits are not available due to RFI. With this test, we specifically want to demonstrate that both orbits (AM and PM) can be merged properly without great consequences on the calibration.

Minimum, Median, Maximum RFI and Number of valid points for the ascending (A) orbits of 2018:

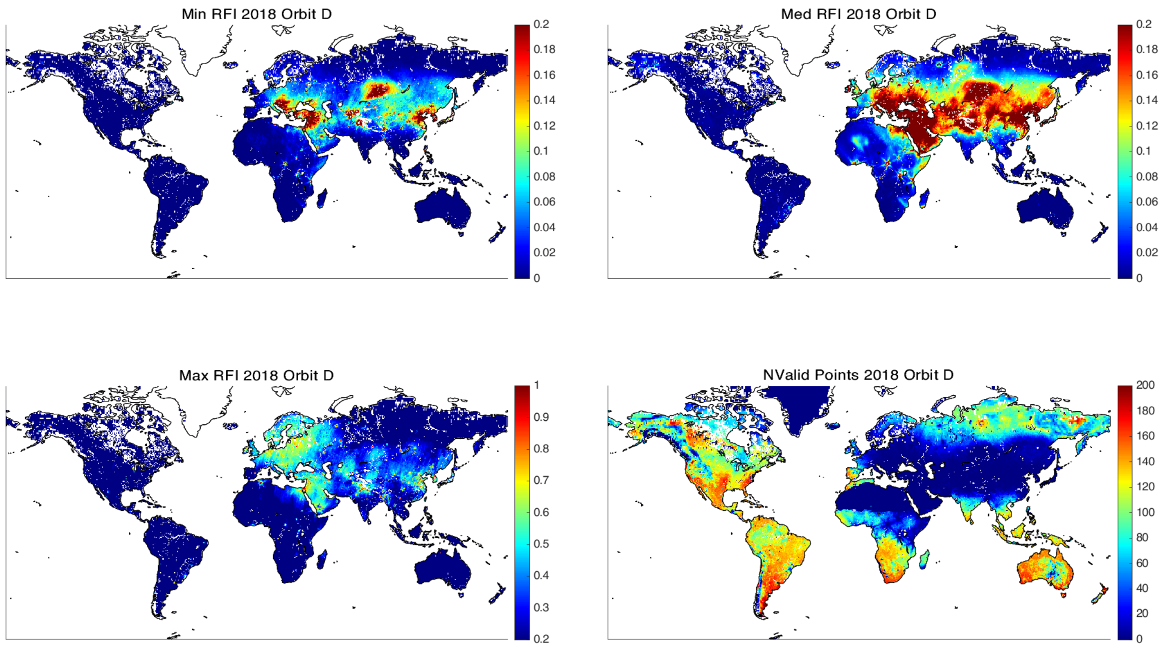


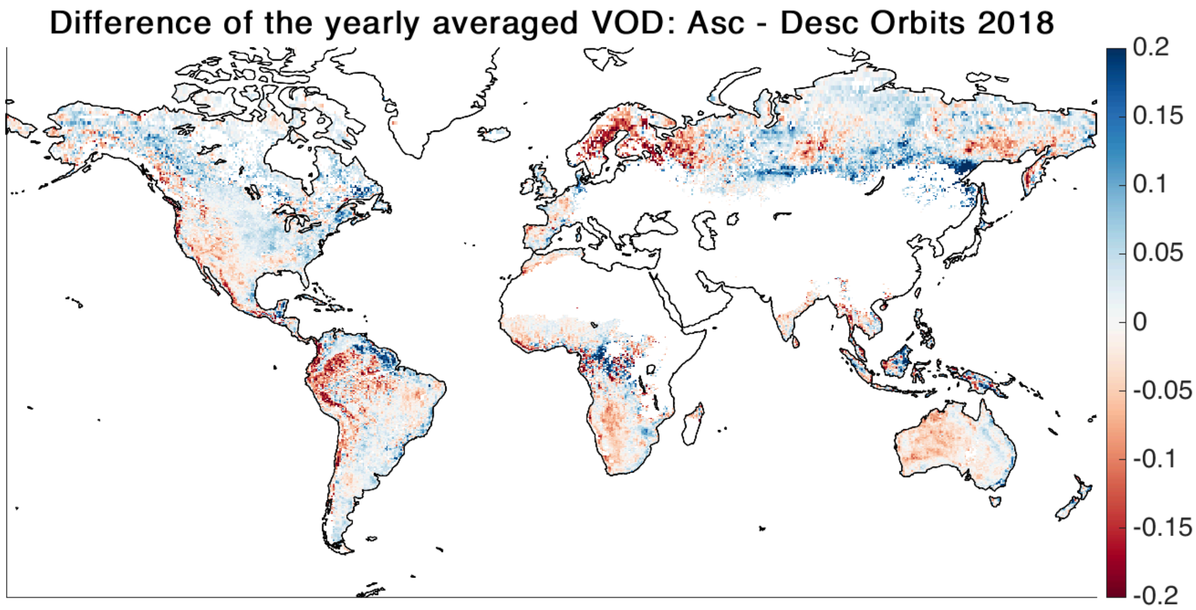


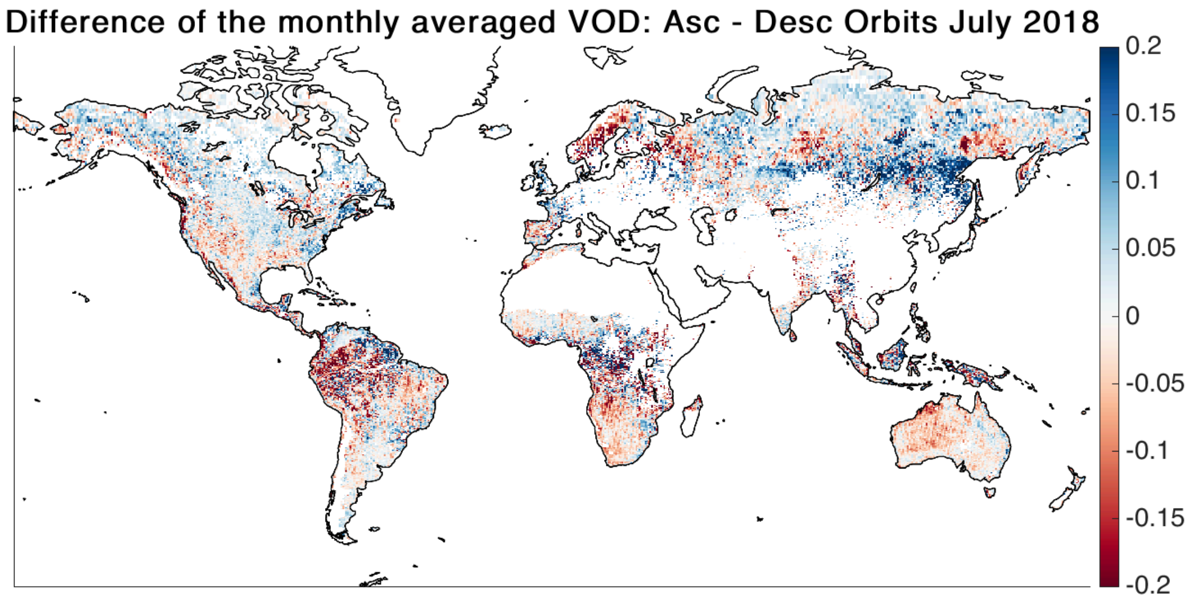




Minimum, Median, Maximum RFI and Number of valid points for the descending (D) orbits of 2018:


Min RFI 2018 Orbit D
0
0.02
0.04
0.06
0.08
0.1
0.12
0.14
0.16
0.18
0.2
Med RFI 2018 Orbit D
0
0.02
0.04
0.06
0.08
0.1
0.12
0.14
0.16
0.18
0.2
Max RFI 2018 Orbit D
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1
NValid Points 2018 Orbit D
0
20
40
60
80
100
120
140
160
180
200


Difference of the yearly averaged VOD: Asc - Desc Orbits 2018
0.2
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
-0.2



Modifications: The hypothesis has been specified along the article:

Abstract line 10-11: First, the spatial calibrations obtained with VOD from different orbit types are compared to check whether ascending and descending VOD can properly be merged.

Introduction line 74-75: The work carefully investigates the influence of: mixing morning and afternoon overpasses to maximize the number of observations per node

line 163: The first factor is the relevance of merging the SMOS overpasses with different local time

lines 166-168: Nevertheless, descending orbits will help fill the spatial gap in areas strongly affected by RFI. If both orbit types can be merged properly without impacting the quality of the calibration, the spatial extent of the AGB estimations will be increased.

Title of section 4.2 line 267: Relevance of merging ascending and descending orbits

line 276: Considering these results, ascending and descending orbits can properly be merged to compute the yearly L-VOD maps.

Sparse modifications in the paragraph lines 357 to 364: The effect of the time of observation is also evaluated. It is admitted that morning overpasses (6 am for SMOS) offer more stable surface conditions as the Earth's surface reaches a thermal equilibrium. Therefore, better (SM, VOD) retrievals are expected using the morning orbits. It has however no impact for the present application as the three cases (i.e. only morning, only afternoon, and the two combined) have similar performances at the timescale considered in this study, with a correlation coefficient R ranging between 0.85 and 0.87 (see Table1). The yearly averaging smooths the daily variability, caused by the vegetation water content. Both overpasses (morning and afternoon) can be merged without a strong impact on the calibration. This increases the number of observations per pixel to compute the yearly VOD average and improves the coverage of regions polluted with RFI.

lines 420-421: In particular, it is shown that SMOS ascending and descending orbits can and should be merged at the yearly time scale to estimate the AGB.

2. The second hypothesis of regional optimization, I think is a very nice one, but I am also surprised about the regional demarcations chosen in this study. I don't see the authors using lessons learned from VOD in previous studies to inform the choice of the selected regions. I provide more details below to hopefully help with this.

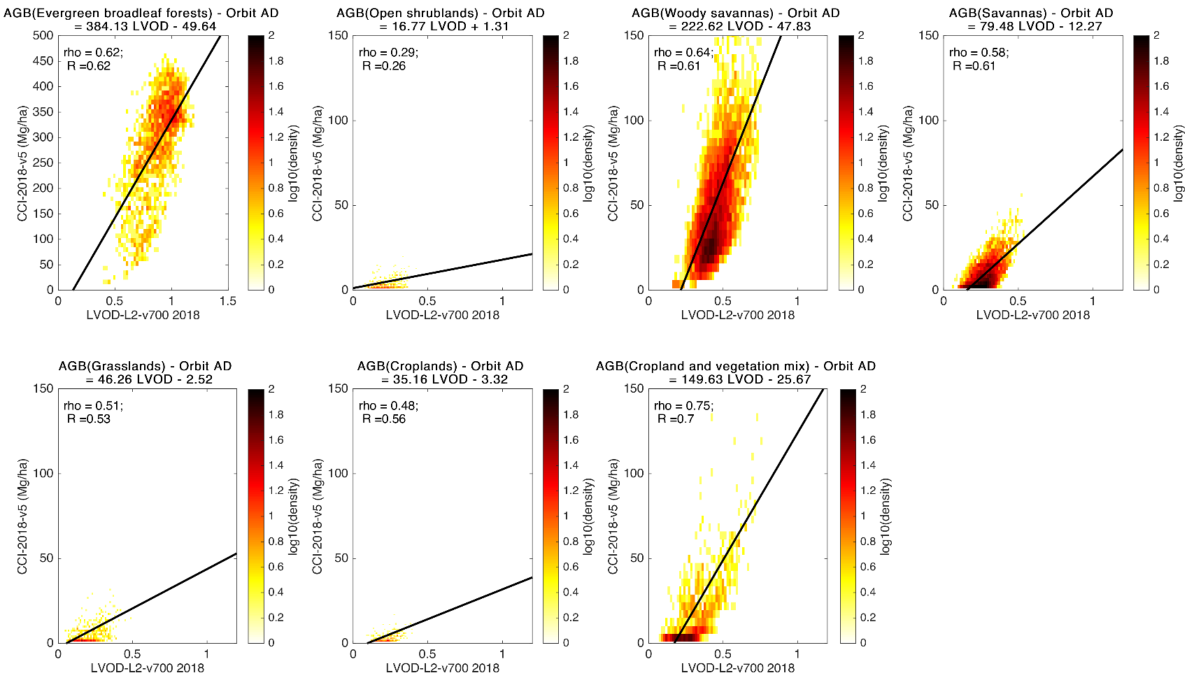
Lines 352-362: I think there might be other ways of properly understand the uncertainties in the datasets. just choosing a region like Africa is not a very practical choice since the continent has at least, three significant climate conditions that would impact the uncertainty propagation in the region. Choosing to look at how the metrics vary over VOD scenarios(perhaps binned VOD values from about 0.15 to 0.9) is a probably a better way to understand uncertainty propagations into the AGB product.

We grouped the reviewer's general comment 2 with their other comment 3 as these two comments point out the same concern on the chosen regional demarcations (specifically the African continent).

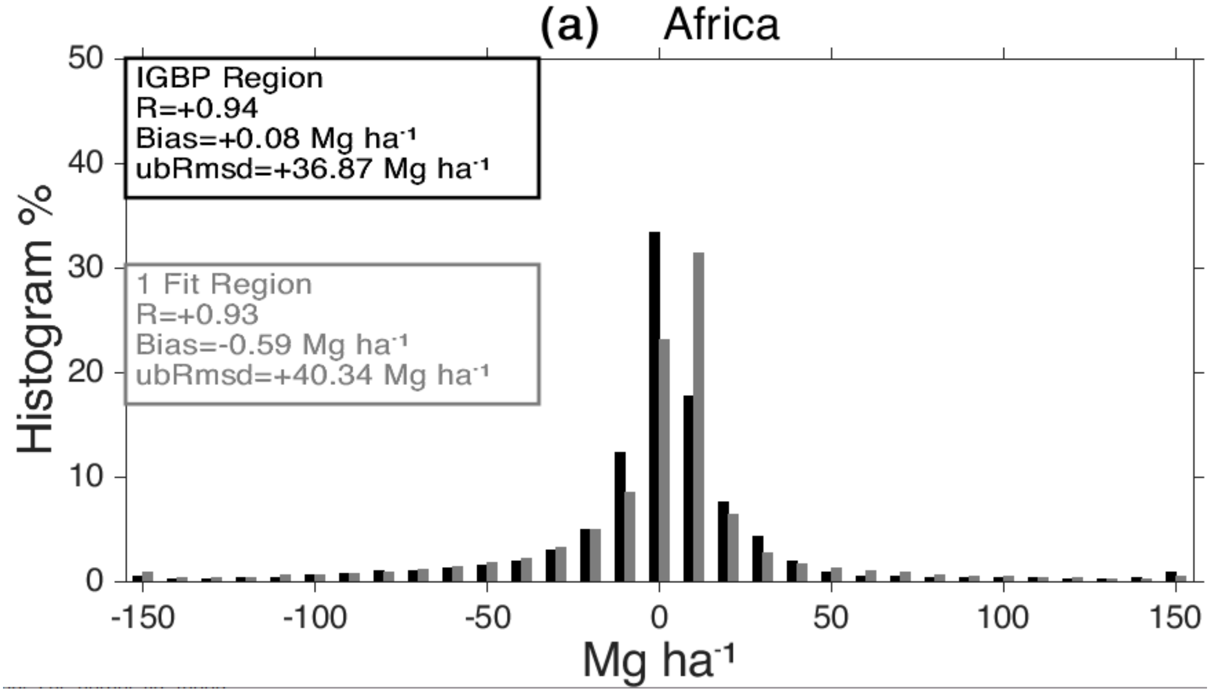
The African continent is indeed a vast region that encompasses several climatic conditions. However, the choice of this specific area is based on previous VOD-AGB studies (Rodriguez-Fernandez et al. 2018, Mialon et al. 2020, Wang et al. 2024). The entire African continent was also chosen as the L-VOD is very well correlated to the whole ecosystems of this continent (Wang et al. 2024, Rodriguez-Fernandez et al. 2018, Brandt et al. 2018), so we thought it would be useful for analysis and comparisons with these published works.

When designing the study, over Africa, we first conducted our analysis based on the IGPB landcover classification (as in Mialon et al. 2020). We established linear relationships between the L-VOD and the AGB per land cover class (see first image below). This naturally creates different VOD scenarios as suggested by the reviewer. Then, we estimated the AGB from the L-VOD per IGBP land cover over Africa. As shown by the second figure below, this gives very close results to the regional calibration with a single logistic fit. Over Africa, estimating the AGB per IGBP class does not impact the estimation as much as Global vs Regional estimates (Fig 7a)

Linear regressions between L-VOD and AGB per IGBP class over Africa - 2018:



Histogram of the differences over Africa between the calibration AGB from CCI v5 and the estimated AGB from a regional logistic fit (grey) and several linear fits per IGBP class (black):



As for the choice of the other regions (Tropics, Amazon and boreal regions), after conducting a per IGBP class analysis for the whole Earth, we figured that some classes could be grouped because they showed very similar results. Then, exchanges with biomass experts led us to consider and to analyze more particularly the forested regions such as the boreal and the tropical forests.

Rodríguez-Fernández, N. J., Mialon, A., Mermoz, S., Bouvet, A., Richaume, P., Al Bitar, A., Al-Yaari, A., Brandt, M., Kaminski, T., Le Toan, T., Kerr, Y. H., and Wigneron, J.-P.: 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, 15, 4627–4645, https://doi.org/10.5194/bg-15-4627-2018, 2018.

Mialon, A.; Rodríguez-Fernández, N.J.; Santoro, M.; Saatchi, S.; Mermoz, S.; Bousquet, E.; Kerr, Y.H. Evaluation of the Sensitivity of SMOS L-VOD to Forest Above-Ground Biomass at Global Scale. *Remote Sens.* **2020**, *12*, 1450. https://doi.org/10.3390/rs12091450

Mengjia Wang, Philippe Ciais, Rasmus Fensholt, Martin Brandt, Shengli Tao, Wei Li, Lei Fan, Frédéric Frappart, Rui Sun, Xiaojun Li, Xiangzhuo Liu, Huan Wang, Tianxiang Cui, Zanpin Xing, Zhe Zhao, Jean-Pierre Wigneron, Satellite observed aboveground carbon dynamics in Africa during 2003–2021, Remote Sensing of Environment, 2024, https://doi.org/10.1016/j.rse.2023.113927

Brandt, M., Wigneron, J.-P., Chave, J., Tagesson, T., Penuelas, J., Ciais, P., … Fensholt, R. (2018). Satellite passive microwaves reveal recent climate-induced carbon losses in African drylands. Nature Ecology & Evolution, 2(5), 827–835. doi:10.1038/s41559-018-0530-6

3. Thirdly, I am sure the authors are already aware that we don't really have any standard data for benchmarking AGB at such a global scale, thus, using any statistical analysis to quantify uncertainties needs to be done with care, especially when we use words like errors, which imply there is a benchmark. Perhaps, RMSD sounds like a more thoughtful word instead of RMSE. Biases basically inform us of which product may be potentially dryer or wetter and in which regions. And this interpretation should be more pronounced in the paper. In this case, Biases don't necessarily mean deviations as there is no standard.

Indeed, we fully agree with the reviewers comments. First of all, we changed the statistics and replaced the RMSE with the unbiased RMSD (as suggested in your other comment #2). The maps we used for calibration are our references for the study but are no benchmark. The reviewer also pointed out the wrong use of "error". We also agreed that we should use "uncertainties", and changed the manuscript accordingly.

Modifications:

line 274: *For this study, the bias is not the most important metric as it does not necessarily reflect a deviation from the true AGB value which is unknown, as no benchmark AGB map exists.*

Other Comments

1. Line 55: Perhaps it is more helpful to properly introduce the L-band VOD product here. The current form makes it look quite trivial.

We changed the sentences, to hopefully better introduce the VOD.

Modifications

line 56 : " This L-band VOD (or L-VOD, Wigneron et al., 2007) characterizes how much the surface signal is attenuated by the vegetation, as well as the vegetation's own emission. It is then defined as an optical depth. Theory and previous works (Jackson and Schmugge, 1991; Grant et al.,2012), showed that the L-band VOD is strongly influenced by the Vegetation Water Content (VWC). "

2. Lines 215-218: Authors should note that these statistics are no independent. Therefore, one would hardly find additional information from RMSE or Bias if R is already known. Secondly, I disagree with the choice of RMSE and Bias since there are not blind to systematic errors that may mask out relevant information from the anomalies of the data. In such a case, R becomes the most reliable metric or some sort of unbiased RMSD?

We agree that the RMSE and the bias are not independent. As suggested, we replaced the RMSE by its unbiased counterpart that is the ubRMSD, with the "D" referring to a difference (and not an error). We think the bias can add information for our analysis (e.g. regional vs global relationships), even though R and the ubRMSD are more important for our study. As shown in Fig 7, similar R values can hide very different ubRMSD and biases (example of the African continent).