Dear Editor

We approach ESSD with a revised version of the manuscript "The global forest above-ground biomass pool for 2010 estimated from high-resolution satellite observations". The revision considered Short Comments and Reviewers Comments. We have already replied to the Short Comments individually. Our replies to the two reviews are part of this document. Our point-by-point replies are in italic. We hope that we have correctly addressed in our replies the issues raised in the discussion and in the review phase.

Best regards
Maurizio Santoro (on behalf of all co-authors)

Replies to Reviewer Comments (RC1)

Santoro et al eloquently describe their global maps of [woody] forest biomass in a way that is both thorough and approachable. Unlike previous biomass maps, they leverage global SAR composites to first generate a map of growing stock volume, from which they then generate an additional map of the biomass density. As I understand it, this approach contrasts with those used to generate previous forest biomass maps which have instead used more direct approaches that are thus more correlative and perhaps, then, less reliable. Santoro and colleagues also generate an uncertainty map, making their product distinct from its predecessors in that regard; I particularly appreciate the transparency this affords the product. Overall, I commend the authors for a well written paper and robust product. I am admittedly not an expert on non-optical remote sensing and would thus characterize my feedback below as more representative of a potential end user. While I can't speak much to the methods Santoro et al use, I do feel this perspective is important given the number and breadth of biomass maps that have been published in the last 10+ years. Below, I have identified several minor issues that I feel should be addressed to ensure that users like myself properly understand and interpret these data. Some are inevitably a matter of opinion and I respect that the authors may feel differently. Overall, though, I think this manuscript and the data it described will (at last!) be great contributions to our field.

We are pleased that the manuscript has been well received. It is important that the dataset is correctly presented and we have taken care of implementing the changes suggested in this review. Our point by point reply to the comments below are in italic.

Minor Comments:

Line 87 – Baccini et al. (2017) would seem to be an appropriate reference for forest degradation.

Suggestion accepted.

Line 85 – Gibbs et al. (2007) would seem to be an appropriate reference for REDD.

It is indeed. The reference has been inserted.

Line 86 – You might also consider mentioned less conventional potential applications like attaining co-benefits from the CBD (e.g. Soto-Navarro et al. 2020).

Good point. Also this reference has been added.

Line 91 – The authority you seem to give the Bar-On estimate seems odd given that your maps could theoretically be used to improve their estimate. I would suggest (though, I do not require) that you change the language here to imply that Bar-On's estimate is speculative. Something like "Previous estimates have suggested that....". I would also explicitly describe the Bar-On estimate as a *carbon* stock estimate. By contrast, you map biomass (i.e. dry mass). If that distinction isn't made clear, readers may mistakenly compare your estimate to that of Bar-On which would not be inaccurate. Finally, I suggest that you report the Bar-On estimate in units of PgC (like you do later in the paper when presenting your own results) instead of GtC.

These are valuable suggestions and have been implemented. We agree that the Bar-on numbers are based on less accurate data than the dataset here presented. The reason for citing Bar-on's work was primarily to highlight that most of the live biomass on Earth is stored in woody vegetation.

Line 94 – The body of this paragraph is a bit discordant with its first sentence. I think your aim here is to say that biomass estimates are uncertain. If that's correct, I think you could do so more effectively. You might do so by changing the second and third sentences to something like: "However, our knowledge of the terrestrial biomass stock is relatively uncertain (Houghton et al. 2009). This uncertainty is well illustrated by the variance among forest biomass estimates: Pan et al..." Otherwise the transition from discussing total biomass C stocks (e.g. Bar-On and Houghton) to forest biomass C stocks (e.g. Pan, FRA) is an abrupt logical leap.

That's correct. We have modified the text following these suggestions while also trying to keep the original meaning of the paragraph, with Bar-on's work being cited primarily for their numbers on biomass repartition.

Line 105 – "Observable": do you mean "variable"? This is a term I haven't seen before.

We have slightly rephrased this sentence and focus now on the concept of "measurement".

Line 105 – "AGB": From this point forward, I think you're using AGB to implicitly refer to woody AGB (or the biomass of trees). If so, It's worth making that explicitly here. Many less-knowledgeable users of these maps mis-interpret them as representing the AGB of all plants rather than just that of trees and that leads to underestimates. I recommend explicitly defining it here (or earlier). Something like: "AGB (which hereafter we use to refer to aboveground biomass exclusively in forest trees)". And/or changing subsequent mention of "AGB" to "forest AGB". Do you use a certain definition of for forest/woody biomass? E.g. the FAO definition, A certain height, Etc.? If so, it would be worth stating that somewhere (if not here).

The paragraph between lines 104 and 114 is a review of previous mapping activities on "AGB" from a method perspective. Each activity might have had its own definition of AGB, which is, however, in this context of minor relevance. Further down, we review AGB data products; at this stage, we now provide details on the type of AGB that was estimated also taking into account that these data products have been assessed in our study. Our definition of AGB was provided first on line 119 (forest AGB); following this comment, the definition has been updated in the revision as well. The detailed definition of the AGB mapped in the dataset is provided in Section 3.1. Overall, we have revised the text to ensure that we always refer to forest AGB and woody biomass.

Line 115 – Baccini et al. (2012) represents 2007 and Baccini et al. (2017) represents even more recent years.

We have revised the sentence by referring to the full range of years covered by all global and biome-specific datasets cited two lines above. Baccini et al., 2017, is not referred to in this context because not publicly available and because not contributing to a global dataset.

Line 118 – The year of the Erb reference should be 2018

Thanks for spotting this. The reference has been updated.

Line 180 – I think you specifically mean investigations targeting *direct* estimation.

Yes. We have modified this sentence according to this comment.

Line 198 – The last sentence of this brief paragraph is awkward. Consider rephrasing.

We have removed the second part of the sentence and linked the first part with the text in the next sentence.

Line 298 – Does your uncertainty layer represent the standard deviation, as you say here, or the standard error? Below it sounds like you're propagating standard errors and the GlobBiomass website lists the uncertainty layer as representing the standard error (https://globbiomass.org/wp-content/uploads/GB_Maps/Globbiomass_global_dataset.html). Whichever it is, please make it abundantly clear here in the text and make sure your description is accurate and consistent throughout the manuscript (and beyond).

It is standard deviation. The term "standard error" was erroneously used in initial reporting. We ensure that the data distribution also refers to standard deviation.

Line 391 – It may also be worth comparing to regional/national AGB maps? Users often want to know whether it is appropriate to use global products to answer local questions. Obviously such a comparison wouldn't be comprehensive but even just showing for a few areas how well your map agrees with local products, might give users a sense of appropriate confidence or caution. This could also instead be done in the discussion if there are studies that have already made these comparisons with your data. One short comment on this manuscript notes that there is at least one such comparison that has been done in the U.S. Perhaps there are others as well and thus no need to do an extra analysis?

It is an obvious question to which extent this paper should compare the global dataset with local maps. This work was actually undertaken but the results were not easy to interpret. Discrepancies were often related to the different methods used to estimate biomass from the remote sensing data. Given the already long and dense paper in its current format, we opted to omit such analyses. Nevertheless, the suggestion is valuable and we have added a sentence to explain why we restricted our analysis to "global" maps. We have also added reference to the first few papers that have investigated the local value of the dataset. Considering the still small number of scientific publications highlighting the usefulness and caveats of the dataset, it is preferred to stay with the citations without attempting to provide a big picture, to which extent such a global dataset can be of use at the local scale.

Line 407 – Again, is "standard deviation" correct here?

Standard deviation is correct.

Figure 2 – How would you explain the areas with ~100% uncertainty? You don't mention these in the text but I suggest that you do. Are these areas that underwent a land cover change (fire, forest clearing, etc.) c.2010? Or does the model just do a poor job predicting biomass in certain areas (e.g. those with sparse woody vegetation)? It would likely help users to know this. Also, in my examination of your data, uncertainty can exceed the mean estimate (i.e. CV > 100%). How would you explain this to users? Is this a sign that error is not normally distributed? Also, this possibility should be indicated in the figure, either by allowing the colour-bar to surpass 100% or by changing the max label to something like "100+".

This comment made us aware that we omitted mentioning that the colour bars in Figure 2 were truncated at 500 Mg ha^{-1} AGB and 100% uncertainty to increase contrast. This information has been added to the figure caption. We have also updated the legend of each colour bar with a "+" in correspondence of the upper bound. A paragraph describing the map of the relative standard deviation in Fig. 2 has been added in reply to this comment.

"The AGB standard deviation, expressed in Fig. 2 relative to the AGB estimates, was on average 50% with an inter-quartile range of values between 44% und 61%. The rather constant relative uncertainty is also illustrated by the horizontal bars in the latitudinal profile of Fig. 2, which scale with the AGB level. The relative standard deviation was smaller than 100% for approximately 95% of the mapped pixels which explains truncating the colour bar of the AGB standard deviation map in Fig. 2 at 100%. The large majority of the AGB estimates for which the standard deviation exceeded the 100% level were below 20 Mg ha-1 such as in sparsely vegetated regions corresponding to the transition to tundra in Canada and Alaska, the Siberian Lowlands and Far East Russia or in poorly stocked forests such as in northern China and west Madagascar." (Lines 442-448)

Line 418 – It would be prudent to state explicitly here (or wherever you report your first result) that the unit you're using is Mg of *dry matter* rather than carbon. Users occasionally assume that biomass maps are reporting carbon stocks.

This is a valuable comment and we share this view. When defining our data product (Introduction), we refer to the unit in terms of Mg of dry matter per hectare.

Line 469 – I feel like your downplaying the fact that your mapped estimates saturate at high values. This is important for users to know and should be stated more frankly than saying "albeit more gently". Without euphemism: "you underestimate high biomass stocks". So, the highest value you give above (757 Mg/ha) for the U.S. Pacific Northwest is likely a gross underestimate? You could illustrate the degree to which that might be true by referencing a field measured estimate from that region. In general, I don't mean to imply that this is fatal issue (every biomass map seems to have this problem) but you should be as transparent about it as possible about it.

We admit that the wording used to support the interpretation of the scatter plots in Figure 5 (original manuscript) was a delicate issue. When looking at the global scatter plot, we agree that there is a strong underestimation at high AGB. The question is whether this is a systematic issue, i.e., it occurred everywhere, or a local issue. For this reason, we split the data per continent. These scatter plots reveal that the under-estimation differs depending on the continent. More to that, the largest underestimation occurred in Madagascar and Tasmania (reported in Table S8 and Figures S12, original manuscript).These islands harbor some of the densest forests on Earth but in terms of magnitude, the carbon stocks in these two regions are not comparable to the stocks from all other regions for which reference data was available and for which the scatter plots revealed minor over/underestimation. The "albeit more gently" tried to summarize these multiple evidences and we believe that it is a correct way of reporting our interpretation of the scatter plot.

Following this comment, we seem to understand that readers may misinterpret our assessment by inferring that if AGB is underestimated starting at 250 Mg/ha, the higher the AGB the larger the underestimate. To avoid this interpretation, we displayed in Figure S12 (original manuscript) an assessment of averages at the level of administrative units. The patterns differ depending on the region but there is no such evidence of a constant "saturation" of the averages or an increase of underestimation, which further confirms the indication by the continental scatter plots. Our strategy concerning validation, verification and assessment of the AGB map is outlined in Section 2.4.

With respect to the specific AGB of 757 Mg/ha as for any single estimate of AGB, we are not in the position to conclude whether this value is positively or negatively biased. This aspect is touched upon in the manuscript and we would refer to Herold et al. (2019) for a wider perspective on validation of maps.

Line 476 – The filled circles nicely show the saturation effect.

We would not refer to it as saturation when looking at the continental scatter plots (see also our reply to the previous comment) but indeed the reason for including filled circles was to aid the interpretation of the validation results.

Line 495 – The highest estimate you give above (757 Mg/ha) is from the *temperate* rainforest of the U.S. Pacific Northwest. That would seem to contradict this statement.

The sentence has been rephrased to avoid this contradicting statement.

Line 503 – I suggest breaking this discussion of the uncertainty layer out into a separate subsection (even if brief). Hardly any biomass maps are accompanied by an error layer so this could be viewed as a real strength of yours. As I've noted above (in the context of Figure 2), I think there are simple ways you could elaborate on this a bit to help users understand what it represents and how to use it.

Thanks for the sign of appreciation. The idea was to present the AGB map and the uncertainty layer (Fig. 2, original manuscript) with some related analyses (Fig. 6, original manuscript) one after the other. By splitting the presentation of the uncertainty layer from the AGB layer, we fear that there would be some back and forth flipping when reading the paper. We value the suggestion but prefer to keep the structure of the results unaltered. Nevertheless, we have added a description of the AGB standard deviation (in addition to the text written to accompany Fig. 2, see comment above) and some explanations concerning the origin of the large uncertainty in some regions. In support of this explanation, we have inserted a new figure in the Supplement (Fig S11). We hope that this approach is satisfactory.

Line 540 – I believe the FRA provides separate estimates for planted vs. natural forests. Are you considering both together here? If so, please make that clear in the text. I'm not sure about the 2010 FRA, but the 2015 version also considers "woodlands" separately. Presumably all of these categories fall under the purview of your map?

This is a well-thought comment. We spent a considerable amount of time to ensure that we would compare the right numbers. FRA 2010 provided separate AGB numbers for forest land and other wooded land. It also provided area statistics for forest land, tree out of forests and other land. Here, we used the numbers reported by each country for forest area and forest AGB. In terms of AGB, the OWL AGB is seldom reported. When reported, it represented a minor fraction of the total (forest+OWL) AGB except for a very small number of countries in Africa and Asia. Finally, accounting for OWL AGB in our map-to-FRA assessment was negligible because the density and stocks were scaled accordingly with the area of the tree out of forest category, representing the extent of OWL.

Line 557 – Mainly? It seems like a substantial portion could be due to your underestimates of high biomass.

We are not sure that we understood this comment correctly. While we estimate 17% more AGB than the FRA and our estimate of the forest area is 40% larger than in the FRA, it makes sense that the AGB stock is 64% larger.

Line 642 – At the U.S. state level, Spawn et al (2020) appear to compare your maps (separate from their modifications) to the U.S. FIA and show good agreement (see their figure 9). They don't employ the CCI Land Cover map in their comparisons so seemingly support your hypothesis that the difference is explained by the CCI map and not your biomass predictions?

It is important that an independent study came to similar conclusions. As a side note, in a separate study, we analyzed the area attributed by CCI to forest in the U.S. and noticed some remarkable differences with respect to the state-wise FIA values. When recalibrating the AGB stocks per state with the ration (forest area CCI / forest area FIA), we obtain a very good agreement with the stocks estimated by FIA.

Line 654 – I think "illustrating" is more appropriate, here, than "indicating".

Corrected.

Line 684 – The statement about minimal biomass at high latitudes warrants a citation.

We emphasize that the 30 Mg/ha level was not exceeded by any of the datasets. To confirm the validity of this AGB level, we introduced citations based on published data from inventory measurements.

Line 749 – I think you're saying, here, that if you further account for the woody biomass in non-forest CCI land cover classes, the total estimate increases? Please clarify in the text.

This is correct. The text has been amended.

Line 751 – Here you're saying that your maps show 600 Pg of *woody* biomass. This is the number readers might improperly compare to the Bar-On *carbon* estimate you note in your introduction section – I'd make it abundantly clear here that this is 600 Pg of *dry biomass*, not carbon to prevent any unjust comparisons with Bar-On or others. This may also be an opportune place to mention the maps by Spawn et al. (2020) which use your woody biomass and additional maps of non-woody biomass to generate a total AGB (and separate BGB) estimate. Users may want to know when when/why one is more appropriate than the other. Whether here or elsewhere (perhaps the last paragraph?), I'd suggest (though, don't require) you make appropriate recommendations. Spawn et al. may also help put your number in appropriate context (i.e. make it more comparable to Bar-On).

We have clarified that our AGB estimates are relative to biomass and added a number for carbon so to make numbers comparable in this unit. We also added a reference to Spawn et al. to highlight how the integration of our dataset with other can provide a more comprehensive picture of the global carbon stocks in vegetation. Nonetheless, we prefer to avoid a comparison of numbers from other studies that is beyond our analysis (lines 749-760 of the original manuscript) because it would involve an explanation of discrepancies. Here, the nuances of definitions adopted by the different study may be easily overlooked, leading to a potentially wrong interpretation of numbers.

Line 773 – please add "woody" before "AGB".

Added.

Overall, fantastic work!

Thanks

Replies to Reviewer Comments (RC2)

1. GENERAL COMMENTS

The manuscript addresses a topic of immense importance for climate change research and national reporting to international environmental agreements, providing improved geospatially explicit estimates of the global carbon pool of forest ecosystems. It describes the development of global above ground biomass and growing stock volume maps at 1 ha spatial resolution from, primarily, satellite-borne radar data, and includes a thorough accuracy assessment. A comparison with other global AGB maps is provided, as well as with country reports from the FAO Forest Resources Assessment. The manuscript is well-written and generally easy to follow, with the authors making great effort to clarify and discuss both strengths and weaknesses of the dataset. Only the sections on the assessment of global forest biomass resources (3.4) and comparison of AGB map estimates (3.5) are long and rather heavy to ingest for the reader. Consolidating these to reduce size somewhat would improve readability. The dataset is easily accessible for download and the geotiff format accommodates straight-forward use with standard GIS software.

We are thankful for the review, the comments to the manuscript and the positive feedback to the data product. We have implemented the suggested modifications. We have also thought how to reduce the amount of information in Sections 3.4 and 3.5. Both sections underwent substantial pruning during the internal review before the original manuscript was submitted. In all honesty, we find it hard to further consolidate these Sections without impacting the overall message given by this paper in terms of reliability and new evidences. To better guide readers through Section 3.4, we have split results in two sub-sections. Some modifications were introduced in Section 3.5 as well but without changing its structure. Our point by point reply to the comments below are in italic.

2. SPECIFIC COMMENTS

Line 136: ": : :PALSAR acquired images in the Fine Beam (FB) mode with 25 m spatial resolution: : :" should be ": : :PALSAR acquired images in the Fine Beam Dual polarisation (FBD) mode with 20 m spatial resolution: : :". Note: acquisitions were made at 20 m resolution for the FBD data (and 10 m for FBS).

Corrected.

136-138: "Image acquisition followed a predefined observation scenario with the aim of achieving spatially and temporally consistent large-scale observational datasets". Add reference to ALOS PALSAR acquisition strategy: Rosenqvist, A., Shimada, M and Watanabe, M. ALOS PALSAR: A pathfinder mission for global-scale monitoring of the environment. IEEE Transactions on Geoscience and Remote Sensing, Vol. 45, No. 11, pp 3307-3316, Nov. 2007. doi.org/10.1109/TGRS.2007.901027139: ": : :yearly mosaics of the radar backscatter: : :". Mention also that the PALSAR mosaics have been subject to radiometric terrain correction and are provided as gamma-0.

One sentence and a reference have been added.

161-172: Please add a sentence to clarify how the time difference between the GLAS data (2003-2009) and the other datasets (approx. 2010) was handled to avoid systematic bias.

We have added one sentence to explain that the LiDAR observations were not used as predictors of GSV but to derive estimate of model parameters that can be considered time invariant. For this reason, the impact of having a LiDAR dataset not contemporary to the SAR observations is minimal.

Section 2.2 (AGB estimation): Throughout section 2.2 (and a bit of 2.3) and in equations 1, 3, 7 and 8, sigma-0 is used to denote the radar backscatter, while the SAR data (or at least the L-band mosaics) are provided as gamma-0. This probably does not matter for the models but a brief statement to acknowledge this would be useful to add.

The symbols with sigmas in these Sections and Equations indeed need to be interpreted in a generic way as backscattering coefficients. To make sure that in particular the symbol sigma0_for is correctly interpreted, we now explain that it represents "the forest backscatter, i.e., the SAR backscatter observation from an ALOS PALSAR or an Envisat ASAR image (Section 2.1)" directly after Eq. (1) is introduced. We hope that this is sufficient without having the need to specify that an image was processed to a certain type of backscatter coefficient.

239: "hyper-temporal". How many ASAR scenes were typically available for any given location? The word hyper-temporal gives the impression that Sentinel-1 style temporal density was available, which was hardly the case for ASAR (over perhaps Europe). Perhaps "multi-temporal" would be a more appropriate description?

Thank you for this comment. The term hyper-temporal was first used in relation with previous studies with ASAR observations of the northern hemisphere to indicate a very large number of observations (> 100). In support of our statement on the "hyper-temporal dataset", we have realized that we did not present a figure showing the number of observations available per pixel. In the revision, the term "hyper-temporal" has been removed because redundant. At the same time, we have introduced a figure in the Supplement (Fig. S1) showing the number of observations, which by itself is in our opinion important to understand the necessity of a large number of observations at C-band to ensure that the resulting estimates of GSV are reliable.

260: ": : :uncompensated topographic effects in the ALOS PALSAR mosaics: : :". How did you parameterise the uncompensated topographic effects to define the weighting factor? Could you also add a sentence (here or elsewhere in the text) about the cause of these uncompensated effects? Was it due to a lack or, or insufficient, radiometric slope correction in the PALSAR mosaics?

Since we decided to focus on the data product in our manuscript, technical aspects not directly involved in the evaluation of the results were reported in the Supplement. In this case, we refer to Eq. (S10) in Section A.4 of the Supplement. However, in our revision we have added "residual" to "uncompensated topographic effects" in the main body of the manuscript to clarify that topography was compensated but because of either the quality of the DEM or the procedure adopted, the correction did not completely compensate for the effect of sloped terrain on the backscattered intensity.

285: Explain how p1 and p2 in Eq. 6 are derived. Do they vary regionally?

Again, since technical aspects are reported in the Supplement, we refer to Section A.5 in the Supplement as well as Quegan et al., 2017. Both are duly cited in the main body of the manuscript. In reply to the question, in situ data on stem biomass and total biomass were stratified by biome and the model was fitted for each biome. Unfortunately, the paucity of data did not allow for a finer regionalization. The implications are discussed throughout the paper at several instances.

395-396: ": : :the dataset were converted: : : from AGB carbon units (MgC ha-1) to AGB (Table S5)". A bit confusing statement. (Is this perhaps a typo?) Is there a difference between "AGB carbon units" and "AGB"? Table S5 offers no clue what you mean here. Please clarify. The dataset comparisons in Section 3.1 illustrate the great spatial detail of this dataset compared to earlier mapping efforts and the importance of using high resolution satellite data as basis for the mapping. Impressive.

AGB refers to the density of the organic mass, AGBC refers to the density of the carbon mass. AGBC is roughly half of the AGB. We have rewritten the sentence to avoid misunderstanding by specifying the carbon fraction value used to convert AGBC to AGB.

3.3 (Spatial distribution of AGB) For clarity, suggest to indicate early in the section that the acronyms used (TAr, TeDo, TeM, etc) refer to the FAO global ecological zone nomenclature (it becomes clear further down in Fig 6 but one is left wondering for a while)

To avoid this issue, we cite now Fig. 6 before any of the FAO acronyms are used in the text.

3.4 (Assessment of global forest biomass resources). 3.5 (Comparison of AGB map estimates) The sections go into a high level of detail and are rather heavy to ingest for the reader. While I leave it to the discretion of the authors, consolidating these to reduce size somewhat would improve readability.

We agree that Section 3.4 is very dense and long. This Section underwent substantial pruning before submission and reports the main results at global/continental and national level. In our opinion, it is important to report result as such level of detail to build confidence in the dataset. Also, our paper targets a large variety of readers with interests that range from global perspective to national interests. To account for different points of view, we have broken down the Section into two sub sections, each dealing with one of the levels mentioned above. We hope that this minimal intervention can be considered acceptable.

Section 3.5 is also dense and it underwent pruning before submission. After reading this Section through, we have identified several reasons for opting to leave it unchanged. The text is not particularly long for a results section, as it extends over 1 and ½ page and the is supported by two figures and one table. Grouping the Section according to an analysis per biome would have resulted in many small fragments. As for the assessment against the FRA data, we wanted to ensure that the comparison of maps is done and reported thoroughly to provide evidences concerning the reliability of our map product in a broad context. Finally, we think that this Section attracts the attention of a readership interested in map-based estimates of biomass, who probably pays less attention to the content of Section 3.4.

Figure 7: - The figure does not seem to indicate "Country AGB", but actually "Country AGB density", or rather "Country AGB average density", as you indicate the unit [MG/ha]. - Spent quite some time trying to get my head around the rather complex figure 7. It would be helpful to include some kind of legend which clarifies the different parameters you are using to illustrate not only a comparison between your AGB estimates at those of the FRA report, but also country absolute and relative forest area. Something like: - Size of circle - country forest area in CCI map (same scaling across all continents) - Colour of circle - relative country forest area on continent – Colour ramp - Add units to indicate (blue - smallest area on continent, brown - largest area on continent)

Throughout Section 3.4, which includes Figure 7, we have noticed this misleading wording (country AGB) and have replaced it with average AGB. The suggestions to improve the readability of Figure 7 are very valuable. The legend of the color ramp has been added. The figure caption has been modified.

652: "All datasets showed: : :". Suggest to remind the reader about which datasets, AGB maps, you are referring to here (as one may have lost track of that after the rather dense section 3.4!)

We refer now to the "map datasets" and to Table 3 where the datasets are listed.

Figure 10. Very helpful summary of section 3.5

Thanks for the positive feedback

722: "Overall, our maps reproduced: : :". Suggest to modify to "Overall, the results indicate that our maps reproduced: : :"

Corrected

3. TECHNICAL CORRECTIONS
Line 129: "Phase" should be "Phased"
140: "publically" should be "publicly"
141: "0.00022_ " should be "0.000225_ "
809: "SoloEO" should be "soloEO" ;-)

All corrected.

Table S7. (very informative) "COUNTRY" should be "COUNTRY/TERRITORY" "{malvinas)" should be capitalised: "(Malvinas)" "Guinea0Bissau" Typo. Should be "Guinea Bissau" Taiwan is missing Suggest to report separately as "Republic of China (Taiwan)" and "People's Republic of China", resp. "The former Yugoslav Republic of Macedonia" new official name "North Macedonia"

We have updated the table following the terminology used in the FRA. "Country" has been replaced with "Country/area". There is no country report for the Republic of China (Taiwan) nor there is an entry in the FRA 2010 global tables . For North Macedonia we used the name adopted by the country in the country report submitted to the FRA 2010. All other typos have been corrected.