We appreciate the time and efforts of the editor and referees in reviewing this manuscript and the valuable suggestions offered. In addressing all issues indicated in the review report we trust that the revised version meets the Reviewers' comments and the journal's publication requirements.

[Reviewer 1] General Comment:

Chen et al. generated high-resolution (~1 km) explicit maps of above- and belowground biomass) for woody vegetation in China between 2003 and 2020. I believe the combined use of low-frequency microwaves and laser remote sensing data provides a more accurate estimation of biomass. Overall, the manuscript conducted good work on data collection, statistic analysis, and results presentation. This map could be important for monitoring and estimating woody biomass in China. It has the potential to serve as input or for calibration in Earth System Models. I think it is publishable if several minor issues can be addressed.

Response: Thank you for your positive comments to our work. We have addressed the suggestions you raised, and improved the data selection and data making algorithms.

[Reviewer 1] Specific Comment:

1) Line 126: for MOD44B v006- dataset need citation and description.

[Response]: Thanks for the remind. In the revision, **we adopted the newest version: v061** of this dataset, and then added the citation and the description as follows: "By adopting the MODIS vegetation continuous fields (VCF) data (MOD44B v061) **which includes three ground cover components: percent tree cover, percent non-tree cover, and percent non-vegetated (Dimiceli et al., 2022)**, we first calculated the mean tree cover (hereinafter, TCmean) and non-tree vegetation (short vegetation) cover (hereinafter SVCmean) during 2011–2015, and resampled them from 250 m to 1/120°." (Lines 149~154 in the revised manuscript).

2) Line 144: 10 m×10 m plots were not included as the training target here. What is the plot area for the training dataset, such as mean, sd of the area?

[Response]: In the original manuscript, the plot sizes, the time periods and the methods to obtain the AGB records were all different, which may introduce uncertainties to the benchmark AGB mapping. Therefore, following your comment and the comments of other reviewers, we **selected another more standardized** in-situ forest AGB carbon stock dataset. We revised the data description as follows: "A reviewable, consistent ecosystem carbon stock inventory was conducted in China between 2011 and 2015 (Tang et al., 2018). We requested the AGB carbon stock (AGBC) data at more than 5,000 30×30 m sized forest plots from the authors. Due to the scale mismatch between the maps of biomass, canopy height or tree cover and the field measurements, we dropped out the data within the 1/1200° resolution grids in which the standard deviation of tree cover was greater than 15%, according to (Chang et al., 2021), leaving 2444 homogeneous forest plots remaining (Figure 2)." (Lines 117~123 in the revised manuscript).

3) Line 146: the conversion of plot level AGB and pixel-scale introduce uncertainties, you could mention it in discussion?

[Response]: Following your comment, we have revised the source of forestland fraction data to reduce the uncertainties in the conversion of plot level AGBC to grid-scale wall-to-wall AGBC, following: “The AGBC records in these forest plots were further multiplied by the mean fraction of forestland over 2011–2015 in the corresponding grid, which was computed from the annual 30 m resolution China Land Cover Dataset (CLCD) (Yang and Huang, 2021).” (Lines 124~127 in the revised manuscript). Moreover, we also **added a discussion on this issue**, following: “During benchmark AGBC mapping, we converted the in-situ AGBC data at forest plots into the grid-scale average AGBC by multiplying the fraction of forestland during the time period of field investigation. Considering the overall high-quality of the China's land-use/cover datasets developed via **human–computer interactive interpretation** of Landsat images (Liu et al., 2014; Yang and Huang, 2021), and that the producer’s accuracy (PA) and user’s accuracy (UA) for forestland classification in the CLCD dataset used in this study were **73% and 85%** respectively, the errors within the benchmark AGBC mapping induced by the scale conversion based on the forestland area fraction was generally limited.” (Lines 371~378 in the revised manuscript).

4) Line 156: you need to describe hyperparameter tuning for RF.

[Response]: We have added the description accordingly, following: “… we trained ten-fold RF models **using MATLAB R2021a®**. **The number of regression trees was set to 500**” (Lines 210~211 in the revised manuscript). Please also note that during the benchmark forest AGBC mapping in the revision, we applied nonlinear regression between GlobBiomass 2010 and the plot measurement-based grid-scale AGBC instead of using the RF model.

5) Line 232: 2.3 High-resolution woodland AGB mapping in China from 2003 to 2020. Why is RF simulation under 1/12 resolution instead of keeping everything at 1/120°?

[Response]: Following your advice, we have **changed the algorithm**, and now perform the long-term continuous forest AGBC simulation **at 1/120° resolution** by directly utilizing the MODIS VCF data. Please refer to section 2.2 in the revised manuscript for details.

6) Line 246-250: data processing and rescaling include many assumptions which can introduce uncertainties, should mentioned in discussion.

[Response]: We agree that the previous method contained many assumptions which can introduce uncertainties. In the revision, we have **largely simplified the method but without reduction in data quality**. Now, there remains **just one main assumption**. This assumption is described as follows: “…, we first calculated the mean tree cover (hereinafter, TC_{mean}) and non-tree vegetation (short vegetation) cover (hereinafter SVC_{mean}) during 2011–2015, and resampled them from 250 m to 1/120°, the same resolution as the benchmark AGBC map for 2011–2015. Because the canopy heights of trees are usually similar **within a small area**, the **regional AGBC per TC_{mean} can be assumed the same**, which is referred to as the ‘homogeneous assumption’ hereinafter.” (Lines 155~157 in the revised manuscript).

In order to keep this assumption basically correct, **the maximum searching window was set to 9×9**, which is a small area of approximately **9 km× 9km**. The related description reads: “However, if the regression failed even if the window size has reached 9×9, we **stopped expanding the searching window to avoid the ‘homogeneous assumption’ being invalid**” .

7) Line 314: what is the data distribution of the plot sites AGB and BGB? Normal distribution or other?

[Response]: Following this comment, we have added the cumulative frequency curve and histogram of the AGB carbon stock measurements at 2444 homogeneous forest plots, as well as those of AGB and BGB data at 8182 forest forests with both AGB and BGB measurements as Figure S1 in the revised Supplementary Information. This figure is also shown below. According to the figure, the collected forest AGBC records, AGB and BGB data in this study **all exhibit a positive skewed distribution**. Whether the data follows the normal distribution or not may not influence the random forest model efficiency.

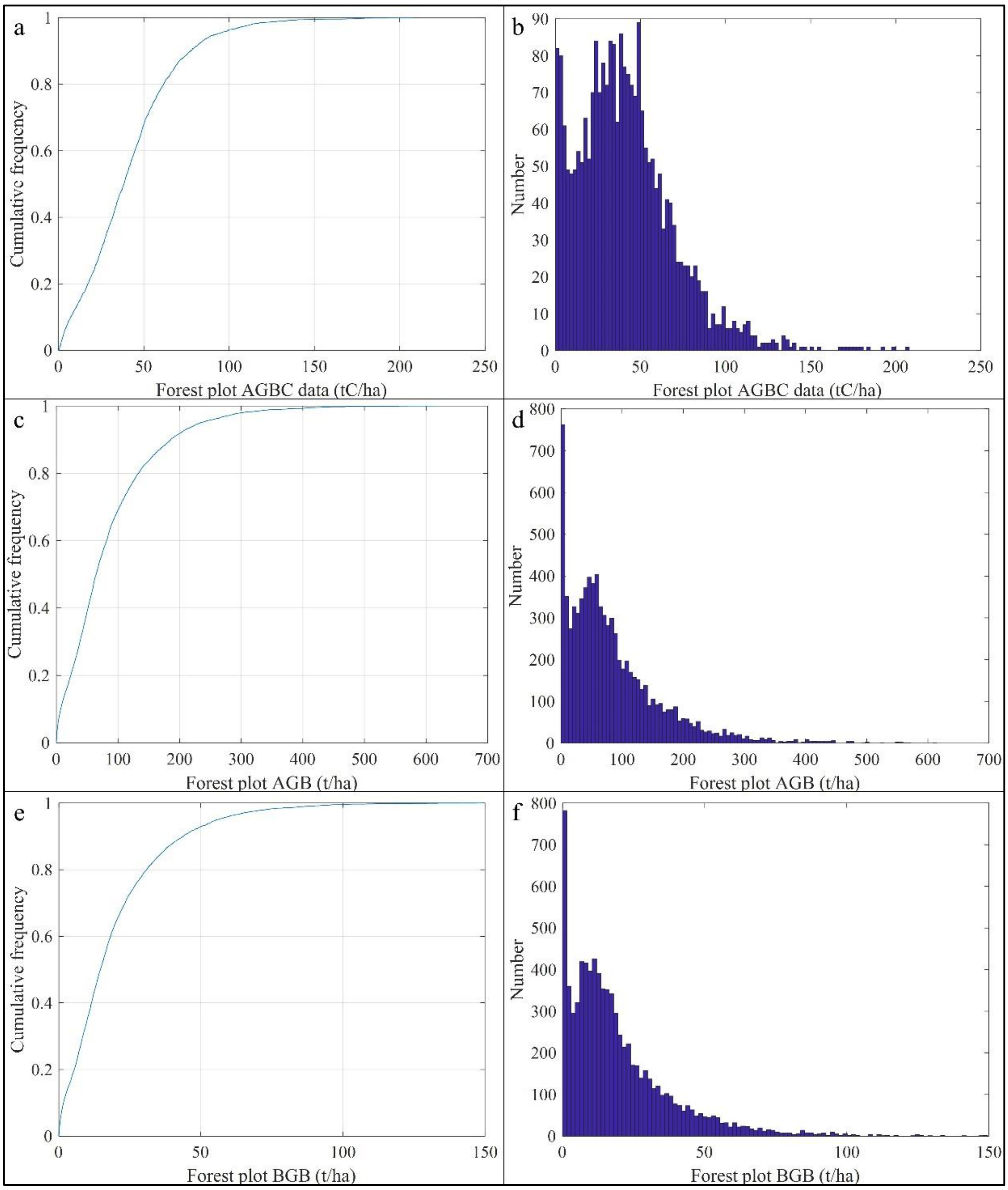

Figure R1. The (a, c, e) cumulative frequency curves and (b, d, f) histogram of (a~b) AGB carbon stock measurements at 2444 homogeneous forest plots; (c~d) AGB data at 8182 forest plots with both AGB and BGB records; (e~f) BGB data at 8182 forest plots with both AGB and BGB records.

8) Line 376: why does why RF of BGB show higher R^2 than AGB?

[Response]: Because **BGB is closely related to AGB** (Huang et al., 2021), the R^2 of **plot-level BGB** estimation using **plot-level AGB measurements**, forest stand age records and climatic backgrounds is expected to be very high. Meanwhile, we related multiple **remote sensing-based AGB maps** with **plot-level AGB records**. Due to the **errors and uncertainties within these AGB maps** and the **scale difference** between these AGB maps and in-situ measurements, the random forest model R^2 in the original manuscript (or the regression R^2 in the revised manuscript) will be a bit lower (Chang et al., 2021).

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We appreciate the time and efforts of the editor and referees in reviewing this manuscript and the valuable suggestions offered. In addressing all issues indicated in the review report we trust that the revised version meets the Reviewers' comments and the journal's publication requirements.

[Reviewer 2] General Comment:

This manuscript introduced the annual high-resolution (~1km) maps of woody biomass from both above- and belowground in China from 2003 to 2020. National biomass maps at such high temporal and spatial resolution are very important for ecological studies, earth system modeling, forest and ecosystem managements etc. The author utilized a wide range of remotely sensed and field plots data from different sources to generate the maps. Lots of efforts dedicated to clean data, match datasets of different spatial and temporal resolutions. Limited by the inconsistency among different datasets, a number of assumptions have been proposed to in the generation of final products. Some of them are not reasonable, particularly about the "benchmark map", and "calibration factor". Substantial experiments and changes are needed to make the final maps more trustable. Here are more specific comments.

[Response]: Thanks for your suggestions. We agree that in the original manuscript, there are too many assumptions and 'calibration factors', which could introduce uncertainties. Following your comments, we have changed the data producing algorithms substantially. Now, the revised method just contains one main assumption, whose validity has been well discussed in the manuscript, and **no 'calibration factor' exists any more**. We believe that by following your suggestions, the revised algorithm is a great improvement to our previous version, and thus the final dataset would be more trustable.

[Reviewer 2] Specific Comments:

1) Figure 1: Too complicated to read. Summarize the main workflow instead of list all method details. More details can be described in caption and manuscript.

[Response]: We agree with you that the previous method was too complicated, and contained too many assumptions. We have **simplified the method** and got rid of those suspicious assumptions. Now, the main workflow (Figure 1 in the revised manuscript) is simplified as below, and the details are described in the manuscript.

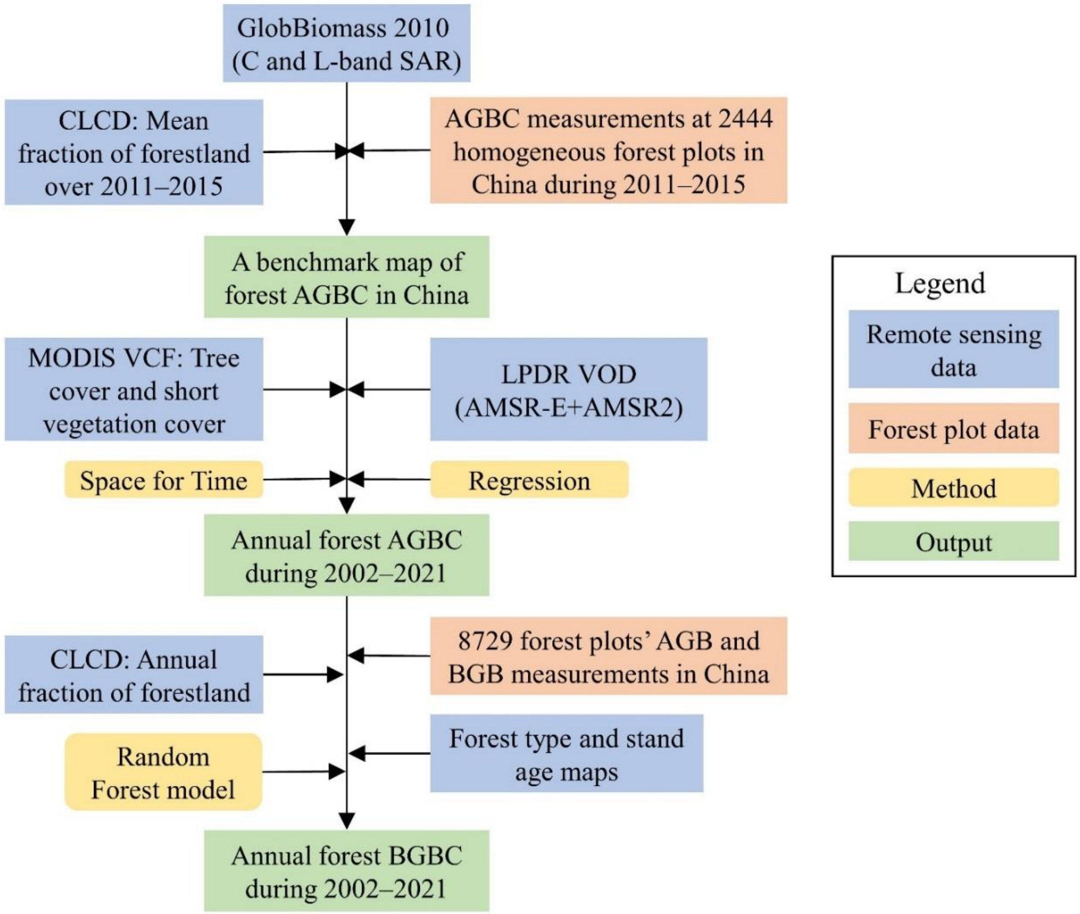

Figure R1. Workflow of forest biomass carbon pool monitoring in China during 2002–2021. AGBC, BGBC: Aboveground and belowground biomass carbon; VCF: Vegetation Continuous Fields; LPDR VOD: global land parameter data record- Vegetation Optical Depth; CLCD: China Land Cover Dataset.

2) Line 110 MAD: Need more details. How were outliers detected and defined?

[Response]: MAD means the '**Median Absolute Deviation**' method. MAD of a set of data (X) is calculated following: MAD=median(|Xi−median(X)|). Then, the data values that are more than 3.5 times of MAD deviating from the median of X was detected as outliers. In the revised manuscript, following your 4th comment, we applied the canopy height dataset developed in Liu et al, 2022 (https://doi.org/10.1016/j.rse.2021.112844), and do not process ICESat-2 data by ourselves. As a result, **the MAD outlier detection method is no longer utilized although it is an efficient outlier detection method**.

3) Line 110 "the average weighted by the corresponding canopy cover fractions." How was the weighted average height calculated? Averaged over 1km pixel or ICESat-2 footprint?

[Response]: In the original manuscript, it was the weighted average **over each ICESat-2 footprint**. In the revision, following your next comment, we **directly adopted the high resolution (30 m) forest canopy height map** for China, which was developed by interpolating the **ICESat-2 and GEDI data** in 2019 through a neural network (Liu et al., 2022). Therefore, **this data processing procedure no longer exists, and the sentence was removed from the manuscript**.

4) Line 110: “we mapped forest height over China” There are tree height maps at fine resolution over China such as Liu et al, 2022 (https://doi.org/10.1016/j.rse.2021.112844). Have you compared your mapping with these products? These height products can be used directly instead of generating a new height product.

[Response]: Thank you for this data recommendation. We missed this dataset because that article has not been published when we were doing this study. Following your recommendation, we have **downloaded the tree canopy height map developed in (Liu et al., 2022)** and compared it against the plot measurements-based grid-scale aboveground biomass carbon (AGBC) instead of mapping forest canopy height in China by ourselves.

5) Line 140: “it is supposed that the spatial pattern of woody biomass at 1 km resolution would not change much from around 2000 to 2017~2020”: It is a not solid assumption. If the spatial pattern would not change much, why do you want to estimate temporal AGB/BGB? Land cover change, urbanization, reforestation, restoration and natural forest growth can all lead to changes in spatial pattern of woody biomass.

[Response]: We agree that land cover changes, e.g., urbanization, reforestation, can lead to changes in the spatial pattern of AGB. In the revision, this suspicious assumption was no longer used because we requested a reviewable, consistent forest aboveground carbon stock (AGBC) inventory dataset for China between 2011 and 2015 (Tang et al., 2018) from the authors. Because in this inventory dataset, all **AGBC measurements were conducted in the same way over a short time period that is close to the nominal year of GlobBiomass 2010 dataset**, we can now directly regress GlobBiomass 2010 against the grid-average forest AGBC derived from these plot measurements, and then used the regression relationship to obtain an improved benchmark AGBC map in China between 2011 and 2015.

6) Line 145: “The extreme values (the highest and lowest 1%) were excluded as well. “The definition of extreme values is pretty arbitrary, sometimes, 1%, sometimes 2.5% (above) need more justifications.

[Response]: The exclusion of extreme values of BGB predictors was just designed to **avoid the PDP (Partial Dependence Plot) lines being extended to data scarce areas** which may contribute to some incorrect understanding on the partial influence of forest AGB or stand age on BGB. Hence, **this step is not related to the forest AGBC and BGBC data producing**. Following this comment, we have **abandoned all other extreme value definitions** (e.g., 2.5%) in the revised manuscript, and added the explanation in Line 237.

7) Line 160: “AGB maps are for 2017~2020, they are just used as the indicators of the spatial variability rather than the absolute values).” Not a reasonable assumption. The AGB maps for 2017-2020 are more likely to be AGB backmark-2020s rather than 2000s.

[Response]: We have changed the reference plot measurements dataset as well as the predictor AGB map in the revision. Now the **difference between the nominal time periods of the AGB map and the plot inventory data are minimized**. Please refer to the response to your 5th comment for details.

8) Line 170: Through cumulative distribution function (CDF) matching among different VOD products, the vegetation optical depth climate archive (VODCA) was developed (Moesinger et al.,

2020).” Unclear how VODCA was developed. Did you do these or it is a product by itself??

[Response]: VODCA is a long-term continuous VOD dataset developed by Moesinger et al. In the **original manuscript**, we **utilized this product and performed some calibrations** on it. In the revision, following the recommendation of another reviewer, we **selected a higher quality global long-term microwave VOD dataset** called the ‘land parameter data record (**LPDR**)’, which was generated by using similar calibrated, X-band brightness temperature retrieved from both AMSR-E and AMSR2 (Du et al., 2017).

9) Line 215: Therefore, the mean bias of AMSR2-based VODCA data during 2013~2018 compared to that before 2012 could be estimated as the difference between the mean annual VOD calculated based on the above regression coefficients as well as LAI and VCF data during 2013~2018 and the mean value of the adjusted VODCA’s medians over that period.” Unclear. Unclear what is generated and how or why?

[Response]: Sorry for the unclear descriptions. The bias between the AMSR2-based VODCA data and AMSR-E-based VODCA data will contribute to temporal inconsistency within the VODCA dataset. In the original study, **we estimated this bias by referring to other temporally-continuous vegetation cover dataset, including LAI, just as these sentences said**. However, we agree that the calibration of VODCA dataset in this way was still quite rough. Therefore, in the revision, **we abandoned the use of VODCA dataset.** Instead, we chose the ‘land parameter data record (LPDR)’ product, which was generated by using **similar calibrated** brightness temperature retrieved from these two sensors, and thus the bias will be smaller (Du et al., 2017).

10) Line 220: What is “the CDF matching algorithm”?

[Response]: The Cumulative Distribution Function (CDF) Matching is a method used to **remove systematic biases** or **rescale** the signal from **two different sensors or datasets**. Please refer to the article ‘Bias reduction in short records of satellite soil moisture’ (Reichle and Koster, 2004) for details. **In the revised manuscript, this method was no longer used** since there are no such two datasets that need to be rescaled and fused.

11) Line 230: Many paragraphs with only one/two sentences should be combined.

[Response]: Thanks for your advice. We have **deleted such short paragraphs** accordingly.

12) “Calibration factor” Is the same "Calibration factor" applied to VOD maps for all years? Seems to be a very risky step. 1. bench mark AGB is not actually from 2003, but actually vary among all years 1990s to 2020. 2. The calibration factor could change among years, depending not only on tree growth difference, but also land cover type changes. Will be more reasonable to use the corresponding year (or every 5years) of true AGB to find the calibration factor for that year of VOD AGB map?

[Response]: Following this comment, we **have got rid of all the ‘calibration factors’ in this study**. In the revision, by altering the data sources, the benchmark AGBC map (AGBC is 0.5 times of AGB) **now refers to a much shorter time period** (2011~2015). In addition, we devised a **‘space for time’** method, using the long-term optical-based tree cover and non-tree vegetation cover, as well as a long-term microwave remote sensing VOD dataset to **directly retrieve the interannual variation of AGBC**. Please see section 2.1 and 2.2 in the revised manuscript for more details.

13) Line 274-275 "AGB decomposition generally followed" The description is very unclear, I suggest author to re-write it and use equations or conceptual figures to explain. The usage grid and pixels are very confusing. Suggesting using AGB-1km grid and LC-100m pixels if this is the correct understanding. "50 pixels": use % instead, not sure how much is the 50 pixels taken.

[Response]: Considering the high complexity and potential uncertainties in the linear regression-based forest and shrubland AGB decomposition, following your valuable comments, **we did not apply this method in our revision**. Instead, we **only calculated the annual belowground biomass carbon (BGBC) per area forestland in grids that were dominated by forestland** (forestland fractions were consistently over 50%). Finally, for grids with forests but are not dominated by forestlands, we sequentially searched for at least five valid RSR values (the ratio of forests' BGBC to AGBC) nearby (Chen et al., 2019) and then multiplied the annual forest AGBC in the grid with the median of nearby RSR values in each year to estimate the annual forest BGBC (Lines 212~228 in the revised manuscript). This approach was **much simpler than the original one, and is also logically reasonable**.

14) Line 280: "05, or R^2 was below 0": REALLY?? do you mean R?

[Response]: Sorry for the unclear information. Actually, **for a linear regression without constant, the computed R^2 can be negative.** But under this situation, **the R^2 is not valid at all**. Accordingly, in the revised manuscript, these sentences have been deleted, and we **did not perform linear regressions without constant any more**.

15) Line 285: Unclear. "situation. Specifically, for 1/12° grids with less than 50 pixels with forests, but the pixels with shrubland are sufficient, we can reliably estimate the AGB per area shrubland as the ratio of grid average AGB to the mean shrubland area percentage in the grid." Do you mean assuming those 1/12 grids as all shrubland grids?

[Response]: Yes, **in the original manuscript, in this situation we assumed those 1/12 grids as all shrubland grids**. However, upon careful inspection, we agree that because the forests' AGB per area can be much higher than that of shrublands, forests' AGB should not be ignored even in shrubland-dominated grids. Hence, in the revised manuscript, we only retrieve forests' aboveground and belowground biomass carbon stock following the method described in the response to your 13^{th} comment, and **abandoned the estimation of shrublands' biomass carbon**. We also changed the title of this manuscript accordingly.

6) Line 300: Another 'calibration factor': Do you mean the ratio of AGB map in 2017 between pre- and post-decomposition? Anything to do with 2003-2020 years? I am assuming the ratio of forest and shrubland area could change during 2003-2020. Are you using the same ratio from 2017 over the entire period?

[Response]: Thanks for your careful reading and accurate understanding. We agree that too many calibration factors that are not rigorously calculated will introduce errors to the final results. So, **we have changed the method**, and now there are **no such 'calibration factors' remaining**. Please see our response to your 13^{th} comment for details.

17) Figure 4: Mean annual woody biomass is a confusing title, sounds like annually stocked woody biomass. Do you mean "averaged woody biomass in 2003-2020"? please change accordingly. same

for c.

[Response]: We have **revised the figure title according** (Figure 6 in the revised manuscript). Please note that the new dataset now covers 2002~2021 (the past 20 years).

18) Line 490: what is "vegetation continuous fields"?

[Response]: Here, we refers to the MODIS vegetation continuous fields (VCF) data (MOD44B v061) which includes **three ground cover components: percent tree cover, percent non-tree cover, and percent non-vegetated** (Dimiceli et al., 2022). To make it clear, we **added the information** in Lines 150~152.

19) Line 490: what this the data that you compared? if not VOD, what it is? Not clear about the logic of this sentence.

[Response]: Sorry for the confusing sentences. In this study, we **compared our dataset with some existing global long-term continuous forest biomass or biomass carbon stock datasets**, including the well-received global long-term terrestrial biomass data between 1993–2012, which was developed mainly based on a long-term integrated VOD dataset (Liu et al., 2015), as well as an updated woody biomass dataset covering 2001–2019 whose long time series was derived from optical remote sensing data (i.e., MODIS VCF dataset) (Xu et al., 2021) (Lines 230~235 in the revised manuscript). Please also note that following the comments from another reviewer, **we have deleted these sentences**, which discussed the possible influence of soil moisture variation on VOD.

20) Line 505: if the absolute value is incorrect, the spatial and temporal variation will be impacted too. Not a reasonable assumption.

[Response]: By changing the data sources, **this problem or assumption no longer exists in the revised manuscript**. Now, the accuracy of absolute value can also be generally guaranteed as well. Please see our response to your 5th comment for more details.

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We appreciate the time and efforts of the editor and referees in reviewing this manuscript and the valuable suggestions offered. In addressing all issues indicated in the review report we trust that the revised version meets the Reviewers' comments and the journal's publication requirements.

[Reviewer 3] General Comment:

This study produced a long-term (2003-2020) above- and belowground woody biomass at 1-km resolution in China. Authors did a huge and impressive work to integrate multiple remote sensing data, to evaluate the spatial pattern of AGB, BGB, RSR and to investigate their dynamics. This paper provides a useful dataset for researchers to study the ecological changes in China. But there are still some comments needs to address/answer.

[Response]: Thank you for your positive comments to our work. We have addressed the comments you raised.

[Reviewer 3] Main Comments:

1) Line 159. Why you averaged the three sets of simulations rather than put the three LIDAR-based timber volume into one random forest model? You may consider the high relevance of these LIDAR-based timber volume, but why you put three biomass datasets (GlobBiomass, CCI Biomass and GLASS-Biomass v2) into one random forest model? They are also highly correlated with each other.

[Response]: Thanks for this remind. In our revision, following the comment of another reviewer, we **directly adopted a new high resolution (30 m) forest canopy height map** for China, which was developed by interpolating the ICESat-2 and GEDI data in 2019 through a neural network (Liu et al., 2022), instead of using three LiDAR-based timber volume at the same time. We also agree with you that the three biomass datasets utilized in our original study, especially GlobBiomass and CCI Biomass which were developed using similar methods, were highly correlated with each. So, following this comment, **we changed the candidate indicators of the spatial pattern** of forests' aboveground biomass carbon (AGBC) in China. Now, there are four candidates: 1) GlobBiomass 2010 which was created by combining multiple satellite observations of SAR backscatter, including the L-band ALOS PALSAR and C-band Envisat ASAR around the year 2010; 2) a high-resolution forest AGB for China which was produced by relating the ICESat GLAS (LiDAR)-derived footprint AGB to various variables derived from Landsat optical images (Huang et al., 2019); 3) the forest canopy height map; and 4) the tree cover map derived from composite Landsat data around 2010 (Hansen et al., 2013). Because we found that GlobBiomass has the highest correlation with the grid-average forest biomass computed from in-situ plot measurements (see Lines 127~130 in the revised manuscript), **we finally chose GlobBiomass 2010, and calibrated it according to the regression relationship between this dataset and the true values**. In this way, we have avoided the problems you raised. Please refer to section 2.1 in the revised manuscript for more details.

2) Line 217 'Accordingly, by adding this bias to the VODCA VOD data after 2013, we improved its temporal continuity', why you added the bias to compute AMSR2-VOD and to improve VODCA's temporal continuity? did you assume there is no vegetation change between two sensors (about 10 years)? I deeply doubt this.

[Response]: VODCA dataset was developed by combining the VOD retrieved from both AMSR-E and AMSR-2, so the bias between the AMSR2-based VOD data (2012~2021) and AMSR-E-based VOD data (2003~2011) will contribute to some temporal inconsistency within the VODCA dataset. In the original study, we estimated this bias by referring to other temporally-continuous vegetation cover dataset, including LAI. We **never assume there is no vegetation change** between the two sensors. However, we agree that the calibration of VODCA dataset in this way was still quite rough. Therefore, in the revision, following the recommendation of another reviewer, **we abandoned the use of VODCA dataset.** Instead, we chose the 'land parameter data record (LPDR)' product, which was generated by using **similar calibrated** brightness temperature retrieved from these two sensors, and thus the bias between two periods will be smaller (Du et al., 2017). Therefore, we did not have to further improve the temporal continuity of VOD by ourselves.

3) Line 410-412 AGB density in ENF > AGB density in EBF? It is opposite to our general knowledge; can you explain it?

[Response]: The ENF in China are mainly located in southwest China (see Figure S3 in the revised Supplementary Information), where the forests are **natural or semi-natural and relatively mature**, so the AGB density can be higher than the young plantation forests in southern and southeastern China (Yu et al., 2020; Zhang et al., 2017). Massive field inventory across China also distinguished that the average AGB density in ENF is the highest among different forest types in China (Tang et al., 2018). We have **added the explanation** in Lines 299~302 in the revised manuscript.

4) Figure S5. For the decomposed AGB density in shrubland (b), high values could reach 237 t/ha, is it too high for shrubland?

[Response]: Thanks for your careful reading. We admit that the uncertainties within the regression-based AGB decomposition procedure in the original manuscript has introduced some extra errors, including the extremely high AGB density value for shrublands. Therefore, to avoid these problems, in the revision, we **abandoned the AGB decomposition between forestlands and shrublands.** Now, **we only calculated the annual belowground biomass carbon (BGBC) per area forestland by referring to the AGBC in grids that were dominated by forestland** (forestland fractions were consistently over 50%). Please refer to Lines 212~228 in the revised manuscript for more details. We also changed the title of this manuscript to '1 km-resolution maps reveal increases in above- and belowground forest biomass carbon pool in China over the past 20 years' accordingly.

5) Line 300 'the sum of the decomposed AGBs in 2017 was obviously different from the pre-decomposed AGB in other years'. If they were obviously different, how do you prove the robustness of your method?

[Response]: The decomposed AGBs in 2017 was certainly different from the AGB in **other years** (i.e., **2003~2016**). On account of the uncertainties within the AGB decomposition process, we have **abandoned this method**. Instead, we **only calculated the annual belowground biomass carbon (BGBC) per area forestland in grids that were dominated by forestland**. Finally, for grids with forests but are not dominated by forestlands, we sequentially searched for at least five valid RSR values (the ratio of forests' BGBC to AGBC) nearby (Chen et al., 2019) and then multiplied the annual forest AGBC in the grid with the median of nearby RSR values in each year to estimate the annual forest BGBC (Lines 212~228 in the revised manuscript). This approach was **much simpler**

than the original one, with fewer uncertainties and is logically reasonable.

6) ‘High-resolution…’ it’s hard to say high resolution for 1 km, you can directly say 1-km resolution in title.

[Response]: We have revised the title as ‘**1 km-resolution** maps reveal increases in above- and belowground forest biomass carbon pool in China over the past 20 years’ accordingly.

7) Please improve English in the full text.

[Response]: We have rewritten the manuscript, **simplified it and improved the English** accordingly. We have also asked a native English writer to help us improve the English writing.

[Reviewer 3] Specific Comments:

1) a) ‘AMSR-2’ should be ‘AMSR2’, b) please find the rectangle written by ‘MODIS VCF (tree cover)’, the arrow near this rectangle is unclear. c) please find 3 rectangle written by ‘Calibrated SMAP / VODCA/ AMSR2…’ The data were processed by denoising and filtering, but without calibrating, so it is inappropriate to say Calibrated…

[Response]: Thanks for your careful inspection. We have simplified the method and redrawn the technical workflow as Figure 1 in the revised manuscript. **These problems have been avoided**.

2) Line 86-87, ‘a slightly different algorithm’, please clarify it.

[Response]: We have **abandoned the use of CCI-Biomass dataset** following your 1st main comment, since the methods to obtain the GlobBiomass and CCI-Biomass datasets **are actually very approximate**, which will lead to high auto-correlation.

3) Line 95-98, it’s different to understand the causality in this sentence, please make it clearer.

[Response] Sorry for the confusing sentences. In the original manuscript, this sentence explained why we chose the overlapping areas of these three biomass datasets. However, in the revision, we obtained the benchmark AGB carbon stock map by just calibrating the GlobBiomass AGB map, so the study area was easier to determine, and thus **this sentence has been deleted**. We also **revised other such confusing sentences** during the revision.

4) Line 104: ‘ATLAS has three strong and three weak beams.’ But in line 107, you said ‘If the number of strong beam records exceeded 5, then…’ my question is ‘3 strong beams how to exceed 5?’

[Response]: It is because in the original study, All ATLAS records acquired **during 2018~2020** were incorporated, not just the observation at a single time point. In the revision, we adopted the ATLAS and GEDI-based high resolution forest canopy height map for China (Liu et al., 2022), and **did not process the ATLAS data by ourselves** any more. So, **these confusing sentences were deleted**.

5) Line 184: ‘filtering out the high-frequency fluctuations’, you mean ‘smooth’? how did you filter? ‘other C-band VOD’ you mean ‘LPRM C-VOD’?

[Response]: Sorry for these unclear sentences. In the original study, the high-frequency fluctuations were smoothed using Harmonic Analysis of Time Series (HANTS) filtering, and 'other C-band VOD' refers to the AMSR2 LPRM VOD. In the **revised manuscript**, we adopted the **a higher quality** global long-term **VOD dataset** called the 'land parameter data record (LPDR)', which was generated by using similar calibrated, X-band brightness temperature retrieved from both AMSR-E and AMSR2 (Du et al., 2017). Because the reference plot investigations were conducted in summers (Tang et al., 2018), we **directly averaged the VOD** data from mid-July (the 206th day) till the end of September (the 274th day) in each year to represent the annual AGB status, **without smoothing** the VOD time series.

6) Line 194 'Second, we virtually filled in the data in 2015 and 2021 by using those in 2016 and 2020" why did you filled 2015 by values in 2016?

[Response]: We agree that although this step may slightly improve the smoothing efficiency, it will introduce uncertainties as well. In the revision, we **did not perform the smoothing any more, and thus the virtual data filling was also avoided**. Thanks for your reminding.

7) Line 197 '…after setting the thresholds of minimum distance between two peaks, peak height and dominance of peaks to reasonable values', how did you set the thresholds of minimum distance between two peaks, peak height and dominance of peaks to reasonable values, and please clarify the thresholds of minimum distance between two peaks, peak height and dominance of peaks to reasonable values

[Response]: We admit the determination of some parameters, especially the peak numbers during HANTS filtering was somewhat arbitrary. In the revision, we **abandoned the unnecessary filtering (smoothing) step**, and adopted another much simpler but still reasonable method to process VOD data. Please see our response to your 5th specific comment for details.

8) Line 201-203 not good English, to be written

[Response]: **This sentence has been removed** since HANTS filtering was no longer used. We have rewritten the manuscript and paid more attention to English writing.

9) Line 204 please clarify which year/years AMSR2-based VODCA's VOD came from?

[Response]: AMSR2-based VODCA VOD is between July 2012 and 2018. In the revision, we have abandoned the use of VODCA VOD.

10) Line 234 how did you resampled VODCA C-VOD from 0.25° to 1/12° (=0.083°)?

[Response]: In the original manuscript, we resampled VODCA VOD using nearest neighbor method. We admit that this simple downscaling method will introduce some uncertainties. Therefore, in the revised manuscript, we only used the LPDR VOD product at 0.25° resolution, and **have avoided the data resampling from coarser resolution to finer resolution in the whole study**.

11) Line 227 'AMSR2 VOD' please clarify which band, and correct in the full text

[Response]: In the original study, we used the **X-band** AMSR2 VOD data. In the revised manuscript, We selected the global land parameter data record (LPDR) v3 VOD product, which was generated using similar calibrated, **X-band** brightness temperature retrieved from both AMSR-E and AMSR2 (Du et al., 2017).

12) Line 361-364 R^2=0.36 was produced by RF when GEDI-derived or GLAS-derived wood volumes involved? And please clarify R^2 values when you used ATLAS derived tree volume.

[Response]: In the original study, when **ATLAS** derived tree volume was applied as the predictor, the training R^2 was **0.49** (Line 336 in the original manuscript). When GEDI-derived wood volume or GLAS-derived wood volume is involved, the training R^2 **was the same**, 0.36. In the revision, **we abandoned the random forest method**. To obtain a benchmark forest aboveground biomass carbon (AGBC) map for China, we directly calibrated the GlobBiomass 2010 map by referring to the grid-scale AGBC computed from massive plot measurements. It is because we found that GlobBiomass 2010 AGB matches the best with the grid-scale forest AGBC, with a correlation coefficient (CC) of 0.50, followed by tree cover (CC=0.42), the product of canopy height and tree cover (CC=0.38), and finally the canopy height (0.27) and Huang et al.'s AGB map (0.25).

13) Line 429 'pre-area AGB' could be modified to 'AGB density'

[Response]: Thanks for reminding. We have **corrected this phrase** accordingly (Line 313 in the revised manuscript).

14) 'ENF's per-area AGB' not good English, to be written

[Response]: We have **removed these badly written phrases** accordingly.

15) Line 460 'those improved AGB maps' mean AGB in (Su et al., 2016; Huang et al., 2019)? If yes, they are improved than which product? Do they have higher accuracy?

[Response]: Upon careful checks, we agree that there are currently no solid evidences that can prove the higher accuracy of these two AGB maps than other products. Therefore, following this comment, we have **deleted the inter-comparisons and the related sentences** in the revised manuscript.

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We appreciate the time and efforts of the editor and referees in reviewing this manuscript and the valuable suggestions offered. In addressing all issues indicated in the review report we trust that the revised version meets the Reviewers' comments and the journal's publication requirements.

[Reviewer 4] General Comment:

This manuscript describes the development of a new high-resolution biomass (above-ground AGB and below-ground BGB) map based on remote sensing data from several optical/microwaves/lidar data sets and in situ data sets and then analyses the time changes of AGB and BGB over 2003-2020. I found the manuscript interesting and well written and I recommend its publication after considering the following comments.

[Response]: Thank you for your positive comments to our work. We have addressed all the nice recommendations and comments you raised.

[Reviewer 4] Main Comments:

1) Line 82: I'm surprised by the selection of the three data sets: GlobBiomass, CCI-Biomass and GLASS-Biomass v2. For instance, GlobBiomass and CCI are developed from the same group (using radar) and it could be interesting to use data sets based on other remote sensing data (as the Saatchi data set for instance).

[Response]: We agree with you that GlobBiomass and CCI were developed by the same group, and their methods were also similar. Following this comment, **we changed the candidate indicators of the spatial pattern of forests' aboveground biomass carbon (AGBC) in China**. Now, there are four candidate indicators: 1) GlobBiomass 2010 which was created by combining multiple satellite observations of **SAR backscatter**, including the L-band ALOS PALSAR and C-band Envisat ASAR around the year 2010 (Santoro et al., 2021); 2) a 30 m resolution forest AGB for China which was produced by relating the ICESat GLAS (**LiDAR**)-derived footprint AGB to various variables derived from the **Landsat optical** images (Huang et al., 2019); 3) the forest **canopy height map** developed by interpolating the **ICESat-2 and GEDI data** through a neural network (Liu et al., 2022); and 4) the **tree cover map** derived from **composite Landsat data** around 2010 (Hansen et al., 2013).

2) Line 119: the authors used the "Global Forest Canopy Height 2019 dataset". Recently released GEDI data sets could have a higher accuracy.

[Response]: We agree with you that the recently released GEDI data could have a higher accuracy. In the revision, we adopted the forest canopy height map for China which was recently developed by interpolating **both the ICESat-2 and GEDI retrievals** through a neural network approach (Liu et al., 2022).

3) Line 126: I think canopy height is an effective value: it is already an average value that implicitly accounts for tree cover. So, I'm not sure it is good to multiply by TC.

[Response]: We agree with you that the grid-average canopy height implicitly accounts for the tree cover, and is thus an effective indicator of forest AGB as well. Therefore, in the revised manuscript, we **compared the mean canopy height directly against the grid-scale forest AGBC** computed from plot measurements. However, the **correlation coefficient (CC) was 0.27, lower than the CC between the product of canopy height and tree cover (TC)** and the footprint AGBC data. This result indicates that the product of canopy height and TC is still a better indicator of forest AGB and AGBC (please note that AGBC is basically 0.5 times of AGB) compared to just canopy height.

4) Line 142: "it is supposed that the spatial pattern of woody biomass at 1 km resolution would not change much from around 2000 to 2017~2020, so the time lag problem could be ignored."? No, I think this issue cannot be ignored: it is a key assumption here. Could the authors discuss the impact of this assumption?

[Response]: We agree that land cover changes, e.g., urbanization, reforestation, can lead to changes in the spatial pattern of AGB. In the revision, this suspicious assumption was no longer used because we requested a reviewable, consistent forest aboveground carbon stock (AGBC) inventory dataset for China between 2011 and 2015 (Tang et al., 2018) from the authors. Because in this inventory dataset, all **AGBC measurements were conducted in the same way over a short time period that is close to the nominal year of GlobBiomass 2010 dataset**, we can now directly regress GlobBiomass 2010 against the grid-average forest AGBC derived from these plot measurements, and then used the regression relationship to obtain an improved benchmark AGBC map in China between 2011 and 2015. Please see section 2.1 in the revised manuscript for more details.

5) Line 156: "The RF model trainings were conducted in MATLAB R2021a software". Which RF training do you mean here?

[Response]: In the original manuscript, both the benchmark AGB map and the belowground biomass (BGB) were simulated using random forest (RF) models that were trained in MATLAB. Currently, the benchmark AGB map was developed by calibrating GlobBiomass 2010 dataset, and thus **only the forests' BGB carbon stock (BGBC) was mapped using RF models** trained in MATLAB R2021a software. This information was made clear in Lines 210~211 in the revised manuscript.

6) There are many steps of calibration of many models in this study. To make reading easier, the authors should separate more clearly each steps in different subsection, indicating: what is the model? what is the input data? what is the predicted data? and what is the data used for calibration? For instance, 1st step correspond to the development of the high-res AGB for 2003.

[Response]: We agree that there are too many calibration steps in the original manuscript, which not only lead to confusion but can also introduce additional uncertainties. Following this comment, we **have largely revised the methods**, and **got rid of all the 'calibration factors' in this study**. For example, after changing the data sources, the benchmark AGBC map now refers to a much shorter time period (2011~2015). In addition, we devised a 'space for time' method, using the long-term optical-based tree cover and non-tree vegetation cover, as well as a long-term microwave remote sensing VOD dataset to **directly retrieve the inter-annual variation of AGBC**, **so no calibration factor is needed in this part now**. Please refer to the methods in the revised manuscript for details.

7) Line 160 "averaging that is weighted by the mean R2", could you give a reference for this weighting?

[Response]: Sorry for the arbitrary data averaging step. In the revised manuscript, we **have deleted**

this part, because now we directly adopted a new ICESat-2 and GEDI-based forest canopy height map for China (Liu et al., 2022) instead of using three different forest canopy height maps.

8) Line 171: There are well-known issues in using VODCA VOD to monitor biomass over long term period (these issues are discussed here, requiring calibration corrections). So why using VODCA. For instance, the LPDR data set from the Montana University was found better for monitoring biomass. Cf Li et al., 2021, https://10.1016/j.rse.2020.112208, 2021.

[Response]: Thank you for the recommendation. We **substituted the VODCA VOD by the LPDR dataset** in the revision accordingly.

9) Line 191, I'm surprised the authors used the DCA VOD product. Li et al., 2022 found strong saturation effects of DCA VOD at high biomass level. I would strongly recommend using MTDCA or SMAP-IB (INRAE Bordeaux). CF Fig. 9 in https://doi.org/10.1016/j.rse.2022.112921 (SMAP-IB)

[Response]: Thank you for this nice recommendation. **We have downloaded and checked the MTDCA and SMAP-IB datasets**, and have recognized their high quality. However, because we have changed the data making algorithms, and **now we don't have calibrate the spatial pattern** of the long-term VOD dataset (i.e., LPDR VOD), we **abandoned the use of SMAP VOD** (DCA, MTDCA, or SMAP-IB) maps in the revision.

10) Line 195 "Moreover, we also determined the average annual number of VOD peaks for each grid after setting the thresholds of minimum distance between two peaks, peak height and dominance of peaks to reasonable values… nearby valid values (Chen et al., 2019b)". What you mean here is not clear at all to me: why you do you estimate those peaks?

[Response]: In the original study, we estimated the number of peaks to facilitate the parameterization of HANTS filtering (i.e., estimation of the parameter: number of frequencies to be considered above zero frequency in the Fourier function). However, upon careful consideration, we admit that this step was somewhat arbitrary and suspicious. Therefore, in the revision, we **abandoned the unnecessary filtering step and the related peak estimation** accordingly. Instead, we adopted a much simpler but still reasonable method to process VOD data. In detail, because the reference plot investigations were conducted in summers (Tang et al., 2018), we **directly averaged the LPDR VOD** data from mid-July (the 206^{th} day) till the end of September (the 274^{th} day) **in each year** to represent the annual forest AGB (or AGBC) status.

11) Line 245: how can you assume this calibration parameter is constant in time?

[Response]: We agree that this calibration parameter may not be a constant in time. In the revision, **we have got rid of all the calibration factors**. For example, **this suspicious calibration factor was removed** since we have **abandoned the use of random forest approach in simulating the benchmark AGBC map**. Instead, we directly calibrated the GlobBiomass 2010 dataset by referring to the high-quality plot AGBC measurements across China during 2011~2015, because this dataset has been proven to be the best candidate of benchmark AGBC map in China.

12) Line 358: "The training efficiency is limited by the…" It would be much better to use the observations used to make these maps, rather that the AGB product as input to the RF. Could the authors please comment on this issue? (this is a big change so I do not require here for this reason)

[Response]: Upon further consideration, we agree with you that RF model is not very suitable to the benchmark AGB mapping in China. Following your valuable comments, now we directly chose the GlobBiomass dataset which is developed based on **multiple satellite SAR observations**, including the L-band ALOS PALSAR and C-band Envisat ASAR around the year 2010 (Santoro et al., 2021), and then calibrated against the **in-situ AGBC observations** at large quantities of forest plots across China **during a similar time period**.

13) Line 558: recent high-quality China's forest AGB: Why not using these high quality maps as input of the RF model instead of CCI, Globbiomass, etc?

[Response]: Thanks for the careful inspection. Following your suggestion, in the revised manuscript, we **have incorporated a high-resolution (30m) forest AGB map for China (Huang et al., 2019) as a candidate indicator** of the benchmark AGB map. However, the comparison against in-situ measurements suggested that the spatial pattern accuracy of this AGB map is much lower than that of GlobBiomass (see Lines 127~130 in the revised manuscript). The spatial resolution of another forest AGB map for China is only 1 km (Su et al., 2016), much coarser than the scale of forest plots (Tang et al., 2018). In addition, previous studies have shown that the overall quality of Su et al.'s AGB map is still lower than Huang et al.'s AGB map (Chang et al., 2021). Therefore, in the revised manuscript, we finally selected GlobBiomass as the basis of our benchmark AGB map for China.

14) Line 490: "A recent study revealed that the variation in VODs is correlated with not only biomass, but also soil moisture availability (Konings et al., 2021)." To my opinion, this is not a "revelation". This is just ONE study based ONE particular methodology and some SPECIFIC data sets. Other studies based on different assumptions and different data sets would have found very different results. From what I understood, the main issue is that an AGB data set affected by saturation issues was used to analyse VOD changes, which are not affected by saturation effects…So, to my opinion, the paper by Konings et al. cannot be used as a reference to analyze the effects of soil moisture on biomass changes (identified by L-VOD) and it is a very unsatisfying idea to use here instantaneous SM as input of RF to model AGB. SM can have an effect on Biomass but it is generally a long term (delayed) effect…

[Response]: Thank you for this remind. Upon deeper consideration, we agree with you that the variation in VODs is mainly determined by vegetation biomass change, and the incorporation of soil moisture is actually not reasonable. Therefore, **we abandoned the use of soil moisture data in the revision, and have deleted these misleading discussions** accordingly.

[Reviewer 4] Minor Comments:

1) Line 132: consider revising English here

[Response]: This sentence has been deleted. We have revised the English in other parts of the manuscript.

2) Line 144, why using 'however" here?

[Response]: Sorry for the wrong use of contrastive connective here. We have paid attention to the use of 'however' in other parts of the revised manuscript.

3) Line 146: "observe only the canopy", you mean "forest" here?

[Response] Yes, here we mean the 'forest canopy'. We have avoided these confusing expressions in the revised manuscript.

4) Line 170: first retrieval of VOD for biomass monitoring were shown in Wigneron et al. 1993-1995. https://doi.org/10.1016/0034-4257(94)00081-W

[Response] Thanks for the recommendation. We have added this reference accordingly (Line 174 in the revised manuscript).

5) Line 175, the issues in merging AMSRE/AMSR2 VOD were discussed and solved in Wang et al. at X-band, https://doi.org/10.1016/j.rse.2021.112556 https://doi.org/10.1016/j.jag.2021.102609? For information, Wang et al. are developing a C-band product based on the same principle

[Response] Thanks for recommending this valuable information. We have added this part to the Discussion, following: "In addition, an inter-calibration between the AMSR-E-based VOD and the AMSR2-based VOD will further reduce the potential bias within the long-term integrated VOD datasets (Wang et al., 2021a; Wang et al., 2021b)" (Lines 392~395 in the revised manuscript). We are also looking forward to seeing the new C-band product developed by Wang et al. soon.

6) Line 201, What is the HANTS filtering: why do you use it?

[Response]: HANTS (Harmonic Analysis of Time Series) filtering is a method that can smooth the time series with periodicity (e.g., NDVI) (Menenti et al., 1993). In the original study, we applied this method to remove some potential noises in the VOD data. But now, we directly averaged the LPDR VOD data from mid-July (the 206th day) till the end of September (the 274th day) in each year to represent the annual forest AGB (or AGBC) status, without doing the HANTS filtering in response to your 5th major comment.

7) Line 206: non-tree

[Response]: 'Non-tree vegetation' refers to short vegetation, which includes shrubs and herbaceous plants. We added the explanation in the revised manuscript, following: "By adopting the MODIS vegetation continuous fields (VCF) data (MOD44B v061) which includes three ground cover components: percent tree cover, percent non-tree vegetation (i.e., short vegetation) cover, and percent non-vegetated (Dimiceli et al., 2022), we first calculated the mean tree cover (hereinafter, TC_{mean}) and short vegetation cover (hereinafter SVC_{mean}) during 2011–2015…"

8) Line 220: Cf my above comments, there are really key issues in data continuity in VODCA. Why using it?

[Response]: We have **substituted the VODCA VOD by the LPDR dataset** following your 8th major comment.

9) Line 235: why using both mean and median: they are so correlated …

[Response]: In the revised manuscript, we **only adopted the mean value** of VODs in summers accordingly.

10) Line 240: 80 000 pixels x 10 km x 10 km would do 8 M km2 which is too much for China…

[Response]: Sorry for the wrong information. Since we have abandoned the use of random forest model in benchmark AGB mapping, **this probably wrong sentence has been deleted**.

11) Line 246: you mean here a 1/12 or 120° grid cell.

[Response]: In the original manuscript, in this line we mean a 1/12° grid cell. We have revised the manuscript accordingly. **Now, the spatial resolution of all grid cells are the same, 1/120°**.

12) Line 246-247, you assume height is constant and so biomass is constant… why not assuming directly biomass is constant?

[Response]: In this sentence, we just want to explain why we can assume that the AGB per tree cover can be assumed as a constant in a small area. The reason is the tree canopy heights are often approximate within a small area.

13) Line 250, you mean forced to zero?

[Response]: Yes, in the original manuscript, we sometimes forced the constant to zero. However, we recognized that under this situation, **the R^2 of the regression is not valid**. Accordingly, in the revised manuscript, we **did not perform linear regressions without constant any more**.

14) Line 255: "was considered invalid. » How many pixels are invalid? (number and %)?

[Response]: Thanks for reminding. We have **added the information as Figure 3b** in the revised manuscript. Please also note that the detailed methods have been changed as well.

15) Line 467: "yet the two existing long-term datasets predicted 1.26~1.52 Pg. Moreover". Which "existing long-term datasets" do you mean here?

[Response]: Here, the "two existing long-term datasets" refers to the well-received global long-term terrestrial biomass data between 1993–2012, which was developed mainly based on a long-term integrated VOD dataset (Liu et al., 2015), as well as an updated woody biomass dataset covering 2001–2019 whose long time series was derived from optical remote sensing data (i.e., MODIS VCF dataset) (Xu et al., 2021). (Lines 230~235 in the revised manuscript). We also added the information in the Discussion, following: "Compared to this study, the two existing datasets **(i.e., Liu et al. (2015) and Xu et al. (2021)'s datasets)** predicted…" (Lines 349~350 in the revised manuscript).

16) Line 470: define clearly Southern China (province, ha)?

[Response]: In the original manuscript, Southern China refers to Sichuan, Yunnan, Guangxi, Guangdong, Hunan, Guizhou, Chongqing and Hubei provinces, following a published article (Tong et al., 2020). In the revised manuscript, we did not do such too detailed comparison, and thus the word 'Southern China' has been removed.

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