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.

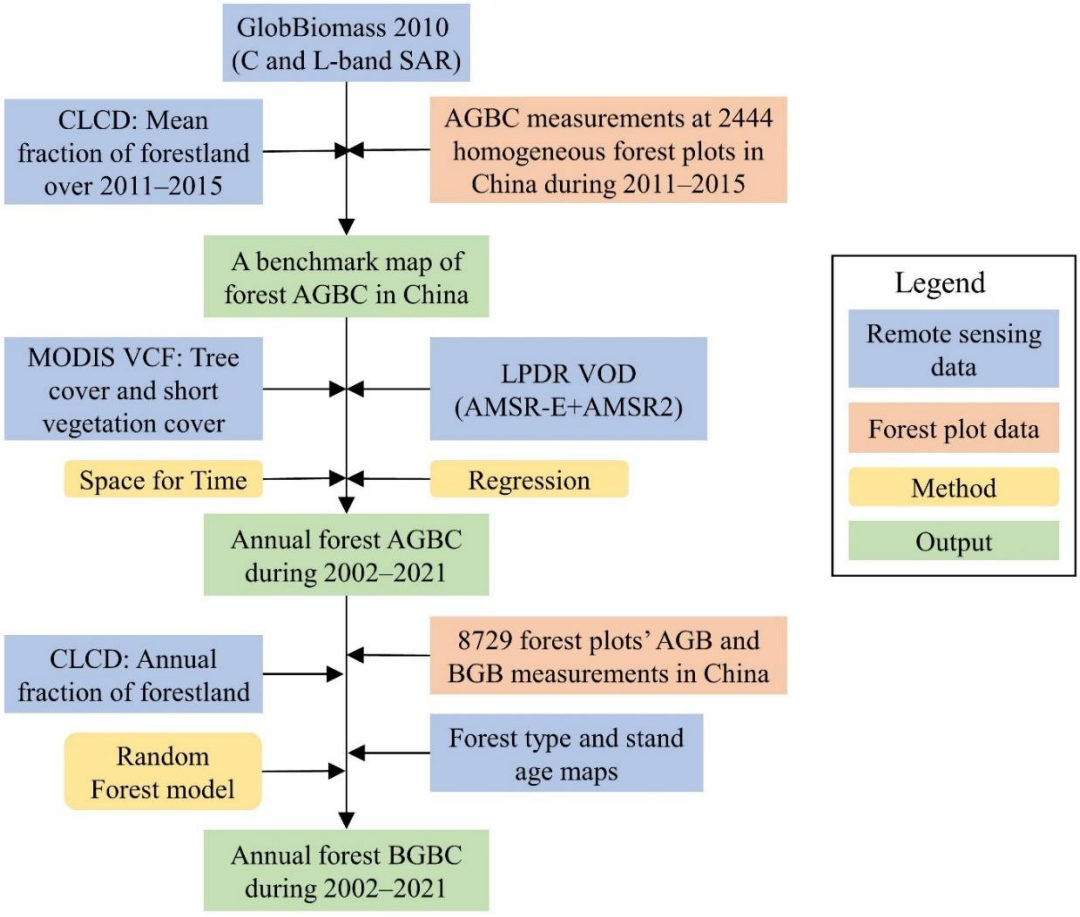

Figure R1. Workflow of forest biomass carbon pool monitoring in China during 2002–2021. AGBC, BGBC: Aboveground and belowground biomass carbon; VCF: Vegetation Continuous Fields; LPDR VOD: global land parameter data record- Vegetation Optical Depth; CLCD: China Land Cover Dataset.

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 13th 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 13th 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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