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 206th day) till the end of September (the 274th 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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