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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