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:

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).

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

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.

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?

MAJOR COMMENT

Line 156: "The RF model trainings were conducted in MATLAB R2021a software".

Which RF training do you mean here?

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.

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

MAJOR COMMENT

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.

MAJOR COMMENT

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)

-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?

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

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),

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 ?

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…

Minor:

-line 132 : consider revising English here

-line 144, why using 'however" here?

-line 146: "observe only the canopy", you mean "forest" here?

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

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

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

Line 206: non-tree

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

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

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

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

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

Line 250, you mean forced to zero?

-line 255: "was considered invalid. » How many pixels are invalid
 (number and %)?
-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 ??
-line 470: define clearly Southern China (province , ha)?

JP Wigneron, INRAE-Bordeaux