Dear Referee,

We would like to thank you for your constructive comments which will contribute to improving the manuscript.

Major comments:

- First, the authors need to test if the posterior distributions are very dependent on the prior distributions. If yes, this suggested that the optimization probably has not really worked.

We can confirm that in our approach the prior distributions have a stronger influence on the posterior distributions. This should not necessarily be seen as evidence that the optimization did not work. Prior distributions should take all our knowledge into consideration and even if posterior distributions are dependent on the prior distribution it does not mean that the optimisation has not worked but just that the observations brought little constraining information to the inverse problem. We propose to address this concern by running a test in which the central value of the prior distribution is changed. We expect that changing the central value will result in different posterior distributions for some land cover classes whereas it will not affect the result for other land cover classes. Although the dependency is already discussed between Ln458 and Ln480 of the manuscript, the revised manuscript will further elaborate on this issue by mentioning the referees viewpoint, the results of the additional test, and our interpretation of this dependency.

- The land cover types associated with trees and shrubs, i.e., 100, 110, 120 have very low posterior wood fractions (0.14, 0.08, and 0.14). This is not feasible, for the tree- and shrub- LC type, the wood fractions should be larger than herbaceous and bare soil fractions. Could you please provide the posterior woody and herbaceous biomass?

In the land cover type classification, land cover types 100, 110 and 120 represent very different ecosystems ranging from desert vegetation to closed thicket. As shown in figure 2, the biomass distribution of these three land cover classes is mainly determined by
desert vegetation with a biomass of around 1 t/ha rather than a thicket with a biomass around 80 t/ha. In ORCHIDEE, we don't have a shrub PFT yet, so we have to choose whether we represent shrubs as a herbaceous PFT or as a closed forest PFT. For example, if we choose to represent shrub as a closed forest PFT, like in the original CWT, we risk to represent shrublands as a very small fraction of dense evergreen forest rather than a large fraction of sparse woodland (as it should be). The biomass of the forest will depend on the climate and the soil. This issue is discussed between Ln526 and Ln539 but given that we consider this issue with trees and shrubs, 100, 110, 120 as one of our major findings, the referee comment made us realize we should better develop this section in a revised version of the manuscript.

- Second, I am wondering if the uncertainty of the AGB reference map would have large effects on posterior distribution? This is very important because different AGB products in some tropical regions have large differences.

We agree with this insight and we therefore suggested it as future work (section 4.4, Ln576 and 585). Given that the current manuscript already contains a lot of material, we did not include this analysis for the moment. In equation 8 the uniform distribution for \( \sigma_{b,lc}(0, 200) \) plays that role since for a given pure pixel biomass measurement (Bpp), the standard deviation ranges from 0 to 200 t/ha. This accounts for the uncertainty of the biomass map we used. Future work should search for feasible approaches to account for uncertainties resulting from the fact that there are several observational-based biomass estimates available. This work would therefore have to deal with both uncertainties and biases. We clarify this in section 4.4 around Ln576 and 585.

Minor comments:

- P2, Ln52: The first and second source of uncertainty looks the same, just interpreted in different ways.

We wrote “Current remote sensing technology does not enable distinguishing individual tree species, hence, vegetation is observed as land cover types [Defourny, P., 2019] which group vegetation with similar sensory characteristics. Remote sensing observations as well as classifying them in land cover types is a second source of uncertainties [Hansen et al. 2013, Mitchard et al. 2014, Hurtt et al. 2004].”

Rereading this sentence while keeping the referee’s comment in mind, we still think the sentence is correct but might benefit from an extra line. The first source of uncertainty is that the signal we get from RS is already mixed because its resolution is too coarse to distinguish individual trees. The second source of uncertainty comes from the model architecture: the models are using PFTs which are a much coarser classification than species. We think our statement is correct because species-level RS data would still result in the PFT classification uncertainty. On the other hand, a model that would run at the species level would still be uncertain because of the mixed-species signal of the present day RS data. Given that the elimination of one source of uncertainty does not result in eliminating the other source, made us conclude that these are two separate sources of uncertainty. We will clarify this issue in the introduction around L52.

- P3, Ln65: Cannot find this reference in the reference list.

Thanks for noticing, we will add this reference in the reference list in the revised manuscript.
- P4, Fig 1: “ABG simulated” => “AGB simulated”

We will replace ABG by AGB in the new revision of the manuscript.

- P5, Ln 145: Please clarify the downscaling method.

The AGB map was downscaled by an average resampling method, i.e., computing the weighted average of all contributing pixels. To do so, we used the Gdalwarp function from GDAL (https://gdal.org/programs/gdalwarp.html). We will add this information in section 2.2.3 around Ln145 in the revised manuscript.

- P7, Ln 170-171: Could you please show the locations of discarded and retained pixels? Are the retained pixels representative?

We can provide this information as a tiff file but in a figure formatted for an article, it would be unreadable, simply because the 1% remaining pixels are very small and will not be apparent from the figure. This is the reason why we choose to show the biomass distribution of the pixel instead (Fig. 2). We will add a sentence in section 2.2.3, around Ln150 clarifying to the reader that Fig. 2 shows the representativeness of the sampled pixels.

- P10, Ln287: The first sentence is not completed.

We overlooked this issue while checking the manuscript before submission. We will complete the sentence in the revised manuscript.

- P11, Ln306-307: The forcing data of the PFT map varied over time or not? If yes, the ESA CCI LC data starts with the year 1992. How to create the PFT maps before 1992?

Yes, the forcing data of the PFT map varied over time but our cross-walking table is only based on the 2015 ESA CCI LC data. In order to create PFT maps at other times than 2015, we use our cross-walking table especially crafted to this purpose. In the manuscript at line 116, the sentence may mislead readers about which data from ESA CCI LC is used. We will refine this sentence in the new revision of the manuscript.

- P16, Ln396: What are the numbers in parentheses? Please clarify.

They represent the standard deviation of the mean change in forest cover fraction between the refined PFT maps and the original PFT map. A standard deviation has the same units as the mean for which it is calculated, hence, we added % after the standard deviation. Following the question of the referee we realized this might be misleading as it could be interpreted as the percentage of a percentage. We will remove the % symbol and report the standard deviation without units in the revised manuscript.

Section 3.2 and 3.3: I agree that comparing the percentiles of 2.5 and 97.5, which represent the lowest and the highest values, is very important. However, the mean and median values of distribution are more important than the upper and lower bounds. I think the authors should add the comparison of mean (or median) values. Another reason for this is that generally almost modelling studies only use the mean (or median) values to do evaluation, attribution, or projection assessments.
Our initial reasoning for only showing the 2.5 and 97.5% maps was that if these maps do not differ too much, it is not so informative to show the median map because it can’t differ too much either. We agree with the referee that it is more straightforward to add a cross-walking table based on the median cover fractions and the subsequent simulation outputs in the section 3.2 and 3.3. We will do so in the revised manuscript.

- P19, Ln 437-438: I don’t really understand this. Using the PFT approach, we can set one type of forest PFT as 50%, and at the same time can set one type of grass PFT as 50%. Isn’t it the coexistence of trees and grasses?

In the ORCHIDEE PFT’s approach, splitting a pixel into 0.5/0.5 will create two independent ecosystems. By prescribing a forest PFT, ORCHIDEE will simulate a closed-canopy forest which is not at all representative of savanna trees. Also, because both PFTs are simulated independently the trees do not shade the grasses, do not reduce the throughfall for the grasses, and trees and grasses won’t compete for the same soil water nor for the same soil nutrients. Although the referee is right in saying that the PFTs coexist on the same pixel, the coexistence lacks the most basic interactions to qualify as coexistence or co-dominance as defined in the ecological literature (Sankaran et al. 2004). We will elaborate on “coexistence” in the revised manuscript around P19, Ln 437-438.