

Geosci. Model Dev. Discuss., author comment AC2  
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## Reply on RC2

Guillaume Marie et al.

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Author comment on "Constraining a land cover map with satellite-based aboveground biomass estimates over Africa" by Guillaume Marie et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-93-AC2>, 2021

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Dear Referee,

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

Major comments:

- First, I think it is ethically questionable that a group of European scientists publish an analysis on Africa's natural resources without any involvement of African scientists. My motivation for this comment is the recent debate generated by the article of Misnany et al. (2020, <https://doi.org/10.1016/j.geoderma.2020.114299>), where they describe the concept of Helicopter Research as a form of neo-colonialism. Although the situation here is slightly different than that of collecting samples for scientific analysis, I still believe that similar questions can be asked in this manuscript. Why are African scientists not involved in this manuscript given the importance of defining PFTs and biomass for their own ecosystems? In their article, Misnany et al. highlight four negative aspects of this type of studies that do not involve local scientists: 1) Ignoring land ownership and disrespecting sovereignty. 2) Having little contribution to local science and development. 3) Promoting exclusivity—potential benefits to the studied country are often neglected, and further widens the gap between developed and developing countries. 4) Creating negative sentiments in local scientists towards international research.

We noticed the ethical objections of referee 2 and suggest that the editorial board of Geoscientific Model Developments seeks advice from scientists specialized in decolonization to settle this discussion rather than through a referee-author discussion. As the current journal policy does not stipulate the requirement of a geographical representation of the authors in line with the study domain, we will limit the discussion to the technical and scientific comments made by the referee.

- Second, the use of the Bayesian approach is poorly developed. In particular, the choice of prior distributions is not consistent with formal theory for the specification of conjugate priors. For instance, the use of an uniform distribution  $U \sim (0, 200)$  for the prior distribution in equation 8 has no theoretical support; it leads to a distribution of biomass that extends to the negative side. In general, the formal Bayesian concepts for specifying hyperparameters are not used in this analysis. Therefore, I question the

theoretical validity of the results presented in this study.

We agree with the referee that a wide uniform prior distribution in equation 8 will lead to unreasonable negative biomass values. To overcome this issue, we used a truncated normal distribution for biomass ( $B_{pp}$ ) as described by  $C(0,)$  (line 7) in the OPENbugs model ([https://github.com/volarex84/R-script\\_African\\_biomass/blob/main/model\\_OPENBUGS.txt](https://github.com/volarex84/R-script_African_biomass/blob/main/model_OPENBUGS.txt)). Following the referee's comment, we realized that we forgot to update equation 8 in the text in line with the actual model used. We apologize for this oversight and will revise equation 8 to include the truncated normal distribution in the revised manuscript.

Minor comments:

- Line 21 and thereafter. You use commas to separate decimal places. This is not standard notation in the English language.

We were advised by the editor to replace all decimal points by decimal commas as it represents the standard since 2018.

- Ln 145. ABG -> AGB.

Thank you for noticing, we will replace ABG by AGB in the revised manuscript.

- Ln 204. The correct spelling is 'confidence interval'. However, notice that in Bayesian statistics the correct term to use is 'credible interval' (see [https://en.wikipedia.org/wiki/Credible\\_interval](https://en.wikipedia.org/wiki/Credible_interval)).

We will replace "confidence interval" by "credible interval" in the revised manuscript.

- Equation 5. Why do you assume a normal distribution? Biomass at the landscape level usually has a few sites with very large biomass. A distribution with a longer right tail would be more appropriate. Please provide a rationale for the selection of the gaussian distribution.

We agree with the referee that the biomass at the landscape level is not normally distributed but in the equation 5, we use the reference biomass which can be seen as the maximum potential biomass when soil, disturbance regime and climate are favorable, rather than the landscape biomass. The assumption underlying the normal distribution used in equation 5 is supported by the uncertainty around the reference biomass for a specific pixel which was derived from the CSBIO map where the uncertainty follows a normal distribution. The referee's comment made us realize that this assumption is not well explained in the manuscript. We will add it in section 2.3.2 around Ln210 in the revised manuscript.

- Equation 8. This choice of prior for the standard deviation is unreasonable. It inevitably leads to negative biomass values.

see major comment 2

- Equation 9. I also see a problem with this choice of distribution. Once you pick one random value for one of the fractions, the other values are not independent. The Beta distribution alone cannot deal with this situation. The classical way to address this problem is with Dirichlet priors (see [https://en.wikipedia.org/wiki/Dirichlet\\_distribution](https://en.wikipedia.org/wiki/Dirichlet_distribution)).

We agree with the referee, that using a multivariate beta distribution (also called Dirichlet

distribution) is conceptually better than using two univariate dependent beta distributions as is now the case. We will revise our method and rerun all relevant analyses in order to include this suggestion in the revised manuscript.

- Ln 270. Why a reference to a study in preparation? There are hundreds of papers using Orchidee, and it has been described extensively everywhere else.

Although many authors cite Krinner et al 2005 when referring to the ORCHIDEE model, this reference is no longer accurate as almost all approaches described in this paper have been refined or replaced by a different approach in the 16 years that passed since its publication. The model version that is used in this manuscript is not accurately described by any of the papers already published which justifies referring to a paper that is in preparation and that aims to describe this model version. The most accurate description of the ORCHIDEE model used in this paper can be found in Boucher et al 2020 (<https://doi.org/10.1029/2019MS002010>) but the description of the ORCHIDEE model is rather concise because that paper focuses on the entire IPSL Earth System model of which ORCHIDEE is the land surface component.

- Ln 287. Revise sentences.

We will revise the sentence in the new revision of the manuscript.

- Ln 306. Three?

We will replace "tree" by "three" in the revised manuscript.

- Ln 630. Without the African AGB map being publicly available this study would not meet reproducibility standards.

A hybrid map has been used for the analysis ([https://github.com/volarex84/R-script\\_African\\_biomass/blob/main/Hybrid\\_biomass\\_map.tif](https://github.com/volarex84/R-script_African_biomass/blob/main/Hybrid_biomass_map.tif)), details of this map are given in section 2.2.3. The analysis presented in this study can be reproduced starting from this map. The original biomass map is publicly available from: <https://www.theia-land.fr/en/product/african-biomass-map/>