

## Comments for gmd-2020-417

In short, this study intends to evaluate the suitability of the model LandscapeDNDC in capturing the aboveground biomass for different vegetation type (croplands, pure grasslands as well as tree/grass mixtures) in the Sahelian and Sudanian ecological zones of West Africa. This seems the first step for furthering assessing the impact of climate and land use change. The authors conducted extensive calibration and validation. Overall, I am enjoy reading the manuscript. I have some suggestions that might be useful for improving the paper.

1. The authors underscored the importance of the soil water availability to this special region and ecosystem, they considered 70% of the extractable soil water content, I am wondering whether it is vegetation specific since there are three distinct types here (croplands, pure grasslands as well as tree/grass mixtures)? I suggest adding a sensitivity test to illustrate its influence. Or, just set different thresholds for different vegetation (this might be doable since the simulation here was conducted at site level instead of regional level).
2. For me, the LAI at Niakhar was underestimated, and that at Wankama 1 was overestimated, NEE at Bontioli was overestimated, is it possible to have more discussion about the reason or adjust the parameters to have a better capture?
3. There is a long description about the model in 2.3.1, I suggest adding the important equations to make it easy to understand the relevant biophysical process of the model since not all the readers are familiar with the model. For example, how is the actual evaporation calculated from the potential evaporation? Also, Thornthwaite approach mainly depends on temperature, it seems water content is important in this special region with large variation of precipitation, so, how is the impact on the result? How is the performance of the modelled evaporation compared to the flux observation?
4. How is the parameters relevant to soil water content (field capacity and wilting point) at each site? Please clarify.
5. I suggest adding the vegetation distribution in Fig. 1, which is more intuitive and easier to understand.
6. The authors used the Modis LAI, which is 500-m resolution, if the grid is a mixture of different vegetation, it might have a big impact on the validation. Such discussions are needed.
7. Line 318-320, how is the standard for spinup? It says it accounts for the competition on light and water at the sites. Please clarify this in detail.
8. Minor, I think gas exchange might be removed from the title since it is not reflected in the main body.
9. Minor, I felt the abstract is quite long and should be more concise.
10. Minor, superscript in the figure is ignored, please modify.