The authors have addressed my previous comments. I only have few minor comments to help further improve the manuscript.

R1: 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).

A1: The extractable soil water has in fact been set from field measurements per site as the difference between field capacity and soil wilting point, stratified in different layers. The 70% of extractable water has been set as the threshold value after which photosynthesis is linear reduced until it is zero when the wilting point is reached. **For consistency reasons we thus added the respective parameter H2OREF_A into Table 3 and refer to it in the text. However, we found in the FAO database that 70% is considered rather at the upper end of the range and a value of 50% (45% for C4 plants) is recommended. Therefore, we now use these values and have recalculated the simulations accordingly.** Since the literature information for this threshold value is still rather scarce, we have additionally checked the impact of the threshold parameter by varying it from 30 to 70%. The sensitivity analysis showed that the change in biomass production was between 0 and 11 percent depending on sites and crops. By using 50% for all investigated crops the results vary by +/- 7 percent at a maximum. This indicates that the parameter is not overly sensitive and that the selected values are a reasonable choice.

For applying the model at less known sites (not in this investigation), we regard the total extractable water as directly related to the soil type (e.g. Sand, Clay, Loam, etc.) and thus only indirectly to vegetation type (since agricultural vegetation is generally grown on more fertile soils).

Ccomment: The authors have considered FAO values, this is acceptable. The authors mentioned the sensitivity analysis in this reply, but without showing specific figures or tables. So, I suggest adding them (in the supplementary is also fine) to have a better understanding on the uncertainties.

R3: 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 revenant 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, the 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?

A3: Since LDNDC is a rather complex model, it is difficult to supply a few equations that are most important. In particular if the description should not be extended. **However, we have gone through the description in order to clarify potential difficulties. In particular, we have now better explained the Thornthwaite approach, which indeed is based on temperature (and daylength period), and added the description of how potential evapotranspiration is used to derive potential transpiration demand as well as the various evaporation fluxes (L255ff).**
The performance of the model to represent water fluxes above grassland and groundnut has been evaluated at the Bontioli site in a previous publication (Grote et al. 2009b). Regarding the other sites, we compared evaporation data from graphs in various publications of the sites Agoufou 2006-7, Wankama 2005-6, and Sumbrungu/Nazinga/Kayoro 2013 which were visually in good accordance

to the simulated fluxes**. In order to demonstrate that the water balance is reasonably well represented, we have introduced the measured soil water conditions at each site along with the simulated ones (new Fig. 3, 5, and 7). The comparison of both shows that the dynamics are very well met, with minor deviations at Nalohou and Agoufou only, where the water storage capacity has been underestimated in both cases. Results are additionally discussed (L366ff, L412ff).**

Comment: Thanks for adding the comparison of soil water content, is the simulation for 10 cm? please clarify the methods sections on the soil water content simulation. How about the average root depth for each vegetation/crop at each site, why choose 10-cm soil water content to compare?

I cannot totally agree with the authors that it is only minor deviations at the Nalohou and Agoufou site. It seems a serious underestimation especially at Nalohou site. I am wondering is it attributed to the overestimation of the evapotranspiration? I recommend more discussion for this relatively large bias.

R7: 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.

A7: The standard spin-up procedure has been previously explained in the same chapter with regard to agricultural sites. It consists of a three-year simulation run that is primarily needed to account for uncertain initializations of soil carbon and nitrogen pools. In case of grasslands, where the biomass initialization is also highly uncertain and varies from year to year, this spin-up period also helps to avoid steep adjustments due to specific site conditions during the first years. **Since we recognized that this description should not be placed into different paragraphs, we now join the text passages and put it in front of chapter 2.3.2 (Model setup, L278ff).**

Comment: How about the field management at each site? Is there no irrigation and fertilization as the simulation settings?

Minor comment:

Line 259, please define the RWC at the first appearance here?