

Geosci. Model Dev. Discuss., author comment AC3
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Reply on RC1

Jaber Rahimi et al.

Author comment on "Modeling gas exchange and biomass production in West African Sahelian and Sudanian ecological zones" by Jaber Rahimi et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-417-AC3>, 2021

General remarks

We are grateful for the remarks and suggestion that we happily used to improve the manuscript as described in detail below. In particular, we added a new figure that evaluates satellite derived LAI information (Fig.2), complemented Figure 1 and Table 1 with additional site information, improved the model description including new parameter indications, and added information into Figures 3, 5, and 7 for a better evaluate the representation of water balance simulations. Along the revision, a couple of minor corrections have been made regarding language or logical problems. As far as these led to additions to the text, they are all indicated in track-change mode as are the changes that had been done in response to the specific remarks.

General remarks (both reviewers)

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Specific answers to the remarks of reviewer 1 (R: Repeated remark of the reviewer; A: Answer, concrete changes in the text as indicated with bold letters)

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 linearly 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).

R2: For me, the LAI at Niakhar was underestimated, and that at Wankama 1 was overestimated, NEE at Bontoli was overestimated, is it possible to have more discussion about the reason or adjust the parameters to have a better capture?

A2: Thank you for the remark. Indeed, the local measurements of LAI in Niakhar indicate a slightly higher LAI than resulted from the model while the remote sensing misses the peaks that we represent in the Savanna region of Wankama 1. Regarding Niakhar, trees in the area that are not considered for the simulation of agricultural site likely lead to a higher LAI than simulated. In Wankama, this is likely related to the widespread occurrence of bushes in the region that were not considered in the modelled grassland type, leading the NDVI and thus LAI estimates to be larger outside the season but providing competition to the grass which prevents its full biomass development. **We have now included these discussion points into the text supplying also literature references that support these arguments (L360ff, L431ff).**

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 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, 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).**

R4: How is the parameters relevant to soil water content (field capacity and wilting point) at each site? Please clarify.

A4: In order to save space, **we added the water holding capacity (total field capacity – wilting point in mm m⁻³) for each site into Table 1.**

R5: I suggest adding the vegetation distribution in Fig. 1, which is more intuitive and easier to understand.

A5: Thanks for the recommendation. **We added the vegetation type distribution into Fig. 1 accordingly.**

R6: 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.

A6: We agree that this issue is worth investigating in more detail. Therefore, **we have added a comparison of MODIS derived and site-specific LAI measurements (new Fig. 2) and discuss the deviations between both (L205ff).** Indeed, the resolution of MODIS data is an important point here, which is further corroborated by new literature cited.

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).**

R8: Minor, I think gas exchange might be removed from the title since it is not reflected in

the main body.

A8: We beg your pardon but after some consideration we think that the relatively extensive NEE modelling part justifies mentioning gas exchange in the title.

R9: Minor, I fell the abstract is quite long and should be more concise.

A9: Thank you for the recommendation. **We have gone through the abstract and made it more stringent.**

R10: Minor, superscript in the figure is ignored, please modify.

A10: Thank you for noticing. **We corrected the letter style in the Figures (now 4, 6, 8).**