

Comments on “Improved representation of plant physiology in the JULES-vn5.6 land surface model: Photosynthesis, stomatal conductance and thermal acclimation” by Oliver et al. gmd-2022-11

The manuscript reports the addition and changes for the representation of photosynthesis, stomatal conductance and thermal acclimation in the JULES land surface model by adapting the photosynthesis model, the stomatal conductance model and adding the thermal acclimation of the photosynthetic capacity. It further evaluated the impacts of these changes on carbon, energy and water fluxes by comparing the simulation results against other available estimates of GPP from FluxCom and MODIS, turbulent heat fluxes and evaporative fraction (LE , H , $EF=LE/(LE+H)$) from FluxCom and ET from FluxCom and GLEAM. The description and argumentation for the improvements are in general clearly stated. After studying the manuscript, I wish to point out the following technical issues for considerations by the authors.

1. Because the used references are estimates themselves, it is rather difficult to ascertain if a better agreement with these references reflects a better representation of the physical processes or a better fitting due to optimized parameters. One notices the fine differences in certain parameters e.g. V_{cmax} in Table 2 and wonders how the simulation results will differ if these parameters are used on a site by site evaluation and the results evaluated against the site observations (instead of the reference estimates). As a minimum, the authors can add such information in the supplementary material and add a short discussion.
2. The treatment of the soil water stress is through its impact on the net photosynthesis in eq. 1b. This is likely not what happens on the process level. Wang et al. (2021, GMD) demonstrated that the soil water stress should be linked to leaf water stress. It is not reasonable to ask the authors to redo all the simulations for different treatments of the soil water stress, but a site by site comparison should reveal the effectiveness of each treatment.
3. What is the meaning of θ in eq. 4?
4. It is not always clearly stated what time step was used in calculating the relative RMSE with eq. 11 for the different evaluations.
5. The authors report (V_{cmax} , J_{max} and $J_{max}:V_{cmax}$) but it is not clear why a third quantity is needed while one can be derived it from any other two.
6. It is cosmetic, but to this reviewer the plots in Fig. 4 visualize better if they are rotated by 90 degrees.
7. The authors stated the overestimation of ET in SON (Fig. 3b) by all model configurations but a short discussion to the reasons should be provided.