

Interactive comment on “JULES-crop: a parametrisation of crops in the Joint UK Land Environment Simulator” by T. Osborne et al.

Anonymous Referee #1

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This paper describe a new simple parametrization included in the JULES land surface model to take into account for specific behaviour of crops into the model. This follow a general and important recent trend in global land surface models to better represent the behaviour of ecosystem largely managed that greatly differ from natural vegetation. I think that it is an important and necessary effort for land surface model and then I greatly support such kind of development in the JULES model. Moreover this kind of paper perfectly fit with the scope of geoscientific model development. So I recommend the publication of the paper. However I think that it can be improved in several ways. This is the reason why I quoted "major revision", even if it doesn't means a lot of additional efforts.

As general comment, I find the paper clear and the equation well described even if the

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style of the paper is sometime a little surprising. In particular, it ask some questions to the reader like "how much detail is required ?" "what's more ?" etc.. which is not very conventional !

My main concern is that the model evaluation part is a little light and should be enhanced. For instance model is only evaluated on 3 sites for a total of four sites/years. Then only soy bean and maize is represented. A large set of sites on crops are now available with some sites that have more than 10 years of data. This allows to cover the main crops types and several regions in the word. So it is really a pity that model be compared to a so limited set of data. I think that evaluation should be really improved by comparing with a larger dataset that allow evaluation of the 4 crops types represented, for different regions and considering longer time period to evaluate the ability of model to represent the interannual variability for each site. Only H and LE are compared. Why did you not included the NEE fluxes that are probably available for these sites ?(or at least an estimation of the GPP as if I understand well, there is only a short model spinup that does not allow to equilibrate the soil carbon).

In the simulation at global scale only maps from model results and comparison to averaged global yields is shown. A more regional view of ability of model to reproduced spatial distribution of yields is missing. Obviously, as mentioned by the authors direct comparison the actual yields is difficult since the model does not take into account for specific local management an species. But at least in would be important to see if model is able to reproduce regional climate driven difference in estimated yield. Likewise several model configurations have been implemented but are not evaluated in the paper. For instance a method to automatically determine the sowing date has been implemented but no results are shown in the paper. A method to take into account for photoperiod constraint on estimation of the development index was also included but not used in the simulations. I think it would be important to add a part showing the impact of these different parametrisations on simulated fluxes and yields.

specific comments:

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p 6780: Even if the different model parameters are defined in table 1, it would be more convenient for the reader to remind it after equation, this is for instance the case for TT_{emr} , TT_{veg} and TT_{rep} in equation 3

p 6780 eq 4: what is the meaning of the 0.012 term ?

p 6781: there is a paragraph that justify definition of continuous coefficients for allocation to biomass compartments that is very long and not very clear. I think this could be shortened as it is obvious for me that defining a parametrisation for allocation coefficient is ever better than a lookup table ! p 6783 eq 9,11,13: I didn't find the definition of f_c ?

p 6785 l 19: Typo, Missing the T of "The"

p 6790 : I am surprised in figure 8 to see so little differences in simulated LE flux for instance considering the large difference in LAI between the standard and crop version. In particular there is a large LE pic in May simulated all the versions even if LAI is very low in the crop version. Do you have an explanation for that ? I think it would be important to discuss this point as it is mentioned that at the end, the new parametrization does not change a lot the result, which is indeed what we see in the site simulation but that is strange for me as LE should be, in spring and summer, largely driven by plant transpiration and then by LAI. So I would expect that the large LAI change induced by the new crop parametrization should has a larger impact on fluxes.

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