Response to Referee #2

The authors thank the anonymous Referee #2 for his/her review of the new version of the manuscript and for his/her helpful comments.

A new version of the manuscript was prepared. It includes all the response elements given by the authors in response to the new comments of Referee #2 (in blue).

2.1 [The authors have made an effort to change the manuscript and take into account the reviewers comments. Some of my comments were addressed, but others were either not understood or not taken as importantly.]

RESPONSE 1

Many thanks for going into these more specific issues. This will improve the Discussion Section of the paper. The editorial comments will be accounted for as well.

2.2 [I requested a discussion of the fact that the model in Equation 5 is known to limit transpiration too early. I believe the authors have not caught on this comment enough. In their response they comment on the fact that this issue is not so relevant for them since they work with water content not water potential. They did however look into the recommended reference and cited additional research. I agree with the citations, but there is more. I want to make my point again, and I have attached a Figure to illustrate it. I am using Eq 1 and 5, with rooting depth = 1.6m and R_e=0.97 (just for illustration-this can be repeated with any parameters to obtain the same pattern). Now I changed water availability as shown in the attached figure (in the blue case the upper soil is drier than the bottom and black the other way around). Fs(i) is plotted on the right hand side and while the Fs of black sums up to almost 1, blue only sums up to 0.55. There is some compensation, but very little. Now the main question is: How is Ft calculated (Eq. 6). In many models it is simply: Ft = To * Fs, with To the potential Transpiration. If this is the case in the ISBA model (needs to be stated, there is only qualitative information), transpiration is severely limited in the blue case although a great deal of water is still available at depth. This is the problem stated in Feddes et al (2001) and needs to be discussed. Plants are able to transfer water uptake to compensate water stress in the top layers, and this model can not adequately account for it. This fact probably explains part of why this model performs rather poorly. I would like to emphasize also that the problem is not with the Jackson model (This power decrease or root length density is likely valid), but with the fact how it is implemented with the model to obtain water stress and stomatal closure. The discussion currently addresses the Jackson model (Eq.1), when it should address Eq. 5-6, and state that other options may have more success.]

RESPONSE 2 [see revised Sect. 4.1 of the new version of the manuscript]

Thanks for clarifying this question.

We propose a new title for Sect. 4.1: "Are the Jackson root profile model (Eq. (1)) and the resulting water availability (Eq. (5)) applicable at the regional scale ?"

Using the Jackson root density profile in Eq. (5) rather than a uniform profile has a marked impact on the simulated water balance (Sect. 4.1). In situations where the top soil layers are drier (wetter) than deep soil layers (i.e. present lower (higher) FS_i values), the total FS value is lower (higher) in DIF simulations than in FR-2L simulations. This tends to trigger an earlier senescence in DIF simulations. This is illustrated by Fig. 12.

Figure 12 shows that situations in which the top soil layers are drier than deep soil layers tend to be more frequent in DIF1 simulations than in DIF1-Uniform simulations, in relation to the enhanced root water uptake close to the soil surface. Therefore, for given MaxAWC and soil wetness conditions, the total FS values tend to be lower in DIF1 simulations than in DIF1-Uniform (and FR-2L) simulations. This results in less evapotranspiration and less GPP. The lower GPP in DIF simulations results in lower BagX values, especially for cereals as illustrated in Fig. 10.

FT is calculated as explained in Sect. 2.2 and Supplement 1. The total FS value is used to calculate the photosynthesis parameters gm and Dmax in water-limited conditions. This will be made clear in Supplement 1.

As noted by Feddes et al. (2001), the limitation of transpiration is DIF simulations when a great deal of water is still available at depth is probably too severe. In the real world, plants are able to transfer water uptake to compensate water stress in the top layers, and DIF simulations cannot adequately account for it. This fact probably explains part of why this model is not able to outperform the FR-2L simulations.

2.3 [I am really worried about the fact that there was apparently no separation between a calibration and validation period. If I understand correctly, the presented model quality matrices were obtained for the calibration run (The model was optimized to maximize the correlation coefficient between AGRESTE GY and modeled Bag. The same data are used to assess the model quality.). This does however not reflect the quality of the model, which can be confirmed in an independent run with data not used during the calibration, since calibration of a complex model may lead to interaction with other parameters and may not represent the real process any more. At the minimum this fact should be stated in the discussion, maybe using Refsgaard (1997) as a starting point (although it is from a different discipline) and work citing this.]

RESPONSE 3 [see L. 533-543 of the new version of the manuscript]

Yes, we agree. This could be confusing and needs to be clarified in the final version of the manuscript (see below). Also please see our response to Referee #3: "A key objective of this study was to benchmark DIF options, not to predict the agricultural yields. Therefore, using an independent dataset to assess yield prediction was not needed".

The model is optimized to maximize the correlation coefficient between Agreste GY (or DMY) and modeled BagX. The resulting r is used a metric to assess the capability of a given model configuration to represent the interannual variability of BagX, over the 1994-2010 period. In studies where the objective of the model calibration is to improve the model prediction for operational applications, the model quality needs to be confirmed in an independent run with data not used during the calibration. An example of rigorous calibration and validation procedure in hydrology can be found in Refsgaard (1997). In this study, a validation run was not performed as the considered period was too short to apply a split-sample procedure and separate calibration and validation sub-periods. Moreover, the objective of this study was to benchmark DIF options, not to predict the agricultural yields. Therefore, using an independent dataset to assess yield prediction was not needed.

REFERENCE

Refsgaard, J. C.: Parameterisation, calibration and validation of distributed hydrological models, Journal of Hydrology, 198 (1-4), 69-97, 1997.

2.4 [Minor point: Please change Eq. 2-3, use mathematical expression rather than prose and nomenclature to express what is meant. This will really improve deciphering what is done. If I understand correctly, than Eq. 2 is SWI_top = sum_{i=0}^{i=k}(…), where k is the index of the deepest layer considered as “top”. SWI(d_L,i) could become SWI_i to keep things simpler. I also think that FS(i) in Eq3 would better be FS_i.]

RESPONSE 4 [see pp. 11-12 of the new version of the manuscript]

Yes. These changes were made.