

Interactive comment on “Evaluation of root water uptake in the ISBA-A-gs land surface model using agricultural yield statistics over France” by N. Canal et al.

Anonymous Referee #2

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The aim of this paper is to investigate how different representations of root water uptake improve the prediction of yield (grain and grass biomass) in a land surface model. To evaluate the suitability of the methods, first two parameters (maximum plant available water and mesophyll conductance) are optimized and afterwards the best model fits are compared, by testing whether the modeled yields correlated significantly with the observations. The topic of this paper is of central interest to the hydrological community, as models for predicting yield are used frequently, and how to best account for root water uptake is still unclear.

However, I feel the paper has substantial weaknesses, some related to the structure

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and some related to the applied methods. Also the statement of the goal of the paper and motivation are diffuse, and do not allow to extract the take home message of the presented investigation.

General comments

(GC 1)

To me the motivation and the message of the paper are unclear. Why is this investigation important and which additional information does it provide beyond CA12? I understand that CA12 showed that the maximum available soil water content (maxAWC) was an important parameter in the model. Possibly, this motivated the idea that below ground processes may need to be represented in a more processes based manner? Any linking statement would be helpful. Also, quite early in the paper it becomes clear that data on soil properties and rooting depth (defining maxAWC, eq. 4) can not be obtained at the scale at which statistical data are available. I am wondering whether this dataset was then at all suitable to address the research question? Could you comment why it is better to use this instead of other data sets? Maybe it is related to the scale, at which the model is supposed to predict?

(GC 2)

I found it difficult to understand whether the envisaged application of the model is in (yield) prediction or in learning about soil/rooting properties? This is of importance for evaluating the suitability of the methods. The model was fit to the observation and afterwards the quality of the (same) fit was interpreted to judge whether one modeling scheme was better suited than another. If the model is intended to predict yield: I would generally assume that such a comparison can only be done on a validation period, i.e. comparing the model to data for a forward simulation, not for the period used to find the best parameter fit. If the model is intended to infer max AWC: I would have assumed that soil data are collected and presented. A discussion of the intended purpose of the model and how this paper adds to this is needed.

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(GC 3)

When comparing the model to the observation, extra information is needed. Presented is only the correlation coefficient and whether it is significant. This does not inform the reader about a potential bias, which would better help to evaluate the results (see for example Gupta et al., 2007)

(GC 4)

The discussion does not present much interpretation and implications of the results, much of it reads like the continuation of the results section (for example section 4.2). At the same time some issues are not discussed at all. For example, the representation of root water uptake in eq. 5 is known to reduce uptake too early when top soil dries out (Feddes et al. 2001), which may explain some of the results. A discussion of this would definitely improve the paper. Also, a discussion of what this paper adds to the very strongly related earlier paper CA12 is needed.

(GC 5)

Please add a table stating the abbreviation to improve navigation through the manuscript.

Detailed comments

DC1 – abstract: There is a discrepancy between the stated aim of the paper and the formulated take home message. The title suggests that root water uptake schemes are tested, the abstract states that both different representation of the soil (including soil hydrology) are tested together with different uptake schemes. But no conclusion is stated concerning whether or not any of those schemes was better suited than the other. It only states than within one scheme there were differences regarding the representation of additional layers below the rooting zone.

DC2 – p5426, L15-19: I feel this explanation cannot be understood by the general audience.

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DC3 – p5428, Eq. 2,3: I do not understand what you mean by $w(d_L)$ as opposed to $w_{top}(d_L)$? Therefore, I do not understand the difference between the equations. Also, consider using θ for volumetric water content.

DC4 – p5429, L1: Do you mean “soil water content” instead of “soil water column”

DC5 – p5430, L24: an instead of a for “an hourly basis”

DC6- p5431, L12-14: Please state, what was done with these data points, were they removed? Do you mean “not considered [i.e. removed], in order to be consistent .. “

DC7-p5436, section 4.1: It would be good to answer explicitly the question stated in the title. Also, this is the main question of the paper and it deserves more in depth discussion of the pros and cons of this model, making use of the available literature (good start would be looking into those citing the very relevant paper by Feddes et al 2001).

DC7-p5436-37, section 4.1: This section reads much like a results section. What is the interpretation of those results? Also, with “vegetation canopy” you probably mean vegetation type (crops and grassland)? It would be easier to understand, since the canopy was not much referred to in the rest of the paper.

DC8, p5437-38, section 4.3: Title: Can you be more specific than using model “use” – do you mean model prediction? Much of this section (p5437,L17-p5437, L11) reads like results and should be moved to the results section.

DC9, p5438-39, section 4.4: The title states an interesting question: How to better constrain MaxAWC at different scales? But I see this question only addressed in a half sentence (stating that the resolution of the database is too coarse). Other comments, such as on radiation, do not match the heading of the section. It would be good to have a more encompassing discussion of this issue here, since it is important.

DC10, p5439, L1: Something went wrong with this sentence, the SAFRAN seems misplaced or needs to be explained.

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References

Feddes, R. A., Hoff, H., Bruen, M., Dawson, T., De Rosnay, P., Dirmeyer, O., Jackson, R. B., Kabat, P., Kleidon, A., Lilly, A. and Pitman, A. J.: Modeling root water uptake in hydrological and climate models, *Bulletin Of The American Meteorological Society*, 82(12), 2797–2809, 2001.

Gupta, H. V., Kling, H., Yilmaz, K. K. and Martinez, G. F.: Decomposition of the mean squared error and NSE performance criteria: Implications for improving hydrological modelling, *Journal of Hydrology*, 377(1-2), 80–91, doi:10.1016/j.jhydrol.2009.08.003, 2009.

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