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Hydrology and Earth System Sciences
Discussions

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.

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

2.1 [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.]

RESPONSE 1

Thanks for this positive comment.

2.2 [GC1 - 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 (max- AWC) 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.]

RESPONSE 2

Although ISBA-A-gs is not a crop model and agricultural practices are not explicitly represented, Ca12 achieved a good representation of the interannual variability of the dry matter yield (DMY) over many grasslands sites in France. On the other hand, representing the year to year variability of the grain yield (GY) of winter/spring cereals was more difficult. In particular, they showed that the modeled above-ground biomass was markedly sensitive to the representation of the soil moisture stress, through the Max-AWC parameter (especially at low MaxAWC values). The study of Ca12 was carried out with a simple, single-layer representation of the root-zone soil moisture over the 1994-2008 period. The main objective of this study was to assess to what extent using more refined representations of the soil hydrology and of the root water uptake could improve the representation of the interannual variability of GY (and possibly DMY). Since several options could be envisaged to implement the DIF simulations, a side objective of this study was to benchmark these options. These motivations will be clarified in the Introduction part of the paper.

2.3 [Also, quite early in the paper in 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 is model is supposed to predict?]

RESPONSE 3

So far, the French annual agricultural yield data are publicly available at the département scale, only. In order to take advantage of the existing information on soil properties, an option could be to use satellite-derived LAI products at a spatial resolution of 1 km x 1 km in conjunction with soil maps at the same spatial resolution (e.g. derived from the Harmonized World Soil Database, Nachtergaele et al. 2012). Since these products are now available at a global scale, the methodology explored in this study over metropolitan France could be extended to other regions. As suggested by Feddes et al. (2001) and Decharme et al. (2013), the obtained "effective root distribution function" could be validated using river discharge observations by coupling the LSM with an hydrological model. We will investigate this possibility in a future work. Note however that the river discharge is often impacted by anthropogenic effects such as dams and irrigation. Such effects are not represented (or not completely represented) in large scale hydrological models (Hanasaki et al. 2006).

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Nachtergaele, F., van Velthuize, H., Verelst, L., Wiberg, D., Batjes, N., Dijkshoorn, K., van Engelen, V., Fischer, G., Jones, A., Montanarella, L., Petri, M., Prieler, S., Teixeira, E., and Shi, X.: Harmonized World Soil Database (version 1.2),

2012, available from: http://webarchive.iiasa.ac.at/Research/LUC/External-World-soil-database/HWSD_Documentation.pdf

2.4 [GC2 - 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.]

RESPONSE 4

The ISBA-A-gs model is intended to bridge the gap between the terrestrial carbon cycle and the hydrological simulations (e.g. river discharge). In previous works, the ISBA-A-gs model was coupled with hydrological models able to simulate river discharge (e.g. Queguiner et al. 2011, Szczypta et al. 2012). While simulating vegetation requires a good description of the soil water stress, hydrological simulations are sensitive to changes in the representation of the surface water and energy fluxes. The latter are controlled to a large extent by vegetation. In Ca12, an effort was made to benchmark two options of the vegetation model (drought-avoiding vs. drought-tolerant). In this study, an effort was made to benchmark several options of the soil hydrology model. ISBA-A-gs is not a crop model and does not predict yield per se. The background assumption of this work was that the regional scale above-ground biomass simulated by a generic LSM could be used as a proxy for GY or DMY in terms of interannual variability. This assumption is discussed in Sect. 4.3 (a Discussion section). Since several options could be envisaged to implement the DIF simulations, a side objective objective of this study was to benchmark these options at a regional scale and learn

about the root water uptake.

2.5 [GC3 - 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)]

RESPONSE 5

The quantitative consistency between the simulated biomass and the agricultural statistics was extensively discussed by Ca12 (Sect. 3.3 and Figs. 12 and 13 in Ca12). For cereals, they considered the ratio of crop yield to the maximum above-ground biomass, called the harvest index. The later ranged between 20% and 50% and this was consistent with typical harvest index values given by Bondeau et al. (2007) for temperate cereals. The same result is obtained in this study (not shown). For grasslands, Ca12 simulated both managed and unmanaged grasslands. For managed grasslands, DMY was explicitly simulated and ranged between 0.1 and 0.8 kg m-2. The scatter of the simulated DMY was relatively small, with a standard deviation of differences with the Agreste DMY of 0.20 kg m-2. ISBA-A-gs tended to slightly underestimate DMY values, with a mean bias of -0.08 kg m-2. For unmanaged grasslands, the simulated Bag was 0.17 kg m-2 higher than the Agreste DMY values, on average. In this study, unmanaged grasslands were considered, only, and results similar as those of Ca12 were found (not shown).

REFERENCE

Bondeau, A., Smith, P. C., Zaehle, S., Schaphoff, S., Lucht, W., Cramer, W., Gerten, D., Lotze-Campen, H., MÂĺuller, C., Reichstein, M., and Smith, B.: Modelling the role of agriculture for the 20th century global terrestrial carbon balance, Global Change Biol., 13(3), 679–706, 2007.

2.6 [GC4 - 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.]

RESPONSE 6

Yes, we agree. The Discussion section of this paper needs to be improved. The responses to the referees' comments will be incorporated into the Discussion section of the final version of the paper. Section 4.2 was placed in the Discussion section as we considered that the impact of the representation of photosynthesis on the results was a side objective of this work. Contrary to Feddes et al. (2001), the root water uptake in ISBA-A-gs is driven by soil moisture, not by soil water pressure head. We use a more refined representation of the soil water stress function, in relation to key photosynthesis parameters (see the response to Referee #1 comments). The impact of the shape of the root density is discussed in Sect. 4.1. It is showed that the top soil layers dry out first (through SWItop at dL = 0.46m), and the consequence is an earlier senescence than for a control simulation with a uniform root profile (DIF1-Uniform). It must be noted that Fig. 10 shows that root water uptake is reduced earlier with FR-2L than with DIF1, in relation to a faster plant growth in the FR-2L simulation. For C3 crops, a drought-avoiding response to soil water stress is simulated, triggering an increase in WUE (and in the plant growth rate) as soon as theta < theta_fc. Since the DIF1 simulations tend to accumulate water above the field capacity (i.e. theta remains longer above theta_fc than for FR-2L), the increase in WUE tends to occur later than for FR-2L.

2.7 [GC5 - Please add a table stating the abbreviation to improve navigation through the manuscript.]

RESPONSE 7

Yes, a lot of abbreviations have been used in this manuscript. A nomenclature Table listing the symbols and their definition will be added.

2.8 [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.]

RESPONSE 8

The following sentence could be added to the abstract: A rather neutral impact of the most refined versions of the model with respect to the simplified soil hydrology scheme is found. This shows that efforts should be made in future studies to reduce other sources of uncertainty e.g. using a more detailed soil and root density profile description together with satellite vegetation products.

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

RESPONSE 9

Yes. The following sentences “For moderate soil water stress, the drought-avoiding (drought-tolerant) response results in the increase (decrease or no change) of the Water Use Efficiency (WUE). In the drought-tolerant response, WUE does not change or decreases” could be simplified and replaced by: “For moderate soil water stress, the drought-avoiding response results in the increase of the Water Use Efficiency (WUE). In the drought-tolerant response, WUE does not change or decreases”. Moreover, a Supplement could be added to the final version of the paper in order to better describe

the photosynthesis model.

2.10 [DC3 - p5428, Eq. 2,3: I do not understand what you mean by w(dL) as opposed to wtop(dL)? Therefore, I do not understand the difference between the equations. Also, consider using "theta" for volumetric water content.]

RESPONSE 10

Yes, "theta" could be used for volumetric water content. Eq. (2) is used to assess the soil moisture stress in a single soil layer or in several soil layers forming a bulk layer from the surface to a depth dL. Eq. (3) is used to assess the soil moisture stress of an individual soil layer at depth dLi. Eq. (2) and Eq. (3) are used to calculate the stress function in FR-2L and DIF simulations, respectively.

2.11 [DC4 - p5429, L1: Do you mean “soil water content” instead of “soil water column”]

RESPONSE 11

Yes, we agree. We will replace the term “soil water column” by “soil water content”.

2.12 [DC5 - p5430, L24: an instead of a for “an hourly basis”]

RESPONSE 12

Yes, it will be done.

2.13 [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 .. “]

RESPONSE 13

This sentence : "In the case of crops, Bag values after 31 July are not considered, to be consistent with the theoretical averaged harvest dates in France." could be replaced by "In the case of crops, simulated Bag values after 31 July are not considered, in order to be consistent with the theoretical averaged harvest dates in France."

2.14 [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).]

RESPONSE 14

Yes, more elements of discussion about the question stated in the title will be added. A special attention will be made with the literature that can be found in Feddes et al. (2001). In particular, one may emphasize that the approach used in this study to simulate the root water uptake is relatively simple and may not be relevant to represent what really happens at a local scale. Higher level models are able to simulate the root network architecture and the three dimensional soil water flow (Schneider et al. 2010, Jarvis 2011). Also, the hydraulic redistribution of water from wetter to drier soil layers by the root system (hydraulic lift) is not simulated in this study. Siquiera et al. (2008) have investigated the impact of hydraulic lift using a detailed numerical model and showed that this effect could be significant.

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Schneider, C. L., Attinger, S., Delfs, J.-O., and Hildebrandt, A.: Implementing small scale processes at the soil-plant interface – the role of root architectures for calculating root water uptake profiles, Hydrol. Earth Syst. Sci., 14, 279–289, 2010.

Siqueira, M., Katul, G., and Porporato, A.: Onset of water stress, hysteresis in plant conductance, and hydraulic lift: Scaling soil water dynamics from millimeters to meters, Water Resour. Res., 44, W01432, doi:10.1029/2007WR006094, 2008.

2.15 [DC7 - p5436-37, section 4.2: 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.]

RESPONSE 15

For the sake of clarity, the title of Sect. 4.2 could be replaced by: "Have changes in the representation of photosynthesis an impact on the model performance ?". An introduction sentence could be added: "In this section, the impact of the revised vegetation radiative transfer scheme and refreshed gm parameter (DIF1-NRT experiment) is discussed.". See also Response 6.

2.16 [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.]

RESPONSE 16

Yes, part of Sect. 4.3 could be moved to the results section. The title of Sect. 4.3 could be reworded as : “Can the ISBA-A-gs model predict the relative gain or loss of agricultural production during extreme years?

2.17 [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.]

RESPONSE 17

Yes, title of Sect. 4.4 is confusing. One could remove "at various scales" from the title.

2.18 [DC10 - p5439, L1: Something went wrong with this sentence, the SAFRAN

seems misplaced or needs to be explained.]

RESPONSE 18

Yes, "is SAFRAN" should read "in the SAFRAN atmospheric analysis".

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