Hydrology and Earth System Sciences Discussions

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Comment on hess-2021-316

Anonymous Referee #2

Referee comment on "Coupled modelling of hydrological processes and grassland production in two contrasting climates" by Nicholas Jarvis et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-316-RC2, 2021

Major comments

This is an interesting and very well written paper, that introduces an novel approach, that of transferring intact lysimeters from a wet cool climate to a much drier and somewhat warmer climate in Germany, to study the effects of climate (change) on the water balance and growth of grass vegetation. The paper has long introduction and materials and methods sections that already reveal a substantial amount of results and bring in a lot of literature references. At times it feels more like a review, and some of these references could perhaps have been saved for the discussion. The methods used are overall sound, although it was not fully clear to me why they had not selected a more recent and complete grass hydrology/growth model. Why was interception not included? This would not have caused much computational burden.

What frustrated me a little was the fact the Results and Discussion section is rather short. After all this pre-amble on how the model works, the reader is still left with quite a few questions. For example, what are the reasons for the upward flow in the drier lysimeters, even though LAI and Dry Matter are lower? Is this real? Is it the deeper root depth for these lysimeters that could have caused it. I would like to see the authors elaborate a bit more hear, for example by going back to the findings of their sensitivity analyses.

Minor comments

Line 195-196: "It is also striking that the actual evapotranspiration slightly exceeds precipitation at Selhausen, so that the net percolation at the base of the lysimeters is negative (i.e. an upwards directed flow; Table 2)."…. Does this mean that the percolation was not measured separately? You say that the lysimeters enable the measurement of a complete (closed) water (line 113)? What is causing this negative percolation (capillary rise?).

Chapter 2, Materials and Methods already contains a lot of results on the lysimeter water balance, AG biomass etc. Should that be moved to the results?

Line 214: water use efficiency (WUE) of the grassland in the drier climate was lower than that of the wet climate. Would we have expected the opposite (plants becoming more efficient under dry conditions)? So, is it really the leaf-level WUE hat has changed, or is this the result of other factors?

How exactly is infiltration calculated in the model? And the flow at the bottom boundary of the soil profile? What about Runoff?

In Line 424 and further you talk about feedbacks from the plant growth model to the hydrological model. You mention the effect of LAI and height on the aerodynamic resistances and hence on the ET fluxes. Surely LAI also affects radiation extinction, and therefore the energy available for ET. This could be mentioned too? Also, the aerodynamic effects will have been relatively low. From that point of view would it not have been better to also consider interception (as it is also affected by LAI)? Although the values for grass are low, they are comparable to winter values of ET, and the equations required are straightforward?

Lines 489-492 You say: "The measurements from the matric potential sensors installed in the uppermost soil horizon (0-24 cm depth) appeared to be unreliable. We therefore also used the HYPRES pedotransfer functions to estimate the shape parameter n in the topsoil, while α was set equal to the same value as the deeper horizons. First of all: why where these measurements unreliable? Was the soil too dry? How do you know that the deeper sensors could be deemed reliable?

Also, can I ask why you did not use the VG parameters for the medium-layers where you did have measurements, instead of having to revert to generic HYPRES PTFs? Were these horizons too different?

Your alpha parameter in Table 3 appears to be the same throughout the entire profile, yet n varies considerably. Is this realistic? How come that theta_s in the first soil layer is so much higher in Rollesbroich?

In Table 5 you talk about post-priori parameter ranges, whereas in the text (line 579-580) you mention that the posterior uncertainty ranges are much smaller than

the prior uncertainty ranges. In figure 3 you talk about posterior distributions of the four parameters. Where exactly are you showing the prior uncertainty ranges? I find this all a little confusing.

Lines 595-596: You say: "The simulations suggest that the maximum root depth at Selhausen has increased to ca. 80 cm,while the maximum stomatal conductance has roughly doubled". Were you not able to measure the root depth? I guess this would have caused destruction of the lysimeter core. Also, you say that stomatal conductance has doubled, but could it be that the parameter had assimilated aerodynamic effects to changes in vegetation structure?

You hint at this perhaps in the following sentences, but it is not clear.

In Figure 4 you need to make it clear which set of 3 graphs is representing which site (the same goes for Fig. 5). It is hidden in the legend but should be more explicit. Also, based on these plots it is surprising that you opted for a theta_s of 0.55 for Selhausen (estimated "by eye"). I understand that these are daily averages (?), but during the winter months there much have been saturated conditions?

In Figure 5 it is quite hard to see what is going on. Would it be possible to separate the years somehow? Or perhaps make cumulative plots?

Line 611-612: You say that the "model performs very well, matching the temporal dynamics in the high-time resolution data on state variables and fluxes as well as reproducing the differences in the overall water balances at the two sites". I am not sure that Table 6 reflects that statement? While model efficiencies are high the values for ET are much lower and values for LAI are negative (with LAI being a crucial variable in many equations), which makes me wonder whether Figure 5 hides a multitude of sins..?

I find the discussion around Fig. 7 somewhat incomplete. While soil evaporation clearly depends on soil moisture content, windspeed etc. it also depends on incoming radiation (which would have been lower in Ro and higher in Se), and radiation reaching the soil through the canopy. Seeing the DM was lower in Se (and therefore LAI, see also figure 9) I would not necessarily have expected soil evaporation to have been lower for the Se site.

Line 628: You say: At Rollesbroich" grassland is harvested 3-4 times during the growing season" Was this not the case at the Selhausen site? This makes comparison between the two sites difficult?

The discussion around water stress, with a focus on Figure 8, seems to ignore the fact that

one of your earlier figures indicated that the rooting depth at Selhausen was much deeper (80 cm) than at Rollesbroich. Is that mot the main reason for the relatively modest water stress experienced at this site?

Line 692-693: Are some of these ME values less than excellent if the best value is 1? Can you provide a scale for what consititues poor, good, excellent etc. in the methods where you introduce ME?