Review of 'The effect of chalk representation in land surface modelling' by M. Rahman and R. Rosolem, HESSD 2016

The work evaluates an alternative representation of soil hydraulic properties to estimate soil moisture, latent heat and runoff in a chalk catchment using the land surface model JULES. The Bulk Conductivity (BC) scheme is chosen to approximate the chalk dual permeability behavior, and is compared to the default JULES soil parameterisation. The scheme is assessed to reduce JULES soil moisture dry bias, and to improve ET and runoff estimates.

One of my largest concerns regarding this work is its **novelty and contribution to knowledge**. The abstract (l. 13-15) states the work significance as "it is hypothesized that explicit representation of chalk hydrology in a land surface model influences land surface processes by affecting water movement through the shallow subsurface", and that the results corroborate the proposed hypothesis (l. 23). While being a very confusing statement (as our representation in a model does not actually affect any water movement or land surface processes), it is not clear why such a hypothesis is novel and/or requires another round of consideration, as it has been previously shown by others that explicitly accounting for the chalk behavior in the same land surface model and for the same location does have an effect on all the fluxes and states considered in the study, i.e. soil moisture, ET (latent heat), and runoff (see the cited Le Vine et al., HESS 2016, and Bakopoulou, PhD thesis 2015 found on https://spiral.imperial.ac.uk:8443/handle/10044/1/28955).

In this light, it seems that the main development is the use of the previously proposed by others **Bulk Conductivity model** to represent chalk hydraulic properties in the land surface model. The BC model is given by eq. (1), which shows that the BC model is activated only when soils are wet (relative saturation above 0.8), and that drier soils are governed by a more traditional van Genuchten soil hydraulic representation with parameters given in Table 3 (note that the two out of four parameters for this model are taken from Le Vine et al., 2016; and the used third parameter Ks equals Ks in the same reference). Figure 9 shows that catchment average relative saturation (S) never exceeds 0.8 (a threshold when the BC model is activated), and there is no point scale characterisation of relative saturation provided to make a judgment about what happens at the application scale of eq. (1). Does this mean that the BC scheme is never activated, and the obtained results are based on a simple re-iteration of the van Genuchten parameterisation, which was used and evaluated by others previously? If not, then the instances when the BC model is activated have to be shown, as it appears to be what distinguishes the work from the work of others. Lastly on this point, the statement in the summary section (l. 373-374) that "BC model was able to reproduce the hydrological processes in chalk without model calibration" is confusing and incorrect, as 1) it is not clear whether the BC portion of the model given in eq. (1) was ever activated, and 2) the chalk behavior for drier states – an inseparable part of the BC model to represent chalk - was governed by van Genuchten representation with parameters calibrated by others for the same application site.

Furthermore, the work compares two setups: 'default' when the standard soil parameterisation is used in JULES, and 'macro' when soil is **uniformly** represented as a

30 cm topsoil, and as chalk from 30 cm to 5 m depth. And it is stated (l. 373-374) that based on the macro model simulations “BC model was able to reproduce the hydrological processes in chalk”. I find this statement surprising at the catchment scale, as **macro is an incorrect model setup** for the catchment, as approximately a third of the catchment hydrogeology is not chalk (see the BGS hydrogeology map, or Figure 1 in Le Vine et al., 2016), and thus the application of the chalk soil model uniformly throughout the catchment is erroneous.

Lastly, could the authors comment on how river **flows** were estimated at the outlet when there is no groundwater model available while the catchment is groundwater dominated? It will be very interesting to see the flow hydrographs to compliment the provided flow statistics (given in Table 4).

Regards,

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