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Interactive comment on "The effect of chalk representation in land surface modelling" *by* M. Rahman and R. Rosolem

A. Ireson (Referee)

andrew.ireson@usask.ca

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Review of "The effect of chalk representation in land surface modelling"

Rahman and Rosolem

The authors are to be congratulated for writing a paper on modelling flow in the Chalk unsaturated zone which includes only one equation - surely a record! In fact, this flippant comment underlies a more serious point, which is that the potential strength of this study lies in it's simplicity. The authors are working in a field where others have proposed various different, highly complicated models. The authors have taken a very simple model configurations and applied it to the Chalk in a manner that goes beyond

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a completely naive model that might be used in routine large scale model applications (i.e. the default configuration shown here) in a manner that could be easily included within routine applications of large scale models. All of the data used to configure this model are readily available in soil databases and the Chalk literature. This is the potential contribution of this paper, in my view - it is a model for the Chalk that could be readily picked up and used by almost anyone. I think that is the holy grail which I myself and others have been searching for in the Chalk.

However, that is the potential contribution. The fundamental problem with this study is that the default model outperforms the macro model in a number of important respects. The authors failed to recognize this because they focused on which model better fits the absolute water content. This is really not an important metric of model performance - far more important is the changes in water content and the groundwater recharge signal. The default model probably outperforms the macro model in simulating the changes in water content. The authors appear not to have thought critically about the simulated potential recharge flux in relation to the water table responses - again in this regard the default model seems much better, as I detailed further below.

So, I think the authors are on the path to having a significant contribution, but not there yet. I suspect that it is the parameters they are using, specifically the matrix K is too low, that are to blame for the poor model performance. Model calibration, ideally combined with a parametric sensitivity/uncertainty analysis, is essential before this work can be published. I am therefore recommending major revisions.

Another issue in this paper is that the contributions are not well described. The abstract and conclusions in particular are poorly written. It is my own suggestion that the contribution here is the simplicity of the model - the authors do not say that. Instead, a very tepid and vague hypothesis about their parameterisation having some sort of influence on the model. This must be strengthened, since there is a potentially very nice piece of work here.

Major comments

The abstract is poorly written. The hypothesis is not well phrased, and uninteresting as phrased. The conclusion in the abstract is vague and doesn't make me want to read the rest of the paper. This can be improved with some more careful thinking about what the contribution of this study is, and highlighting this clearly for the reader.

The premise of the paper, while poorly described, is good - that is to take a new conceptualization of the hydraulic properties of the Chalk, test it at a point scale against local observations, and then apply this at the catchment scale.

In Section 3, it feels like the description of how the soils and Chalk were parameterized is spread out and not well organized. Perhaps you should mention this parameterization at the begining of Section 3.4 before talking about the two different scales.

In Figures 3 and 4 you show the performance of the default and macro model, respectively, in reproducing observations of soil moisture. In the text (L. 223-245), your focus is the marked improvement in the macro model at simulating the absolute values of water content in the deeper soil layers. This is also the message of Figure 5 which uses relative bias as a model performance metric. This is valid but actually I'm more interested in how well the models capture changes in soil moisture, which is a more important metric from the perspective of the water balance and recharge estimates. In this respect, it appears to me that the default model may actually be better than the macro model, and an optimal model might be somewhere between these two extremes. To highlight this point, consider Figure 6 d), which shows the potential recharge flux (or the drainage flux, if you prefer that term) from the base of the 5m model. Consider the fact that this flux will ultimately drive water table fluctuations in the Chalk, 10s of meters below ground level. As is well documented elsewhere (e.g. Wellings and Bell, 1980, who put together the classic understanding of Chalk recharge in their excellent Figure 1.) the water table follows a clear seasonal pattern. Only the default model here could result in that pattern. (Note also that water table observations at this site are available, e.g. see Fig 12 in Ireson et al., 2009).

In Figure 7 the differences in latent heat simulated by the two models are shown. Why aren't the actual values shown and compared with observations from the local flux tower that exists at this site through the NERC LOCAR program (e.g. see Roberts, J., Rosier, P., Smith, D.M., 2005. The impact of broadleaved woodland on water resources in lowland UK: II. Evaporation estimates from sensible heat flux measurements over beech woodland and grass on chalk sites in Hampshire. Hydrology and Earth System Sciences 9 (6), 607–613.)? We can infer that the macro configuration results in more evapotranspiration, but which configuration is more realistic? Again, this goes to the heart of whether or not the macro model is an improvement, though I can't comment on this since the results are not presented clearly.

In terms of the catchment scale model application, it is notable that the macro model improves the evaporation estimates, and apparently improves runoff, although it looks rather conspicuous that no runoff plots are included. Why is this?

Corrections to the text

L. 8 Is it really the 'efficiency' of simulations that is the critical limitation?
L. 11 Poor grammar in this sentence, and the meaning is not completely clear. The mass and energy fluxes are influences regardless of whether the hydrology is complex or non-linear. Try to make this sentence more specific and meaningful.
L. 13 I'm not sure this hypothesis is well phrased. It would be very surprising if there was no influence of this representation on the fluxes of water. As phrased, this suggests you are just looking at sensitivity, but I think it would be better to test whether or not this representation results in some sort of improvement? That is presumably what you're actually doing anyway.
L. 17 Change "applied on" to "applied to"
L. 28 "various processes" are there others, other than recharge?
L. 36 The fracture porosity cited (10^{-4}) seems much lower than other published

estimates for the Chalk. I believe Price et al., 1993 actually cite a value of 10^{-2} (though I don't have this reference to hand, please check this).
L. 42 Change "Mathuis" to "Mathias"
L. 55 Change "curve was" to "curves were"
L. 66 What is a "consistent representation ..."? Consistent with what? This sentence restates your hypothesis, which I repeat is very unexciting and somewhat vague. You need to be much more specific here.
L. 100 It is possible, but not certain, that f_m is a sensitive parameter - in which case adopting this arbitrary value without sensitivity analysis or calibration would seem dubious. I'm also not convinced that it makes sense to make the fracture conductivity functionally dependent on the matrix conductivity - is there any physical reason to not to treat these two properties as independent?
L. 167 Here you describe the hydraulic properties of the soil, but you don't described the properties of the Chalk until line 209. Would make sense to rearrange the text so that these are described in the same place inthe text.
L. 188 This is a little bit confusing - it reads like you are saying the hydraulic properties of the soil are uniform over the catchment? However, from Figure 1 and Table 2 this is not the case. Please clarify the text here.
L. 226 I think it is not particularly interesting that the model underestimates the absolute values of the observed soil moisture - I would be more interested in how well it reproduces the changes in soil moisture. It probably underestimates these, but it's not completely terrible. I'm surprised how well this simple default model with no calibration does!
L. 261 In the Ireson al al. (2009) paper referenced here (full disclosure - I am Ireson), Fig 13 shows the drainage flux at 5 m depth, which could be directly compared with Figure 5 in this paper. It can be seen in my paper there was negligible fracture flow at 5 m depth during this period, which is not consistent with the authors interpretation that "fracture flow dominates... during wet periods". So I'm afraid the result here in not consistent with my result - at least the macro model result is not - the default model

might be!
L. 311 Why use R^2 for this? RMSE or bias would be better - we are interested in the absolute values in this case.
L. 336 Again we have references to this weak hypothesis that there is an "influence".
L. 362 Again the word "consistent" - consistent with what?
L. 372 You say these two parameters can be estimated from the matrix without calibration, but this is an assertion, since you haven't tested these parameters in this study. In fact, my central criticism of the findings in this paper are likely due to poor choices of parameter values in your model. If you were to increase your matrix K, I suspect the model would improve.
L. 374 Overall, I cannot agree that the model was able to reproduce the hydrological processes in the Chalk successfully, or even to an acceptable degree. Groundwater recharge is completely wrong.
L. 376 Yes, good that you suggest calibration, but without doing this the model is not ready for publication, since it fundamentally fails to simulate convincing groundwater recharge fluxes. Especially on the 1D model, calibration really is not that hard to do, so must be done before this paper can be accepted, in my view.
L. 401 Delete this final sentence saying you will address coupling with a groundwater model in the future. That is, by definition, outside the scope of this paper, hence irrelevant.
L. 385 + The last three paragraphs of the conclusions are very disjointed.

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