

Interactive comment on “On the Appropriate Definition of Soil Profile Configuration and Initial Conditions for Land Surface-Hydrology Models in Cold Regions” by Gonzalo Sapriza-Azuri et al.

Gonzalo Sapriza-Azuri et al.

gsapriza@gmail.com

Received and published: 12 February 2018

GENERAL COMMENTS

The manuscript by Sapriza-Azuri and co-authors utilizes a well-established one dimensional land-surface model (the Canadian Land Surface Scheme within the MESH) to establish 'how deep does the soil need to be' to appropriately model ground surface temperatures with permafrost to depth, and 'how long do we need' to initialize the climate for the simulation

[Printer-friendly version](#)

[Discussion paper](#)



The main results from this paper are not new, although there are certainly some unique aspects to this paper. First, the fact that one needs very deep soil representation to account for permafrost is well known and unsurprisingly confirmed here. The second contribution of long time-scales of simulation is also not particularly novel, however the authors have conducted the climate simulations in a relatively innovative manner by accounting for uncertainty and variability and providing robust estimates to this.

The paper as it stands requires revision to make an important scientific contribution. While a lot of good work has gone into this paper, it's unique contributions need to be highlighted. Furthermore, there needs to be proper accounting for the site selection and parametrization. It appears that the authors picked the data out of some publicly available archive and ran the simulation with little understanding of realistic boundary conditions. The authors need to carefully consider the surface conditions (vegetation, near surface soils) for this to be an appropriate and meaningful contribution. Norman Wells is not a grassland.

Response to General Comments:

We thank the referee by the constructive nature of his/her criticisms.

To highlight the novelty of our contribution we have restructured the introduction and added more references. We have separated the literature review of previous works into three parts (1) the need for deep soil configuration, (2) the need for proper initialization, including the incorporation of uncertainty and the reconstruction of past climate time series, and (3) parameter uncertainty.

We pointed out in the manuscript the following lines:

"...Some of these facts (deep soil depth, longer spin-up) have already been recognized by the scientific community but, in separate ways. ..."

"... In this study, we address different sources of uncertainty that could affect the model set-up definition (SCD and parameters) and the way that initialization

Printer-friendly version

Discussion paper



is carried out by jointly considering the effect of climate condition, depth of soil configuration, parameter uncertainty and the uncertainty in the reconstruction of past climate variables. ...”

The list of reference added:

- Yang, Z.-L., R. E. Dickinson, A. Henderson-Sellers, and A. J. Pitman. Preliminary study of spin-up processes in land surface models with the first stage data of Project for Intercomparison of Land Surface Parameterization Schemes Phase 1(a), *J. Geophys. Res.*, 100(D8), 16553–16578, doi:10.1029/95JD01076, 1995.
- Rodell, M., P.R. Houser, A.A. Berg, and J.S. Famiglietti, Evaluation of 10 Methods for Initializing a Land Surface Model. *J. Hydrometeor.*, 6, 146–155, <https://doi.org/10.1175/JHM414.1>, 2005.
- Shrestha, R., and P. Houser, A heterogeneous land surface model initialization study, *J. Geophys. Res.*, 115, D19111, doi:10.1029/2009JD013252, 2010.
- Mendoza, P. A., M. P. Clark, M. Barlage, B. Rajagopalan, L. Samaniego, G. Abramowitz, and H. Gupta. Are we unnecessarily constraining the agility of complex process-based models?, *Water Resour. Res.*, 51, 716–728, doi:10.1002/2014WR015820, 2015.
- Bastidas, L. A., T. S. Hogue, S. Sorooshian, H. V. Gupta, and W. J. Shuttleworth, Parameter sensitivity analysis for different complexity land surface models using multicriteria methods, *J. Geophys. Res.*, 111, D20101, doi:10.1029/2005JD006377, 2006.

Regarding to surface conditions, please refer to response to specific comment #5.

SPECIFIC COMMENTS

1. P3 - line 13-15. This is out of place. Unsure as to why it is here.

Thank for your suggestion, we have removed the paragraph.

2. P3 - line 16-17 there is no doubt that deeper soil...’ Yes, this is well established. The question then is why is this work being completed? Additional referencing could be provided as to this.

We appreciate your comment. We have removed those lines from the text and restructured the introduction and adding references. These change look for a better definition of the scope and to highlight the new contribution. Please refer to response to general comments to the reference added and main change in the introduction.

3. P3 - line 25 ‘the depth considered... generally arbitrary’. Can this statement be justified? I find it hard to believe that the work going in to establishment this depth is ‘generally arbitrary’. Referencing would help.

We have removed that line from the text. Please refer to response to general comments to the reference added and main change in the introduction.

4. P3 - lines 30-34. I suggest the authors set up the paper less as a ‘mystery’ and with more direct language in how they are addressing the questions in the paper. I find the set up very colloquial.

Thanks for the suggestion. Those lines were removed from the texts and the introduction restructured. Please refer to response to general comments to the reference added and main change in the introduction.

5. P4 -line 8. The environment here is NOT characterized by grass. What is the influence of this on the simulation? Perhaps it is very little, but regardless, and appropriate upper boundary needs to be established here.

We agree with the referee and regret that the landcover was misrepresented in the original manuscript. Having a large-scale modelling approach in

Printer-friendly version

Discussion paper



mind, the dominant landcover in a pixel of 10*10 km² was named grassland. The confusion here was due to the Land Cover map used in this analysis that came from a reclassification of a land cover map from a bigger area for the Mackenzie basin, where shrubs, grass and other types of land covers were grouped together in a single unit, unfortunately named grassland. In addition, the original pixels were upscaled and only the dominant land cover type was picked.

We fixed this problem in the writing of the revised manuscript. We have corrected the land cover type of this specific location and added a complete description of its vegetation and canopy based on the site description reported in Smith et al., (2004). The analyses and results didn't need to be changed; the reason is as the canopy parameters were perturbed by a Monte Carlo analysis, we have not used a specific land cover type based on a look-up table. The range of variation covered most of possible land cover types present in the area. As an aside, we mention that our analyses showed that regardless of parameter values, a deep soil configuration would be needed in large-scale modelling of cold regions.

6. P5, line 2 - The paragraph starts a bit awkwardly and there is no real justification as to WHY this site was chosen. There is historical data here, but there is elsewhere as well.

We have changed the start of the paragraph as follow:

“Annual soil temperature profiles are available based on the maximum and minimum daily average of soil temperature at several borehole locations in the Mackenzie Valley, administrated by the Geological Survey of Canada (Smith et al., 2004). . . .”

The selection was made on the availability of data and, of course other places could be selected. As future work the plan is to generalize to other

[Printer-friendly version](#)

[Discussion paper](#)



locations as was pointed out in the conclusion.

7. the "Back to the past" language is again colloquial. I am not sure that this type of phrasing will be adopted in the scientific community and I would suggest the authors adjust their language to be one that is more technical.

Thanks for the suggestion. We have changed 'Back to the past' to "Paleo-Reconstruction".

8. Figure 4 is nicely set up and I am wondering if Table 1 can be described in a more technical way or in a figure format as it is repetitive and as a reader not particularly helpful. There is an obvious sequence here than can simply be described.

We have changed Table 1 to a figure format. The Figure 5 has the model discretization.

9. I am unsure as to how the parameters in Table 2 were given their upper or lower bounds. Yes, there was a Monte Carlo sampling with a uniform distribution, but LAI, minimum LAI, albedo, etc., to me seem as if they are incorrect for the environment. Please more carefully consider the rationale for this parametrization scheme and provide the reader with an understanding as to which one of these parameters is the most important for the setup and simulation.

The rational here was to have more flexibility in the parameter range so, the result could be more robust about of what does matter in norther places (climate, soils depth or parameters). The parameter range cover mainly most of the land cover presence in that area from.

To clarify this point we have added the following lines... "... The range of the canopy parameters values represent different vegetation cover that are present in the area based on the look-up table from CLASS user manual (Versegey, 2009). ..."

Printer-friendly version

Discussion paper



10. P10, line 3. Please provide a reference to the end of the first sentence

We have added the following reference: Yang, Z.-L., R. E. Dickinson, A. Henderson-Sellers, and A. J. Pitman (1995), Preliminary study of spin-up processes in land surface models with the first stage data of Project for Intercomparison of Land Surface Parameterization Schemes Phase 1(a), J. Geophys. Res., 100(D8), 16553–16578, doi:10.1029/95JD01076.

11. I have no real issue with the presentation of the results. As mentioned, a lot of thought and time went into the setup here and certainly a lot of computational resources were applied. I do, however, encourage the authors to highlight their scientific contribution here. The result of deeper soil configurations has been well defined for over a decade (or longer?) now. I believe that there is more value in exploring (appropriate) parameter sensitivity and the generation of relevant climate conditions. There were a lot of realizations here, but I am not sure that the authors have detailed the importance of these runs. What clear guidance can the authors provide other groups working in cold environments.

We appreciate your comment. We have restructured the introduction in a way to better highlight the main contribution of this work. Please see response to general comments.

We have added to the Discussion and conclusion section the following lines in relation to reconstruction of past climate time series:

"... . An important remark here is that the effect of stochasticity in the reconstructed time series is minimal, so what is important is to reproduce historical (low frequency) trends. ..."

The recommendation are detailed in the Discussion and Conclusion section and they are:

- (a) **Minimum soil depth of 20 m**

[Printer-friendly version](#)

[Discussion paper](#)



(b) **Initialization in two stages:**

- i. **First stage spin-up using a single average year to reach quasi-equilibrium condition on fluxes and state variables.**
- ii. **Reconstruction of past climate time series, to allow the model evolve over time on the time period preceding the period of records as to be able to simulate current conditions.**
- iii. **Recognize the parameter uncertainty.**

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-614>, 2017.

Printer-friendly version

Discussion paper

