

Interactive comment on “On the Configuration and Initialization of a Large Scale Hydrological Land Surface Model to Represent Permafrost” by M. E. Elshamy et al.

Anonymous Referee #1

Received and published: 30 May 2019

General comments

Elshamy et al. detail testing and resultant guidelines for the configuration and initialization (especially ‘spinning up’) of permafrost in large-scale hydrologic models, with a focus in this study of the Mackenzie River Basin. Permafrost exerts primary control on hydrologic routing in cold regions, and thus this topic is critical for Canada and other countries with high latitude regions that are experiencing high rates of warming. As such, the manuscript scope is a good fit for HESS, and the collective authorship team offers many decades of modeling experience and insight. I think the paper will be a useful contribution to HESS, but I think it needs some reworking.

We would like to thank the reviewer for the time spent to carefully review our manuscript. We greatly appreciate the important points raised. We present our response to reviewer’s comments below. The reviewer comments are listed below in regular black text, and our response in regular blue text. Some of the reviewer’s suggestions have been addressed in the revised manuscript under preparation while other responses point towards what we intend to do further in the manuscript.

Major concerns

1. This is a vague concern, but this comes across as a bit of a high-end technical report in places, more than a research paper. Rather than detail why that is, I list a few specific concerns below that map to this general overarching theme.

While we appreciate this comment, this manuscript was written directly as a research paper. Hopefully by addressing the specific concerns below, in addition to the suggested revisions to the manuscript, this concern will be also addressed.

2. This discussion on changing annual discharge is a bit overly simplified. I’d break this down a bit more into different seasons. There is a pretty consistent increase in minimum flows across the pan-Arctic (see, for example, the recent ECCC report, Canada’s Changing Climate, or Walvoord and Striegl 2007 GRL, St Jacques and Sauchyn 2009 GRL, Duan et al. 2017 Water – for China)

Thanks for pointing out the importance of seasonal changes to streamflows and for the relevant literature. We intend to revise the discussion on that to reflect the complexity of streamflow response due to differences in seasonal changes based on the suggested literature.

3. The intro is quite long – it is 6 paragraphs, of which several are long. Also, the objectives section which follows is normally embedded in the intro in most papers. This would add about another 2 paragraphs. This needs to be trimmed. Paragraph 4 is especially wordy. Paragraphs 5 and 6 could be cut by 50%.

Thanks for the suggestion to shorten the Introduction & Objectives sections. We intend to revise both sections and remove any unnecessary text to make it more focused.

4. Lists or bulleted sections are not written very parallel in this paper, and they are hard to relate and read. L155-168, L459-467, and L686-691 are examples.

Thanks for the suggestion. We have noted the issue and intend to rephrase the bullets to make them parallel.

5. L205-210, this is very late in the paper to be delineating the focus

The focus was already given in the objectives, especially L122-128. L205-210 were giving the rationale for selecting the sites. However, we are considering shortening Section 3.2 as per your next point and as requested by Reviewer 2. Therefore, the text on L205-210 will be revised as we revise the Introduction and Objectives sections.

6. Because there are three sites, the site description is very long (Sections 3.2.1, 3.2.2, and 3.2.3. This takes up about 7 pages, which is similar in length to short paper on its own. Basically, some of this information (especially the inordinate focus on parameterization, when that is not the point of the study) needs to be moved to an electronic supplement. It detracts from the key messaging, and it's not a very invigorating read. I think the site description is key, but could be shorter, but I don't think the reader needs to wade through endless parameter justification, which could be built into tables in a supplement for interested readers.

Thanks for the suggestions. We agree that Section 3.2 has become too lengthy and we intend to move most of the text into a supplement and keep only relevant parts. This is also suggested by Reviewer 2.

7. Oct. 1979 is certainly late in history as a representative climate from which to base the model spin up. I realize this is briefly addressed later, but I suspect a permafrost modeler would object.

We agree that 1979 may be considered late to start model spin-up for permafrost. However, we discussed that in Section 5 – paragraphs 3-5. Previous work at Norman Wells (Sapriza-Azuri et al., 2018) showed some sensitivity of permafrost conditions to the spin-up year but only if a warm year is selected (Figure 12 in the above mentioned paper). Based on that, the authors suggested to use an average year for the spin-up. We checked that the selected hydrological year (Oct 1979-Sep 1980) is close to an average year based on available records (see L440). We intend to show that in the revised manuscript. There are severe logistical problems in using a longer period. One has to use another climatic dataset to use earlier years as WFDEI only starts in 1979. This means that alternative climatic forcing datasets have to be used and this will have impacts on the results, introducing considerable additional uncertainty. The selected year is performing well for most aspects of our simulation and is resulting in a colder rather than warmer temperatures for the minimum envelopes. The discussion around this point will be strengthened in the revised manuscript.

8. I think a small section on the thermal physics in the model (governing equations, soil freezing curves if any, etc.) would be far more useful to the reader than the emphasis on parameters.

Thanks for the suggestion. We will have that in the revised manuscript.

9. This contribution is very qualitative and even anecdotal in places. For example ‘seems’ should up 7 times in the manuscript, while ‘seem’ shows up in 8 places. The difference in model runs are not compared via standard metrics like RMSE or something like that. The discussion seems to rather focus on apparent discrepancies and vague explanations. For examples of this, just consider any section on model comparisons or differences. Also, note recurring appearances of ‘much more’ and ‘too small’ – a few actual numbers would be nice.

Thanks for the suggestion. We relied on visual comparisons to assess differences amongst the different simulations. To fully address this comment, we intend to calculate a few standard error metrics to strengthen the visual comparisons by interpolating temperature envelopes at the depths of available observations (which varies from site to site and year to year). In the revised version, we will use some actual numbers, while acknowledging the high uncertainty level in both observations and simulations.

10. The authors do not frame their permafrost modeling results in the discussion around past contributions. Cryosphere scientists have been modeling permafrost and considering spin up scenarios for a very long time. The authors’ work is new and interesting (especially the focus on the inclusion of permafrost in large-scale hydrologic modeling), but the thermal physics under consideration are not overly new, and it would make sense to relate their study findings.

Thanks for pointing out this and for describing the work as new and interesting. We intend to revise the discussion to compare the results to other relevant studies.

Minor concerns

Many of these are quite trivial

Thanks for helping us improve the manuscript by taking the time to point out these.

L13, comma after ‘average’ L33, shouldn’t basin be capitalized here as elsewhere when preceded by Mackenzie or Mackenzie River?

Changed as advised.

L33, ‘heating up by 4 degC’ . . .over what time period? 100,000 years? 50 years?

Revised to: “... by 4°C between 1948 and 2016.”

L36, ‘American rivers’ should just be ‘America’ and the subsequent semicolon should be a comma

Revised as advised.

L75, ‘implied’ should be ‘inferred’

Revised as advised.

L119, ‘In addition to: : .for spinning’ is a fragment

Removed as it was not addressed in the paper.

L143 and elsewhere, 'etc.' occurs in the paper where it is entirely superfluous in a couple places. It tends to look choppy – 'such as' is sufficient.

Removed in L143 and L117 in the revised manuscript.

L141, Land does not need to be capitalized

Capitalization removed.

L152, does this mean that sandstone thermal properties are always used the for the bedrock conductivity everywhere? This seems less than ideal.

Well, we agree it is not ideal, but this is how it is implemented. We have added it to the discussions as a potential improvement to CLASS.

L161 'thus we use a thaw, rather than a freeze criterion' – I have no idea what this means

"The active layer thickness is defined as the thickness of the layer that is subject to annual thawing and freezing in areas underlain by permafrost" (van Everdingen, 2005). "Strictly speaking, the active layer thickness is defined as the lesser of the maximum seasonal frost depth and the maximum seasonal thaw depth" (Walvoord and Kurylyk, 2016). The maximum frost depth can be different from the maximum thaw depth. In case the frost depth is less than the thaw depth, there is a layer above the permafrost that is warmer than 0°C but is not connected to the surface (a talik). Because active layer observations are usually based on measuring the maximum thaw depth, we adopted the same criterion when calculating active layer thickness in the model.

L167, This is more commonly called the 'seasonal penetration depth', at least in nonpermafrost regions

We do not disagree with the reviewer on the terminology. However, the work is about permafrost. Therefore, we revised the manuscript to use the more standard term – Depth of Zero Annual Amplitude (DZAA) depth as suggested by Reviewer 2.

L170, permafrost is not defined cryotically like this (frozen vs. unfrozen). It's a temperature definition – i.e. ground below 0C for two or more consecutive years – see, for example, Dobinski, 2011, Earth Science Reviews.

Thanks for pointing this out. We revised the text accordingly and added references.

L173, "MESH/CLASS used to output" should change to 'Prior versions of MESH/CLASS outputted merely temperature profiles' or something like this

Revised to "Prior versions of MESH/CLASS merely outputted ..."

175, 'A CLASS typical' should be 'A typical CLASS'

Revised to "A typical CLASS ..."

L192, “these has’ should be ‘these have’, and I’m not sure what ‘to be carried back to the MRB scale’ means

We corrected the phrase to “... these have to be applicable at the MRB scale” to address both parts of the comment. The aim is to establish a methodology that is applicable to the large scale rather than finding the best configuration for the selected sites if it cannot be implemented at the large scale.

L197, ‘North West’ should be ‘Northwest’

Revised as advised.

L216, ‘with’ should be ‘by’ and the rest of this sentence needs to be rewritten as it is confusing what it means

We revised the sentence to read: “The basin is located in the sporadic permafrost zone where permafrost underlies few spots only and is characterized by warm temperatures (> -1°C) and limited (<10m) thickness (Smith and Burgess, 2002)”.

L220-221, Weather and North should not be capitalized

Revised as advised.

L302, should not be semicolon

Revised as advised.

L304, why does deep permafrost imply no groundwater? It would make more sense to note that the cold climate prevents the formation of a lateral talik, and thus there is no perennial shallow groundwater. See Lamontagne-Halle et al. 2018 (Environmental Research Letters) or Connon et al. 2018, JGR-Earth Surface.

Thanks for pointing out this. We will be revising the text accordingly based on the provided references.

L316 (and elsewhere, do a word search), ‘envelops’ should be ‘envelopes’

Thanks for pointing this out. We checked the manuscript for all instances and corrected accordingly.

References

- van Everdingen, R. O.: Glossary of Permafrost and Related Ground-Ice Terms., 2005.
- Sapriza-Azuri, G., Gamazo, P., Razavi, S. and Wheeler, H. S.: On the appropriate definition of soil profile configuration and initial conditions for land surface–hydrology models in cold regions, *Hydrol. Earth Syst. Sci.*, 22(6), 3295–3309, doi:10.5194/hess-22-3295-2018, 2018.
- Smith, S. L. and Burgess, M. M.: A digital database of permafrost thickness in Canada., 2002.
- Walvoord, M. A. and Kurylyk, B. L.: Hydrologic Impacts of Thawing Permafrost—A Review, *Vadose Zo. J.*, 15(6), 0, doi:10.2136/vzj2016.01.0010, 2016.