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Interactive comment on "Improving permafrost physics in the coupled Canadian Land Surface Scheme (v. 3.6.2) and Canadian Terrestrial Ecosystem Model (v. 2.1) (CLASS-CTEM)" *by* Joe R. Melton et al.

Anonymous Referee #3

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In this paper the authors test and evaluate a wide range of improvements to permafrost physics in the CLASS-CTEM land surface model (which is part of the Canadian Earth System Model, CanESM). While there is nothing especially ground-breaking, this is a comprehensive and thorough assessment bringing together many different, disparate developments into a single framework and I believe is worthy of publication.

CLASS-CTEM consists of two components: broadly, CLASS does the physical calculations and CTEM performs the carbon cycle calculations. The authors improve the simulation of permafrost physics in CLASS-CTEM with a series of model developments

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Discussion paper

which are successively evaluated (against multiple observations) and discussed. The default soil scheme has only three vertical layers which leads to a poor simulation of permafrost dynamics. Therefore, very reasonably, the first improvement extends the soil column and adds more layers. A moss layer is added to the surface, as this has previously been shown to improve simulation of soil temperature and freeze/thaw dynamics. Furthermore, the authors experiment with the depth to which the water can penetrate in the soil, and the impact of different driving data sets. Having established a baseline simulation based on what they consider to be these essential improvements, the authors then test a number of further developments in the representation of snow, hydrology and heat transfer. This includes, for example, allowing the presence of liquid water below zero degrees celsius ('supercooled' water), as in real life soils. Of the developments tested, this is considered to make the greatest improvement and therefore to be incorporated into the standard model version. The final simulation of permafrost by CLASS-CTEM is an improvement on the initial simulation, although the capacity for evaluation is somewhat limited by the disparity between the extremely large grid cell size in comparison to site-level observations.

While this paper is relatively clear and well-written, I believe that it can and should be significantly improved prior to publication.

GENERAL COMMENTS

- In terms of experiments included, since de Vries thermal conductivity is considered to be physically unrealistic, I don't see the need to include this experiment.

- The paper is a bit too long and the clarity could be improved. The statistics don't need to be written out in so much detail in the text. As an example, the section 3.11 for 'modified hydrology' could be reduced to something along the lines of: "This development generally reduces water mobility (eqs. A35/A36), resulting in wetter soils, which in turn leads to a significantly deeper ALT but minimal impact on soil temperature errors (see Figures 2,4 and 5). Overall it increases the spring runoff and reduces winter

runoff, which degrades model performance compared to runoff observations (Figure 9). Therefore, this did not provide any significant improvements." [of course this could be better written, just a suggestion for what info to include -ie numbers not necessary in text, and only including the most crucial points] I would then also suggest adding a comment on the process and why it doesn't work, for example my initial thought might be that while it may be a more realistic parameterisation, the fact that the model does not account for macropore flow / large scale cracks and defects in the ground means that the default parameterisation may be providing a compensation for this.

In general I would like to see more reference to the processes and to the direction of the biases, rather than to absolute error, as this gives clearer information about the processes and why each development is having a particular effect. I might suggest to include a directional error in the error tables (bias in annual mean?) as well as just MAE.

- I don't think detailing the heat flux and LAI for each experiment is helpful to the main message. You could summarize heat fluxes and LAI as a separate section, for example noting that in warmer, wetter simulations, the LAI is generally larger.

- In Section 3.13 I suggest adding a further comment on the potential ALT biases. I have noticed from using ALT data that the maximum that depth in the datasets is sometimes not the actual end of season maximum because the field campaigns have not continued right until the end of the season, so this could be the cause of apparently discrepancy between soil temperatures and ALT's.

- It appears that most of the developments are detailed in the appendix, but it doesn't appear to include details of the moss parameterisation. Please add something about the changes to thermal/hydraulic parameters for reproducibility (apologies if I missed this).

- I would expect 'mean absolute error' to be higher for the air than for the soil, because variations in air temperature are typically much larger (both seasonally and on short

timescales). Therefore I don't completely agree with the argument on page 27 lines 3-9.

FIGURES

I find Figure 3 quite confusing and not entirely helpful. It is not clear what either of the axes are: The residual I guess is obs-model, but it's not specified in the text, and it's not at all clear what the x-axis is showing. Is this the observed ALT? If so I think it would be better, and show the same information, if you just to plot model ALT against observed ALT. I am also not entirely convinced by all the discussions that relate to that figure in terms of 'biases with depth', since there are altogether not many points on the plots and the trends do not appear to be very strong. Perhaps these discussions could be removed or modified to focus on the more significant impacts of the developments. For example, the 20 layer simulation has in general too deep ALT- would be enough information.

Figure 7 doesn't add much. We can see basically the same thing but more clearly on Figure 4. If it is to be included, I would suggest in the appendix or supplementary.

Figure 11: I am confused as to why the simulation with moss is warmer than the simulation with no moss. Are these labelled correctly?

MINOR COMMENTS

" with seasonal wMAE values for the shallow surface layers of the revised model simulation at most 1.2 âÛęC greater than those calculated for the model driving screen-level air temperature compared to observations at the sites" - This sentence is difficult to parse. Perhaps by removing the last part ("compared to observations at the sites"), it would make more sense.

Page 7 line 14: How is the 1851-1900 part different from just doing two more spinups? Is the CO2 varying? CO2 forcing dataset is not mentioned but should be included - please add.

Page 8 line 10: "filtered out"- not sure what that means? Does it mean removing the same values from model as are missing in observations?

Please include how you define permafrost presence in a grid cell (again, apologies if I missed this).

Page 9 line 31. 'more grid cells' -> 'additional grid cells' (for clarity)

Page 13 line 7-8. I would expect the absence of water and latent heat to make active layer too deep, because latent heat suppresses the seasonal cycle. But since latent heat works equally in both directions (applicable both in freezing and thawing periods), I am not convinced that its absence would lead to warmer soils. Indeed I am not seeing a major bias in soilgrids vs 20 layers on Figure 6. Perhaps reconsider this sentence.

P 13 line 25: "accurately" -> "accurately simulating"

P 15 line 17-18. Soilgrids has extremely deep soils in West Siberia/Urals: Can you find some reference or ask someone who's been there whether this is at all realistic? Having claimed this is a better validated dataset it would be helpful to provide evidence of this.

P17 line 15-16. Cold bias due to too much moss is a reasonable assumption, but coupled with the fact that there doesn't seem to be a major warm bias without it (fig 6), and that you would overall expect observations to be biased cold (as written in 3.13), I am not totally convinced here. More consideration of processes and what is missing may be helpful.

P29 line 21-22 "thus excluding the simulation of taliks". I am confused by this statement. A talik could be simulated in a 1D model. Discontinuous permafrost, on the other hand, couldn't be. Please could you either clarify or rephrase.

Hope you find these comments helpful to improve an already good manuscript.

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