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Interactive comment on "Change in Frozen Soils and its Effect on Regional Hydrology in the Upper Heihe Basin, the Northeast Qinghai-Tibetan Plateau" *by* Bing Gao et al.

Anonymous Referee #1

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General comments

Gao et al. present a very impressive study that investigates how changing climate conditions influence hydrologic processes in a cold environment (particularly the Heihe basin in the Qinghai-Tibetan Plateau). Their model enhancements include algorithms for considering frozen ground processes. As such, they address how the hydrologic system responds to forcing (changing precipitation) but also to the changing system characteristics themselves (i.e. type and distribution of frozen ground). This study certainly has merit for publication in TC and has potential to be an important paper in this field. There are a few issues that I think should be considered prior to publication. Most of these are relatively minor, but addressing them all will likely warrant a major

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revision.

Major comments

1. I think the introduction should be improved. (1) In the first few paragraphs, the authors go from regional to global and back to regional. I find this confusing. It would be preferable to start global and then narrow down to the Qinghai-Tibetan Plateau. It should be rewritten. (2) There are insufficient references, especially in the second half of the first paragraph. Many of those statements are not self-evident and should be backed up by more studies. I recommend the authors consider the already cited Walvoord and Kurylyk (2016, VZJ) review and references therein to back up these claims about how frozen soil and its evolution influence hydrological processes.

2. There are quite a few places where English issues occur. In general, the manuscript is in pretty good shape though. I do note a number of specific English corrections below in my minor comments.

3. I'm curious if the authors have any citations to provide for the GBEHM model other than the Yang et al. 2015 paper published in a Chinese journal. The description in Section 3.1 is a bit brief, and there is no English source to go to for more information. One question: how are heads calculated in the aquifer since Darcy's Law drives GW-SW exchange?

4. Equation 4. I'm a bit confused on the snowmelt equation, but I'm more used to frozen ground modeling. Why does the latent heat term only allow for ice to water transition and not snow to water? Is this ice referring to firn? This should be more explicit.

5. It is stated that Equations 4 and 8 are solved by the 'finite differential method'. Do the authors mean 'finite difference method'? If so, what method? Are the spatial derivatives solved as forward, backward or centered finite difference? Please state in the text. Is the time derivative implicit or explicit finite difference? Is there a Crank-Nicholson scheme employed? Just one or two more sentences will suffice.

6. The authors would do well to include a figure of their modeling domain – at least their vertical discretization and boundary conditions. I find this lacking.

7. I realize this contradicts the statement above, but this is a bit long of a paper and there are many figures! I went through them and found them mostly valuable, however. I do think that Table 5 could be cut without much of a loss and Figure 14 could be as well. If the authors are in love with these, they could move them to an electronic supplement.

8. I'm confused at how the authors formed permafrost in 50 year spin up run (e.g. L297 and elsewhere). I've modeled even thin permafrost using 1000 year spin ups. Can the authors please describe this better? I wonder if this lack of permafrost formation time might partially cause some of the poor fits seen in Figure 2. Related to this, I wonder if the fits in Figure 2 in the shallow zone would look better at another time in the year (which the authors likely don't have data for). In other words, it could be the seasonal dynamics that are off, not the multi-decadal dynamics.

9. L330 and surrounding text, the authors suggest that RMSE decreases with increasing depth in Figure 3. It looks to me however (somewhat counter to what intuition would suggest) that the errors are increasing with depth (simulation – observation in Figure 3c goes to dark blue at the bottom indicating higher error and is white, indicating low error, higher up).

10. The model results are impressive in Figure 4 (no small feat to get seasonal thaw and frost depths so well captured) and Figure 5 and Figure 6, so I commend the modelers on this.

11. L368. The authors should explain why the warming rate is higher in the shallower soils. It has to do with the surface signal arriving as a 'thermal breakthrough curve' that is retarded at greater depths due to the thermal inertia (sensible and latent heat storage) of the subsurface.

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12. I don't understand the point of presenting some of the correlation information. It is tautological to say that frost depth and thaw depth are correlated or inversely correlated to mean annual air temperature (L398-402). Along these lines, Table 5 is also not that useful as already stated. L463-470 is also a waste of space. You don't have to talk about correlation when you are using a physically-based model. Talk about physical processes!

13. L429 and elsewhere, the authors talk about subsurface flow a bit confusingly. Is this groundwater flow? Or is it groundwater flow plus lateral flow in the unsaturated zone? In reality all the precipitation typically makes its way to the river via subsurface flow paths as Hortonian overflow is very rare except in urbanized watersheds with impermeable pavement. They should be a bit clearer if they are talking about groundwater or what. L479 says that higher moisture increases conductivity and thus subsurface flow. True. But again, what flow? If they are talking about groundwater flow, they could have higher lateral transmissivity, but that effect wouldn't be captured in their model, I don't think.

14. L600-601, is it possible that the increased liquid groundwater storage came from phase change of ice to water, rather than increased recharge? Couldn't the authors just directly determine the recharge from their modeling rather than making inferences based on groundwater storage?

Minor comments

L20, insert 'a' before 'regional'

L47, Comma after 'soils'

L52, delete 'balance' after water

L59 and 507, The Walvoord et al. (2016) study (by the way there are no cited Walvoord papers with more than two authors) should really be Walvoord and Striegl 2007 or one of her related studies as these are the original field studies.

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e.g. Walvoord and Striegl 2007. Increasing groundwater to stream discharge from permafrost thawing in the Yukon River basin: Potential impacts on lateral export of carbon and nitrogen. Geophys. Res. Lett.

L60, The authors miss a very recent study in NE China that is certainly related to the present study Duan et al., 2017, Increasing winter baseflow in response to permafrost thaw and precipitation regime shifts in northeastern China. Water, 9(1).

L61, The authors suggest a 'few studies' argue this point, but only provide 1 citation. Either provide more citations, use 'e.g.' in the citation, or reword this slightly.

L62, These studies typically included long term hydrological (not just meteorological) data

L63, Change 'might lose' to 'obscures' or something like this

L79, 'the complex landscape' is vague – explain.

L80, delete 'the' after 'simulates' and provide a citation for the GEOtop model (e.g. Endrizzi et al.)

L83, insert 'with the inclusion of freeze-thaw' after 'improved performance', otherwise the sentence is a bit unclear.

L86, insert 'the' before 'global'

L87, Change 'The' to 'These

L90, Change 'were inadequate' to 'are lacking' which seems less of a personal attack

L93, Change 'Different from' to 'In contrast with'

L97, Delete 'the' after 'soil,'

L112, Delete 'the' after 'variations of'

L116, Change 'insufficient' to 'lacking' or something like this. This indicates that the

studies are lacking in number not in quality (which insufficient could imply).

L116, Insert ‘an’ before ‘integrated’

L131, Start a new sentence after 1009 km2. Change ‘it supplies’ to ‘. The upper reaches supply’

L133, Insert ‘the’ after ‘dominates’

L134, Insert comma after ‘(Wang et al., 2013)’ as this is a coordinating conjunction connecting two independent clauses

L167, Delete ‘which is’ and insert comma after ‘2013’

L173, delete ‘and’ after T3

L196, delete ‘of’

L197, Insert ‘and’ after ‘glacier,’

L201, Should ‘unit’ be inserted after ‘storage’??

L206, Please provide original citation for Horton-Strahler scheme not Yang papers.

L250, Is it very unusual to represent hydraulic permeability with a capital K. Usually this nomenclature refers to hydraulic conductivity. Recommend the authors use little k.

L260, The authors use the term heat capacity on L232 to refer to a volumetric heat capacity and on L260 to refer to a mass-based heat capacity. To be more consistent, the authors could refer to the mass-based heat capacity using its common term ‘specific heat’

L262, insert ‘vertical’ after 1D to be clearer

L266, insert units after saturated soil hydraulic conductivity

L303, What is ‘groundwater conductivity’? Hydraulic conductivity?

L353, ‘value’ should be plural

L360-361, this last sentence is not needed. The authors should not make statements like this. Allow the reader to come to their own conclusion on the suitability of the model.

L368, insert ‘initiates’ after ‘thawing’

L374, Delete ‘of’

L377, insert comma after ‘seasons’

L497, Insert coma after ‘season’

L500, Insert comma after ‘low’

L509, Change ‘lacked of’ to ‘did not consider’ or something like that.

L511, delete ‘the’ before ‘increased’

Section 5.3: The authors talk about uncertainty and how some processes can lead to overestimation of thaw. They should consider processes that can also lead to underestimation. For example, in warm permafrost environments, where permafrost coverage is discontinuous, and in complex terrain, lateral thawing can be very important and can accelerate thaw. Suggested studies are below. The heat transfer in the present study is only vertical and cannot accommodate lateral permafrost thaw. The authors should acknowledge this.

Noetzli et al. 2007. Three dimensional distribution and evolution of permafrost temperatures in idealized high-mountain topography. J. Geophys. Res.

Sjoberg et al. 2016. Thermal effects of groundwater flow through subarctic fens: A case study based on field observations and numerical modeling. Water Resour. Res.

Kurylyk et al. 2016. Influence of vertical and lateral heat transfer on permafrost thaw, peatland landscape transition, and groundwater flow. Water Resour. Res.

In this section on uncertainty, the Wu et al. (2016) paper deserves a citation as the modeling was similar and the paper was focused on uncertainty.

Wu et al. 2016. Constraining parameter uncertainty in simulations of water and heat dynamics in seasonally frozen soil using limited observed data. Water 8(2):64, doi:10.3390/w8020064

L580-581, 'on the high and cold plateau' is confusing and sounds odd

L588, insert 'sourced by' after 'mainly'

Figures are mostly very good. Figure 10 caption: Change 'at' to 'on' in both instances. Would it be possible to just plot the distribution of the land area that changed from permafrost to seasonally frozen ground?

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