Manuscript: Improved modelling of Siberian river flow through the use of an alternative frozen soil hydrology scheme in a land surface model, by Finney et al.

This study by Finney and others investigate the impact of hydrological parametrizations derived from SIMTOP and enriched by a representation of heterogeneous soil freezing at the model grid-cell scale, on hydrological modelling in the Arctic. It further highlights a major concern for modelling at high latitudes, namely the (lack of) reliability of snow forcing data.
The introduction, as well as the conclusion, are very thorough, detailed and explanatory. Besides, the manuscript has the significant merit of identifying gaps in the literature and trying to reduce them, which is often hard and tedious. For instance, the discussion on the constrains of the model parameters f and α is very useful.
I find the manuscript acceptable for publication, pending some revisions.

Major remarks

Of the two developments already implemented in JULES based on the work by Niu and Yang (P312 L.9), the TOPMODEL approach benefits from a quite explanatory description while the standard JULES frozen soil hydrology and supercooled water aspects are left out. I think the description of the influence of FPA on the existing parameterizations in the model (section 2.2) would benefit from a short introduction on how θ_{ice} is calculated, how soil freezing affects hydraulic conductivity in the standard JULES, and saying that the Richard's equation is used to calculate vertical soil moisture fluxes. Such a description would favourably replace the description of the snow model by Essery et al. (2003) in P.313, which is not very useful for the understanding of the paper and diverts from its main scientific focus. The way the new FPA parameterization builds on, co-exists, or does not coexist, with the standard JULES frozen soil hydrology (described in Best et al., 2011, and initially based on Cox et al., 1999) should be clarified. For instance, equation (4) describes a hydraulic conductivity based on the total water content (frozen + unfrozen) and weighed by a mean permeable fraction ; does it mean that the hydraulic conductivity calculated on the basis on unfrozen soil water content only, as in Cox et al. (1999), is never used in JULES-SIMTOP or JULES-SIMTOP-FPA ?

P.315 equation 4: I think this parameterization choice deserves some more justification: for instance, one could argue that the soil layer with the greatest FPA is limiting in terms of water fluxes and use (1-max(Ffrz)) instead of an average FPA in equation (4).

Minor points

1. The content of the manuscript could easily fit into a classic "Introduction/Model & methods/results/discussion" outline and using it could help the reader find the information looked for.

2. A clear, possibly itemized, description of (i) standard JULES, (ii) SIMTOP, (iii) TOP, (iiii) NEW should be made at some point, and a single denomination used from there on, to avoid possible confusion (for instance P.318 L13: "enhanced model run" is not fully explicit) or redundancies (P326 L13 : "SIMTOP which alters some aspects of TOPMODEL")

3. I would encourage a closer look at the punctuation, the authors seem to have a special fondness for semicolons and just hate colons (eg. P310. L. 18; P. 314 L.10; L.16, 18; P.321 L7…).

4. further comments/suggestions:

P310. L. 15 : “By altering absorption and hydraulic conductivity, permeability is reduced.” “Absorption” is a bit vague in this context, it could also be water absorption by roots… Could you be more precise (soil absorption) or use another wording ?

P310. L.19 : millions.

P311. L.20 : “However, the Clapp and Hornberger pedotransfer functions (Clapp and Hornberger, 1978), commonly used in GCMs, produce a hydraulic conductivity that increases in a non-linear manner.” I would rather emphasize that the natural phenomenon is highly nonlinear, hence the human-made parameterization choices which try to account for this reality.

P.312. L.7-9: the reference is Niu and Yang, 2006. I would rather state “The FPA is calculated as an exponential function of the fractional ice content, which increases the infiltration rate when compared to a linear parametrization”. Even though the authors’ original sentence comes directly from Niu and Yang (2006), the message looses clarity when out of context.

P.312 L10: possibly citing the references (maybe just Best et al., 2011 and Clark and Gedney, 2008) already there.

P.312 L 14: a word is missing there !

P.314 L10-13: though it is implied, I would still specify that surface runoff is compulsorily affected by those modifications.

P. 314 equation 1: equation (1) could specify $F_{frz,i}$ for more clarity.

P. 315, 316: Possibly create a new section (“2.3. sensitivity to parameters and calibration”).

P318 L4: Rivers -> rivers

P318 L10: development -> developments.

P.319: maybe just briefly specify how the lag time introduced by TRIP is calculated (based on topography, river length … etc)

P.319 L.10-15: part of the difference in the winter baseflows originates from different degrees of freezing of the basins and rivers (e.g. Sergutin and Turutin, 1983) which should be mentioned as you insist on this point.

P.319 L19: it does not appear clearly that you checked this hypothesis (higher saturation in parts of the basin) in your model outputs. If it is the case, this should be made clearer. Here again, knowing a bit more about frozen soil hydrology in the CTRL and the TOPMODEL versions would be enlightening.

P.320 L17-18: what is at stake when looking at the total soil water content is briefly mentioned at the end of the paragraph but this aspect could be developed a little bit more and put in the current scientific context. For instance, changes in the Lena basin groundwater storage have been identified by GRACE data (Muskett and Romanovsky, 2009) and remain partly unexplained. I fully agree with the authors that understanding the physical reasons for such changes is a crucial step for model developers.

P.320 L.29: what about topography? Figure 6 suggests a major decrease in the parts of the Lena basin with lowest elevation and rather flat lansdcape…

P.323 L13: additional sources : could you briefly state which ?

P.323 L23: incorporation.

P.324 L17: (Roesch, 2006) -> Roesch (2006)

P.325 L6-8: the different timing of snow melt between the upper and lower parts of the basins have also to be considered, though it does not change your conclusion.

Figure 2 (in link with text p. 316): the dependency in f seems weird: for Ffrz = 0, one would expect from eq (2) that Fsat is a decreasing function of of f. Please check or correct me.

Figure 3: why use MODEL A, B, C in the figure, and not just simply “standard JULES”, “TOP”, and “NEW” ?