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Interactive comment on "Impacts of microtopographic snow-redistribution and lateral subsurface processes on hydrologic and thermal states in an Arctic polygonal ground ecosystem" *by* Gautam Bisht et al.

Anonymous Referee #2

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This is a well-written paper that described in a very clear and clean manner small-scale model simulations of the impact of snow redistribution and lateral subsurface processes. The model is set up on a well instrumented study site and the results are comprehensively analyzed and honestly discussed. The results are clear and allow for an unambiguous prioritization of effects to take into account in lager-scale simulations (snow redistribution seems to be much much more of a pressing issue than 2D or 3D soil physics). This paper clearly deserves being published, but I have some concerns about the modeling strategy in general that should be addressed in a revised version.

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Discussion paper

I also have some specific remarks and comments.

General remark. The framework of the paper is Earth System modeling. The authors implement small-scale snow redistribution and 3D soil physics (2D in the setup used here). The results show that a simple snow redistribution parameterization based on microtopography has a very beneficial effect on a range of simulated variables. This is very nice. However, I think that the paper almost entirely misses a thorough discussion of an implementation strategy for these development in the ultimate context of Earth System modeling. This will happen on much larger spatial scales. How will you move from an explicit fine-scale representation to a sub grid implementation? Will the choice be only to include snow redistribution (i.e. aren't there already enough results to decide that a 3D soil physics will be an ÂńÂăoverkillÂăÂż in the Earth System modeling context)? Will the model have two tiles (polygon centers and rims), with snow being shuffled from one tile to the other? Or is the whole thing probably going to be more complex, with an explicit modeling of 3D soil physics supposing an idealized polygon of some finite size? What will be done if the model domain does include areas that are not polygonal tundra (it's supposed to be a global model if I understand correctly)? If there are issues with computing time already in a 2d setting, is it realistic to go to 3d? Some words on validation/tests on larger scales? Answers to some of these questions might be pretty obvious, but I nevertheless think that a proper discussion of these and other related questions is required.

Specific comments. - L.24 : "Three ten-years long simulations" : Is that good English?

- L.55 : "Xu, 2016#154"

- L61: The reference to Friedlingstein et al., 2006 is good but there has been quite some work on this more recently. In general, there are very many pre-2007 references and much less after that period. Maybe the bibliography could be a bit updated. For example, in line 78, the review by Schuur et al. in Nature 2015 might be worth citing.

- L.166. "The flow water" -> "The water flow" or "The flow of water"

- L. 198. I suggest to clarify the writing here. What about this: ". . .. zeta is the diagonal entry of the banded matrix (eq. 11-17)", then provide eq. 11-17. Then: "small phi is a column vector given by:", then put eq. 18. I think that would be clearer.

- The same applies to eqs. 25-32. Separate eq. 32 from 25-31. I think that eq. 28 should read "eta=..." (not "mu=...") and eq. 29 should read "mu=..." (not "xi=...")

- Line 232: Please say clearly that this means that there is no geothermal heat flux represented in the model.

- L. 261: "to simulate SR", not "to simulated SR"

- L. 273: "its", not "it's"

- L.277: A broken link to some internal reference. same at line 328, 342, 343

- L. 285: with do you put the dimension meters in square brackets?

- L. 289: "SP mode": that's an internal nickname. Its meaning becomes clear at the end of the paper ("satellite phenology") but this is not required here. Either explain the acronym of leave it out.

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