Answer to the editor's comments on our manuscript entitled : "Impact of runoff temporal distribution on ice dynamics."

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1 Overall comments

I appreciate the enhanced description of the dual-continuum model (lines 90-102). However, it is problematic because it is word-for-word the same as the description in de Fleurian et al. (2016). This portion of the manuscript needs thorough paraphrasing before it can be published in The Cryosphere.
This was in fact a copy past of the preceding description of the model and my impression was that it is fine in this type of context to reuse the description as it is still the same model. I rephrased it anyway so it should hopefully not be too similar anymore.

Figure 1 inset – It would be clearer to understand if the y-axis were "Runoff" instead of "Temperature". I get that you used a PDD so your base forcing truly is temperature, but the processes that you are studying relate to runoff, and runoff is more frequently discussed in your text (as well as shown as the forcing in all other figure panels) than temperature.
Done

Figure 4 should include altitude values on panels b and c as well, not just panel d.
Done

2 Minor Changes

- Abstract remove or rephrase "surprisingly" as it is imprecise / unscientific. Similarly, remove or rephrase "and/or" as it is informal.
  Done
- Clarify which of the sensitivity runs (low / medium / high runoff) is the reference simulation. It is fairly intuitive that it should be the medium one, but this is never stated.

Actually table 2 states “reference value” and not medium value. This has been emphasised also at the beginning of section 3.1 for clarity.

- The elevation of the runoff limit is unclear. I understand that it varies between runs (low, medium, and high runoff sensitivity runs). Presenting this information in either text, a table (perhaps Table 2), or on figures would be helpful, as I found myself searching the manuscript for the runoff limit. From Figure 3 I could infer that it lies between 1215 m and 1465 m for the reference simulation.
  A sentence has been added in the forcing section to give those numbers

- SSUmax should be defined specifically. Currently it just says “this shift in behaviour” (line 234), although I think that SSUmax is actually an elevation. What SSUmax stands for should be defined, and what the “max” refers to elevation or speed? should be stated as well.
  The sentence on line 234 has been rephrased to clarify that point

- line 306-7 a few extra words here, "the" and "to"
  Fixed

- line 328 "more larger" should be corrected
  Corrected

- Table 5 Instead of "Colder temperatures", it should say more directly what the experiment actually was: "lower intensity of runoff" (or something similar), and same for "Warmer temperatures".
  The experiments are actually based on warmer or colder temperature but we agree that higher or lower intensity makes more sense in this manuscript.

- line 361 similar to the comment above, replace "colder"
  Changed.

- line 379 instead of "strong" velocities, this should say "high" or "fast"
  Replaced.

- line 385 rephrase "rather inefficient efficient draining system"
  This sentence has been rephrased.

- line 403 rephrase the sentence starting "However, we think..." in order to make it more clear which you are hypothesizing (that there is a reasonable physical explanation) and which is fact rather than speculation (that your results are robust).
  This has been rephrased.

- line 454 "pauses" should be "poses"
  Changed.

- line 462 specify that you mean –spatial– distribution here.
  Fixed.
  Further along in this paragraph, you mention that moulin location is likely unimportant, but would you care to address moulin density (e.g. Banwell et al., 2016)? Essentially, your model has a moulin at every grid node, so it is maximally dense.
  Reading again this sentences it actually feels that moulin density is actually more suited here than moulin position, the sentence has been corrected accordingly.
  Finally, the last three sentences in this paragraph (lines 469-473) are a bit convoluted and should be smoothed out to make your point clear: that the highest injection point in real life is not likely to change from year to year. As currently written, the reasoning switches back and forth with "however"s that seem to me to be misleading (i.e. not actually in contradiction with the previous point). Perhaps this made me misunderstand the basic argument. Please check and revise accordingly.
  This part have been rephrased

- line 475 "without performing the actual simulations" do you mean simulations with real-life runoff inputs?
  No this was more related to the length of the simulation that would be needed to evaluate the response of the coupled system on a longer term, this has been clarified.

- line 501 change "constant runoff" to something that specifies that the time-integrated runoff volume is unchanged from simulation to simulation. As is, "constant runoff" could be read as unchanging in time.
  This has been clarified.

- line 502 change "originally" to "initially"
  Done.

- line 512 "led" seems like not the right word here and is redundant with "at the beginning of the melt season"
  I could not spot the redundancy but changed “led” to “performed” which seemed to be better suited.

- line 514 I believe you’re actually advocating for use of models with –both– inefficient and efficient components, yes? As written now, it could be interpreted that a channel-only model is a good approach. Please revise.
  Yes I am indeed advocating for double components models, that as been reformulated