Interactive comment on "Canadian Snow and Sea Ice: Trends (1981–2015) and Projections (2020–2050)" *by* Lawrence Mudryk et al.

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All responses below refer to the revised version of the manuscript submitted as as supplementary pdf document (highlighted changes in red).

L29: You use at least four different expressions for annual maximum SWE throughout the paper. I don't like peak pre-melt SWE, since melt can also happen before maximum SWE. I suggest using always the same expression.

The expression annual maximum SWE, which we abbreviate as SWEmax, is now used throughout the paper.

L 38: I would like to see some references, which demonstrate "the critical importance of snow/sea ice to Canada's natural environment, ecosystems, and econ-

omy."

Text has been added at line 36:

L 99-104: Please write some sentences about the percentage of affected pixels and which regions are mainly impacted.

We have added additional information at line 106:

L 111-119: For consistency and understandability please introduce here somewhere the investigated sea ice variables, similarly as it has been done in the former chapter for snow. L 116: As a non sea ice specialist, I would like to see short hint saying that sea ice concentration is a similar measure as snow cover fraction and is therefore also expressed in percentage.

We have added additional text at lines 121 and 134.

L 136: Please provide some information about the spatial resolution of the used CMIP5 output.

Additional information has been provided at lines 155 and 171.

L148-149: "...branch off from five different historical simulation..." I don't understand this sentence, please elaborate.

The wording has been changed in lines 163-166. For the purpose of this paper it is only relevant that the ensemble is an initial condition ensemble. [Other ensembles of this sort have been constructed so that all realizations evolve under identical radiative forcings and using identical model parametrizations from small perturbations to a single climate state (for example, the NCAR initial condition ensemble). In the case of the CanESM2 initial condition ensemble, 5 interchangeable initial climate states were used. The initial states differed in that their exact climate tracks — their oceans in particular — had been allowed to diverge from one another over approximately 100 years. However, they are interchangeable because each was itself produced using identical radiative

forcings and identical model settings. As such they differ from one another only due to natural variability. Each of these 5 climate states was perturbed 10 times for a total of 50 realizations.]

L 159: The acronym "SIE" is not used anymore in the following text, so please delete.

Thank you. Removed.

L 179-180: What is the reason you define the seasons different from the usual meteorological definition? Maybe, add a corresponding sentence in the methods chapter.

Text added at line 195:

L 205: "...region with positive SCF trends is slightly more extensive." It would be helpful if you could at least write some sentences about the observed precipitation variability.

We now include an additional figure (figure 5) showing estimated annual snowfall trends and provide discussion starting at line 252:

L 275: "modal thickness"?

We have rephrased this sentence at line 303:

L 283-284: The multi-model projected mean changes in surface temperature are positive in all seasons, hence only reductions in ensemble mean SCF and SIC are evident in Figure 8.

Changed.

L 310: "...balances projected increases in snowfall." It's the first and only time you write about projected increases in snowfall. Could you please elaborate.

Because we don't have projections for snowfall we are unable to elaborate here, but

we have rephrased the sentence to make clear this is speculative (line 340).

L 311: "snow mass", I guess you mean SWE, which would be a more familiar term and consistent with what you used already.

We intended the term to distinguish the integrated quantity of snow water (its total volume or equivalently its mass) across a given area or region, which strictly speaking is not SWE. We have provided brief definitions for this term and snow cover extent which is defined similarly (line 343).

L 329: Is it not 1981-2015 for the present and 2020-2050 for the projection?

We have corrected the years for the historical period. The years included in the future period differ slightly from those used in the majority of the paper and are correct. We have removed the references in the title to specific time periods because along with minor differences in the future projections commented on here, some of the sea ice trends presented extend back to 1968.

L 330: Why using a new variable Ts and not TAS?

This section was unclear. Ts did not represent TAS as shown in Figure 2, it is a diagnostic temperature. We have revised the description at (lines 362-374) and relabeled the quantity as Td. Hopefully this will make the section clearer.

L 349:...shows the observed record of annual sea ice extent...

Changed.

L 377: MYI losses

Changed.

L 413-418: This paragraph seems kind of odd at the first sight, because alpine snow has only been mentioned once at the beginning. Please link this important content to the corresponding analysis.

We have tried to link this point more closely to the analysis contained in the paper (lines 457-461). The 2018 reference added is passed review so we are expecting to be able to update the reference before publication of this paper.

L 427: FYI as not been introduced so far.

Clarified.

L 445: (Laliberté et al, 2016) "Resolving the dates...has important implications for climates studies" Please elaborate.

This was rephrased. We meant only to say that knowledge of when the CAA will become ice free will facilitate adaptation (line 490).

L622: Not in press anymore...

Thanks!

L 664-665: Please elaborate more clearly what's the difference between the region with single hatching and crossed hatching. The text in chapter 3.1 should also reflect these differences.

We agree that the false discovery rate significance was unclear. We have decided that standard calculations of significance are sufficient. Captions and images for Figures 1,2,4, 9,10 have been altered to reflect this change.

L 685: I suggest to put the 10^3 in the axis label for figure 6 a-d.

Good point. Changed.

L 705: Replace snow water mass with snow water equivalent.

Snow water mass has now been defined in the corresponding text.

L 711: Please explain "T_{sd}" and mention that T_{th} is -5^oC, since the scale of the legend bar is not linear. Also elaborate what T_{th} actually controls, i.e. that the bluish colored regions are mainly controlled by precipitation.

The text added in the manuscript should help clarify these points. Please note that T_{sd} was a typo for the original variable label, T_s. We have renamed this variable T_d in the revised manuscript for "diagnostic temperature." The caption now lists the value of T_{th} and describes the different drivers of SWE variability in the different regions as requested.

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2017-198/tc-2017-198-AC1-supplement.pdf

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-198, 2017.