

Interactive comment on “Historical Northern Hemisphere snow cover trends and projected changes in the CMIP-6 multi-model ensemble” by Lawrence Mudryk et al.

Anonymous Referee #2

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Overall I thought this was a good paper and will make a good addition to the literature, especially since snow is a critical aspect of the natural environment (with obvious links to transportation, recreation, water supply, etc.) and is projected to decline with continued anthropogenic climate change. I have only minor comments, as enumerated below.

Line 77: Poor spatial resolution likely plays a key role over mountainous areas but is not explicitly mentioned here.

Line 108: Please note somewhere, for example in Table 1, the resolution of the gridded snow mass data sets before regriding to 0.5 degree resolution.

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Line 112: Please provide information that would enable the reader to understand to what extent this weighting/mixing was performed. For example, what percent of active grid cells had a GlobSnow weight of 0.5 or less?

Line 117: 4 mm seems somewhat arbitrary. Are your results sensitive to using a different value? For example, does figure 3 only look good because 4 mm was used?

Line 120: You lost me in this part about merging the data sets, not tactically (what you did) but strategically (why you did it). The initial motivation for using multiple data sets was that any one data set is uncertain, and by using multiple data sets you take the uncertainties in observational estimates into account. But now you are removing some of the differences between the data sets, which seems to obviate the motivation for using multiple data sets in the first place. I suggest you augment this section with an explanation of why you chose to take this approach given that one of your motivations was to look at the spread of results across observational data sets, and that this methodology seems to diminish that spread.

Line 148: Off the top of my head, I would have said that the *fractional* variability had likely not changed, but (along with the diminished snow cover in the recent period already seen) the actual variability might have. I.e., if you have more snow overall I'd expect first order that you'd have more variability, but perhaps the same fractional variability. Maybe it's just wording, but it would be good to be clear on this point. Presumably the actual amount of variability scales with the amount of snow, but the fractional variability would be more constant.

Line 161: Although you're not wrong about the emissions following RCP 8.5 from 2006-2014 (mostly, but dropping below that towards the end), in fairness, the global mean temperature between, say, RCP 4.5 and RCP 8.5 are statistically indistinguishable during this period.

Line 164: Typo, should say "SSP3-7.0", not "SSP3-3.7".

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Line 166: Regarding "r1i1p1f1", note that the CMIP6 gateway has a warning right at the top of the page that states "WARNING: Not all models include a variant "r1i1p1f1", and across models, identical values of variant_label do not imply identical variants! To learn which forcing datasets were used in each variant, please check modeling group publications and documentation provided through ES-DOC". If that is true, I'm not sure that it is necessarily correct to compare r1i1p1f1's across models. I suppose as the community gains more experience with CMIP6 this will sort itself out. I assume all the models you analyzed did, in fact, have an r1i1p1f1 member?

Line 177: Would I be correct in presuming that models calculate their own snow cover fraction by applying a threshold to their SWE? If so, is there any reason to believe different models used the same threshold? If this is the case and different models used different thresholds, might it have affected your results by adding an element of inconsistency between the analyses of different models?

Line 183: The wording does not make clear whether you used the CNRM-CM6-1 mask for all models, or just the subset of models that did not have the needed masks available. Please clarify. Additionally, if the CNRM mask was only used for a subset of models, please indicate somewhere (in text or in table 2 perhaps) which models used the CNRM mask instead of their own.

Line 191: "IPSL CICALD computer center" is not sufficiently descriptive. Please augment the acronyms with more specificity, such as spelling it out, location, and institution.

Line 261: "there is clear improvement in mean model agreement with observations at the hemispheric scale..." I presume this is based on Fig. 3, not figure 4 (which shows a degradation in model agreement with observations) or Fig. 5 (which does not show observations at all)? If so, please be a little more thorough on line 261 by noting that it is snow cover extent that shows clear improvement while snow mass shows less agreement with observations.

Line 308: Does each dot in figure 9 represent one year averaged across all models, or

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what? Please add a bit more clarity to the text or caption.

Line 371: Please add an X axis label to Fig. 11.

Throughout: I suggest using "autumn" rather than "fall", since "snowfall" is a different thing and too similar to "fall snow".

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-320>, 2020.

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