Response to Reviewer 3.

Review of "The influence of snow on sea ice as assessed from simulations of CESM2" by Marika M. Holland et al

General comments:
The impact of snow on sea ice is difficult to assess due to the relative lack of long-term observations in remote polar regions. Given the many, competing feedbacks between snow on sea ice and sea ice itself, it is important to quantify these feedbacks and their climate impacts. In my view, this paper significantly adds to our current understanding of snow on sea ice and its impacts. It presents a study of the influence of snow thickness on sea ice in both pre-industrial and 2xCO2 experiments in coupled slab-ocean CESM2, varying the snow thickness by multiplying the snowfall on sea ice by different constant factors. The paper finds that overall, in both hemispheres, snow on sea ice tends to result in increased sea ice volume and cooler temperatures, although the ice mass budget response differs between the hemispheres. In a 2xCO2 climate, the study finds that Arctic sea ice sensitivity to snow depth is reduced, whereas the Antarctic sensitivity is similar to the pre-industrial climate.

The paper is overall well-structured, clear, and well-written, and presents results that, in my view, are of high scientific interest. It is well-referenced, with an appropriate number of references. There is a good number of figures, and the figures are generally clear, although I have a few minor suggestions below. There are some parts of the paper where I think some additional detail would be helpful, (as explained in my comments,) but overall, I strongly recommend that this paper be accepted for publication, with minor revisions.

Thank you for your helpful comments.

Specific comments:

My first suggestion pertains to the use of the slab-ocean model (SOM); I recommend that the authors discuss some of the associated caveats in more detail in the paper. (To be clear, I do not think the use of a SOM is inappropriate for this study; I simply would encourage the authors to expand some of the discussion of the SOM.) The paper mentions that the SOM used has a prescribed ocean heat flux convergence from the CESM2 preindustrial control (line 113-114). However, it has been found that ocean heat convergence weakens near the ice edge with increased CO2 (as mentioned in Bitz et al 2005, which this paper cites) and thus, I wonder if the Qflux used in this configuration may still retain some bias related to this. Also, the lack of representation of ocean dynamics in the SOM is briefly mentioned, but I think it would be helpful to briefly discuss possible impacts the lack of ocean dynamics could have on the results of this study, where applicable (eg. possibly in terms of hemispheric differences being modified by ocean dynamics).

We have added some text in the Methods Section to better describe the SOM, the implications for neglecting dynamic ocean feedbacks, and the equilibrated nature of the 2xCO2 simulations. We also return to the implications of using a SOM in the discussion of the mass budgets and in the conclusions.

Secondly, although several snow processes are mentioned, it is not always clear which of these processes are represented in the model (eg. wind-driven blowing snow, rain-on-snow, snow density and thermal conductivity, etc.). I think it would be helpful to include a brief description of which snow processes and properties are described by the model, and possibly what biases could be present from the exclusion of certain processes. (It seems to me that this is briefly discussed in places, but I think more detail would be beneficial.)

Thank you for this suggestion. We now more clearly describe how snow on sea ice is modeled within the methods section. In the conclusion section, we return to some of the limitations in the snow model representation and the need for further work to understand snow processes and to improve the model representation.

Finally, I think there could be additional discussion included in the conclusion about what uncertainties remain, and possible next steps.

Within the conclusion section, we now include an additional paragraph that discusses model limitations and what this suggests for future work.

Specific comments line-by-line:
(N.b.: These comments may be taken as suggestions .)

105: Is this due to processes not being represented in the model?
It is not entirely clear why the snow is thin and accumulates too slowly on Arctic sea ice in CESM2. We expect that it may be due to the ice being too thin and a too warm climate. We choose not to speculate here though since the reasons are uncertain.

116: Can you be more specific about what "mostly equilibrated" means in this context?
We now provide more clarification here.

117: Perhaps describe how missing ocean dynamical feedbacks could impact the results here.
We now mention that differences in ice melt/growth rates across the simulations will not influence ocean mixing and heat transport. We also clarify that for the 2XCO2 simulations, the use of a SOM means that we are assessing an equilibrium climate response.

120: It would be helpful to include some description of the snow processes in the model in or before this paragraph (or somewhere else in the section, where appropriate).
We now include an extra paragraph that describes the snow processes in the model.

145-146: There seems to be a missing citation here for "lack of significant anthropogenic ice loss that has been observed in recent decades"
We now provide a reference.

Figure 1: If possible, I suggest that these map figures be made without the grid, since it is too faint to be visible and it seems to be producing artefacts. Also, for clarity, consider specifying the spacing of the black isolines somewhere (perhaps in the figure caption).

We have revised Figure 1 to remove the grid artifacts and include the contour interval for the black isolines in the figure caption.

Also, although snow observations are limited, I think it could be beneficial to include a comparison with snow thickness observations if those are available; perhaps as a supplementary figure if it does not fit well within the main text.
We now include a comparison of the simulated snow to observations in supplementary material.

164: Are there any ice processes not simulated by the model that could have an impact on the results?
We now mention some of the limitations in the parameterizations of some mass budget terms within the model, including the simple lateral melting parameterization and lack of variable floe sizes and simple frazil ice formation parameterization.

172: Some points that may be helpful to discuss briefly: how might the constant density impact the results? In what regions would we expect results to be most impacted by this density assumption?
We have added some information on the implications of the constant snow density and regions and seasons where it might be most impacted.

Figure 3,5: Consider aligning the months here to the seasons as in Fig. 2.
We have chosen not to align the month here as in Fig. 2 given that many people are used to looking at the annual cycle of these properties over a calendar year.

273: The term "basically equivalent" is vague; could you clarify what do you mean by this?
We now clarify here that equilibrium conditions occur when the long-term average melt and growth are equal.

Figure 10: Some of the overlapping bars are difficult to see; is it possible to make them narrower so that they overlap less?
We attempted to make these narrower but they still have considerable overlap and the figure is really not any clearer. Given this, we have chosen to keep the figure as is.

331 (and other places where ice motion is mentioned): It would be helpful to mention what drives the ice motion in this model.
We now mention in the description of the model within the Methods Section how ice motion is computed.

426: This is the first time in this paper that these cases are specifically referred to as regimes so it seems to come somewhat out of nowhere; consider mentioning it earlier in the paper.
We now mention these regimes earlier in the manuscript.

Technical corrections/notes:
111: I think "mixed-layer-averaged" should be hyphenated, or this could be rephrased to avoid hyphenation (the wording is somewhat ambiguous as-is)
We have revised the wording hear to more clearly describe the slab ocean model.

117: Missing comma after “excluded”
This sentence has been revised.

173: The 3 should be a superscript in kg/m3
Fixed.

212: I think “snow free” should be “snow-free”
Fixed

224: It would be clearer if this said something like “a significant jump in ice volume and area from the Fsnow=0 case to the Fsnow=0.25 case”; clarifying that the jump is between the 0 and 0.25 cases.
Thank you for the suggestion. Revised as suggested.

240: Missing word, should be “the colors are the same”
Fixed

258: Hyphen missing in “high-latitude”
Fixed

259: Missing word, should be “consistent with a reduced”
Fixed

300: As in 212, I think this should be “snow-free”
Fixed

304: Hyphen missing in “snow-covered”
Fixed

341: I think there should be a comma before “but”
Fixed

349,363: Hyphen missing in “snow-ice” (assuming that the convention being used is to hyphenate)
Fixed

379,415,422, etc.: Inconsistencies in hyphenation of “preindustrial” vs “pre-industrial”
Changed everywhere to “preindustrial”

References (general): Many entries here include italicization, but TC guidelines indicate to not italicize text in references.
Italicization removed.