*Section 2 – this paper focuses on snow, hence it would be nice to give a better description of the snow sub-model and how it fits within CICE. Items perhaps worth mentioning are:*

A bit of text is added, throughout the model description, howver I think the most succinct passage added is : "… snow is only transferred between ice thickness categories or grid cells with the ice it overlays. As such, the snow overlaying the sea ice can be considered a feature of the ice, rather than an independent process."

*1. explicitly stating whether or not each ice thickness category maintains its own snow state variables. I believe they do given the sentence "Each category includes discrete thermodynamic treatment" - you could simply add "including snow calculations".*

They are separate, and the suggested clause was added

*2. a description of the snow sub-model structure may also be helpful. My understanding from the documentation is that for each ice category the snow model runs as a bulk layer model (default Ns=1, and that's what was used here), but it could (?) be easily configured as a multi-layer model. ….*

Added to text, as well as default time step (dt=30min, ni=4, ns=1) these can be changed by model users, as indicated in the updated text.

*3. confirm whether snow has a uniform depth across each respective ice category – probably(?)*

confirmed and clarified in the text

*"CICE does not currently include blowing snow" - does any sea ice model in an ESM/GCM currently include blowing snow? I am struggling to think of a land model within an ESM/GCM that includes a proper blowing snow sub-model. While blowing snow processes likely should be included in sea ice models, is it the case that their exclusion is currently the norm, not the exception? Maybe worth mentioning for the sake of context?*

Added this qualification, but given the citation to Chung et at 2010, I think blowing snow is especially important on sea ice. Not only for redistribution, but for loss of ice volume to the ocean via leads. This is a process that doesn't really impact land models. I am not sure this requires a full blowing snow model. Perhaps a parameterization of loss would be sufficient.

*A suggestion is to make the model description section a little more concise. It currently contains unnecessary repetition regarding use of the fully coupled POP2 vs SOM, as well as excessive text justifying the use of the SOM. Some of this is again repeated in Section 4.1.*

Another reviewer requested additional discussion of the SOM, so I have left the perhaps excessive initial description of the POP but cut down the repletion in section 4.1 by simply reminding the reader that the SOM is in use, and therefore there is no ocean to equilibrate.

*Section 3, page 1503 & 1504. A suggestion is to use all_ice when referring to the "all ice" comparisons and thick_ice for the "thick ice" cases. This (or some similar) distinction can avoid later confusion and informs the reader that a specific comparison/data set is being referenced.*

This is a welcome clarification, as thick ice is used in several places in the generic sense.  The new text incorporates this suggestion

*page 1505, line 18: "Russian drift station transect data has a lower standard deviation in snow depth than the model, a mean of 30% of total thickness ...". This section needs work...*

I have made modifications to the text , simplifying where necessary and and removing the confused interpretation regarding the explanation of std deviations.  As it stands now, the text simply points out that the snowstake stddev is higher than the transect due to averaging, and the all_ice is higher than the thick_ice, due to the selection process.  The two in situ variances bracket the two model variances.

*Section 4.1 is in need of clarification – it leaves the reader somewhat lost....In the opinion of the reviewer presentation of the sensitivity experiment design could be more conciseand straightforward. If someone was trying to reproduce your results, would the explanation presented allow them to to do so?*

This section has been largely rewritten, and while *U* is still used in the initial explaination, the equations have been rewritten to focus on *k,* culminating in $k_{sensitivity} = k_{snow} \dfrac{h_{snow(eval)}}{h_{insitu}} \dfrac{\rho_{insitu}}{\rho_{snow}}$ , the term which is actually applied in the model. Clarification is added on smoothing, whereby the derived monthly values of k are interpolated each day.  Generally the methods should be more reproducible

*I don't believe that the quantity of thermal transmittance (U) is used by CICE; it is a convenient diagnostic for showing the potential impact of changing k under steady state conditions*

Clarified that this was more for demonstration, also in the rewrite of this section extensive use of U in the equations was replaced, with use of U only to clarify why altering k results in the intended/predicted response.

*An interesting part of the results is why a change in thermal conductivity of snow (as shown in Figure 4) results in a seasonally uniform increase in snow depth of about*

*11cm (which is about a 30% increase in snow)[Figure 6]? This seems a bit odd to me. Is this caused by an increase in precipitation or a decrease in melt? I know that CCSM4/CAM4 had issues with excessive Arctic precipitation on land - is that same problem simply amplified? What is the mechanism that would make the difference so uniform (and large!) over the annual cycle? Isn't it a bit ironic that the experiment was meant to impose the equivalent of a shallower snow pack, yet snow increases. In any case, I would have thought that a 30% increase in snow depth would approximately compensate for the imposed increase in thermal conductivity, yet we still see a 20% increase in ice volume [Figure 5]. Not to mention that the results in Figure 11 show substantial change in the atmosphere. If indeed all these changes are are a result from only changing k by approx. 0.1 Wm-1K-1, this would seem like a very sensitive model/system …perhaps unreasonably so (I don't know, but it would seem that way to me???). I feel some comment should be made about this... and/or perhaps some comparison made between the impacts of a change in k vs. a change in albedo (e.g. Holland et al., 2012). However, that is up to the authors.*

I agree that this is pronounced, and the new text contains a couple of added statements to that effect. I suspect the survival of the ice pack plays a major role in increased snow depth. By having more ice in the autumn, more of the early snowfall is captured by the ice, rather than lost to the ocean. This effect is described in Blazey 2012, and I don't feel it is necessary or productive to include an extensive investigation here as well. The model is very sensitive to these minor changes, and Holland et al 2011 is a good example, where changes on the order of 1m are observed due to a 1.1w/m2 shift. The reviewer is correct that the model is very sensitive, but this particular study is not unique in demonstrating that.

*Adding blowing snow is no doubt worthwhile, but if CCSM/CESM + CICE indeed has the sensitivity shown here, perhaps there are higher priority issues to deal with? It would have been good to perform off-line sensitivity simulations using CICE so responses could be isolated from feedbacks... perhaps the next step.*

I agree, and in the past performed some CICE stand alone simulations to look at the sensitivity separate from the feedbacks. Some of these are in Blazey 2012. An attempt was made to publish these results, but the reviewers were generally unhappy with the uncoupled environment, specifically because it omits feedbacks.

*Also, the reviewer is not a big fan of introducing "a parameter that could be tuned to compensate for excess snow flux from the atmosphere". While sympathetic to the utility of such a parameter, it goes against the premise of physically based climate modeling. This is just a comment to the authors, not a necessity for change.*

Ideally, a physically consistent parameter would be determined from literature such as Chung et al 2011, and would be fixed at this physically appropriate value. However, given that there is some independence in model development it is likely biases from one model will continue impacting other models. I suspect the reality is such a parameter would be tuned. I believe tuning a process that does occur in nature is better than allowing a bias to persist and impact other processes. Such a parameter will enable us to develop the ice model to respond accurately if the other models do as well, but frees the developers from relying on accurate fluxes from the other models.

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Holland, Marika M., David A. Bailey, Bruce P. Briegleb, Bonnie Light, Elizabeth Hunke, 2012: Improved Sea Ice Shortwave Radiation Physics in CCSM4: The Impact of Melt Ponds and Aerosols on Arctic Sea Ice*. *J. Climate*, **25**, 1413–1430.
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Chung, Yi-Ching, Stéphane Bélair, Jocelyn Mailhot, 2011: Blowing Snow on Arctic Sea Ice: Results from an Improved Sea Ice–Snow–Blowing Snow Coupled System. J. Hydrometeor, 12, 678–689.