

Review of : Arctic Ocean sea ice snow depth evaluation and bias sensitivity in CCSM by Blazey et al.

This paper begins by describing an assessment of the skill of the CCSM model in simulating snow on Arctic sea ice. Following the comparison to observations (as best as possible given their scarcity), some first order sensitivity tests are performed to infer the impact of potential snow depth errors upon the simulation of sea ice and the broader Arctic climate. In general this paper is acceptable for publication but the reviewer suggests some revisions.

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:

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".
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 $N_s=1$, and that's what was used here), but it could (?) be easily configured as a multi-layer model. Is that correct? For examples of model structures see Figure 5 of:
<http://journals.ametsoc.org/doi/abs/10.1175/1525-7541%282001%29002%3C0007%3ATROSIL%3E2.0.CO%3B2>
(You don't need to cite the paper, I mention it just as a method of description.)
3. confirm whether snow has a uniform depth across each respective ice category – probably(?)

"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?

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.

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.

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 - as a reference point, imagine someone trying to reproduce your results; what would they need to know to calculate the same statistics? Over what time/space are you computing the quoted standard deviation and mean? Is it the case that : across all sea ice points with drifting station observations, for each month(???), the average coefficient of variation (std. deviation/mean) of observed snow depth (transect means??) is 0.3, which is lower than the modeled values of 0.54 for all_ice and 0.49 for thick_ice. If the above is true, why do you say that the observed data has a lower variance due to averaging across the transect? From my reading, the model data used for the comparison is the mean of the grid-box, hence, both model and observation data used in the comparison are meant to represent an area-averaged quantity?? Is that right? Ensure the reader is aware of what quantities are being compared and which data is being used for summary statistics- it's a bit unclear at present.

Section 4.1 is in need of clarification – it leaves the reader somewhat lost. As far as I can tell, the only item of consequence that was actually changed in the experiment is thermal conductivity (k). If that is so, it should be stated clearly. Instead of having a value $h_{snow(eval)}$ (which gives the impression that you changed snow depth) your equations should have a term k_{sens} or k_{equiv} (or something like that) which clearly shows how you arrived at the new conductivity value i.e. this term should be the left hand side of an equation within the paper. The main point you need to get across is that k was changed so that the ice (at least initially) experiences the conductive equivalent of the observed snow cover. That is: the change in k attempts to account for both the depth and density differences shown in Figure 1, 2 and 3. The thermal conductivity modifications made to the model do not explicitly treat all the intricacies of heat transfer processes such as altering the heat capacity of the snowpack or maintaining a quadratic relation between snow density and conductivity, but they represent a sufficient first order sensitivity test of errors in snow depth. Other factors that are a direct function of snow mass or depth probably didn't experience a change, right? e.g. extinction/transmission of shortwave radiation (though this likely plays a far lesser role in the energy balance...).

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. As such, it may still serve as a useful demonstration within the paper, but could do with some qualification e.g. state its assumption and that it is used just as a metric for assessing the extent to which thermal conductivity is modified.

In the opinion of the reviewer presentation of the sensitivity experiment design could be more concise

and straightforward. If someone was trying to reproduce your results, would the explanation presented allow them to do so?

“0.5 Wm⁻² increase in conductive flux This change is comparable to the 1.1 Wm⁻² change caused by the addition of black carbon and melt ponds”. Given that one flux is half the size of the other, are they really comparable (?), particularly in light of the impact that such small changes seem to have ... I guess their order of magnitude is similar.

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 11 cm (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 a result from only changing k by approx. $0.1 \text{ Wm}^{-1}\text{K}^{-1}$, 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.

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. 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.

A.G.S