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Interactive comment on "Arctic Ocean sea ice snow depth evaluation and bias sensitivity in CCSM" *by* B. A. Blazey et al.

Anonymous Referee #2

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General Comments:

This paper is an assessment of the impact of snow on sea ice in relation to the thermodynamically driven growth and decay of sea ice in a fully coupled atmosphere-ocean GCM. It does two things: Assesses the snow depth bias in the NCAR CCSM4 in the Arctic, based on a set of long term observations from Soviet/Russian drifting stations in the Arctic from 1954-1993. Secondly, it assesses the impact of this bias by modifying the thermal conductivity of the snow layer in a series of slab ocean simulations and notes the corresponding changes in the sea ice state as well as the impact on several regional climate indicators such as surface air temperature, downwelling longwave radiation and clouds.

As a direct adjustment of the snow depths in the model affects mass and energy con-

servation, the authors choose to adjust the thermal conductivity, partly compensating the overall too thick and insulative snow cover. The strength of this paper lies in the evaluation of the effects of such pertubations in the framework of a fully coupled model, while, as noted by the authors in their manuscript, such studies have only been performed using one dimensional or semi-coupled models. These results are clearly important for publication.

We find a weakness in the paper with respect to the evaluation of modeled versus observed snow depths. The authors use only the Russian drifting snow depth measurements, and complete a fairly thorough analysis of the bias against that measurements. They reject, however, the Ice Mass Buoy assessments of snow depth, for reasons that are not entirely clear (citing buoy displacements through Fram Straight though it seems they could be evaluated in the Arctic interior at least), and fail to consider more recent measurements from IceBridge flights over the Arctic (see Kurtz and Farrell, (2011) and Kwok et al.,(2011)). Much of this data has recently been made available online.

We also find a mismatch between the relatively broad title of the paper and the much narrower focus of the thermodynamic impact of a bias in snow depth, while neglecting the radiative impact of the same bias. We agree with the methodology, i.e., the perturbation of the effective thermal conductivity as an appropriate way to proceed with the thermal impact of the snow depth bias. However, the broad title implies that the radiative effects of the snow thickness bias should also be properly considered.

We find another major weakness in the paper associated with Figure 9. The transient response in CCSM4 is indeed interesting, especially compared to the equilibrium response. It is especially interesting with respect to the increase then decrease in congelation ice growth associated with the experiment compared to the control. However, the Figure 9 and its explanation on page 1512 line 16 continuing through P1512 L27 is unclear and not intuitive. On initial reading, it seems that the data in the graph shows exactly the opposite of what is claimed to be an example of the ice-albedo feedback.

In light of these overall issues, we suggest the following:

1. Focus the paper on the sensitivity experiments performed given the snow depth bias identified for CCSM4. These results are the most interesting. Highlight the impact of these results on other models, as well as the implication for modeling a future Arctic which will likely tend toward higher snowfall rates and thinner ice. Modify the title to follow this focus.

2. The snow depth bias identified for CCSM4 should not be a primary focus of this paper. Though necessary to establish the line of inquiry into the model sensitivity to the bias, in and of itself it is incomplete due to the lack of comparison to other measurements of snow depth (IMBs in the central Arctic basin and Ice Bridge measurements), and lack of investigation into the relative role of the high bias in Arctic precipitation and the role of incomplete and missing processes in CICE (e.g. snow redistribution, densification, aging, etc.). Establishing the sign and rough magnitude of the snow depth bias is certainly enough to justify the sensitivity experiments outlined in Section 4. Revise section 3 so that the purpose is to justify the experiments of section 4, without claiming to be a full evaluation of the the snow depth bias in CCSM4.

3. Completely revise (expanding as necessary) the explanation of Figure 9, potentially with a revision Figure 9 itself. See more specific comments below.

References:

Kurtz, N. T., and S. L. Farrell (2011), Large-scale surveys of snow depth on Arctic sea ice from Operation IceBridge, Geophys. Res. Lett., 38, L20505, doi:10.1029/2011GL049216.

Kwok, R., B. Panzer, C. Leuschen, S. Pang, T. Markus, B. Holt, and S. Gogineni (2011), Airborne surveys of snow depth over Arctic sea ice, J. Geophys. Res., 116, C11018, doi:10.1029/2011JC007371.

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Specific suggestions:

1. Abstract:

P1496 L12-17 "Consequently, we suggest that the inclusion of additional snow evolution processes such as blowing snow, densification, and seasonal changes in snow conductivity in sea ice models would increase the fidelity of the model with respect to the physical system. Moreover, our results suggest that simulated high latitude precipitation biases have important effects on the simulated ice conditions, resulting in impacts on the Arctic climate in general in large-scale climate."

These two statements are too broad and are a bit misleading as to the direction of the paper. We suggest it be turned around to lead with the statement of the thermodynamic impact of snow, and use that to motivate which processes which would be most important to capture correctly. Though a priori all of these processes are important, a statement about which would be most important to implement first would be more useful, especially since (as outlined in the text) there are more complications and computational cost to adding variables in the ice model compared to the terrestrial snow model.

2. Treatment of thermal conductivity/resistance.

Theoretically, terms such as "thermal conductance" or "resistance" are valid only for stationary systems - i.e. the time scales of adjustment of the temperature profile of the snow-sea ice or heat diffusion through the ice and snow are much longer than the time step (or of evolution of snow depth and thermal conductivity). What is the time scale for the heat diffusion equation?

A follow up question arises since it is not clear how often adjustments to the thermal conductivity are made. Monthly? At each time step? We suggest this be made explicit in the methods section.

3. 'Snow conductivity' would better be described as 'snow thermal conductivity' or

'thermal conductivity of snow' throughout the manuscript.

4. What is the treatment of surface properties in the slab ocean configuration? Are they treated the same way as in the coupled model?

5. The results presented in the manuscript become increasingly more important as the Arctic ice cover continues to thin at the same time that precipitation increases. Might it be possible to make a statement about the necessity of capturing correctly the snow properties on ice in order to capture the proper evolution of the ice cover into the future at the end of the paper? (i.e. strengthen and specify statement starting P1515 L28 'In particular, ...'

6. Is it possible to comment on the impact of the uncorrected bias in snow cover on the evolution of future Arctic sea ice simulations in CCSM4? What would be the result in longer simulations if the thermal conductivity of the snow was more similar to what is presented here? Or if the atmospheric snowfall bias was corrected?

Following are comments to specific lines in the text. (P = page, L = line).

P1498 L15, 0.15W/m/K may not necessarily be "more physical" for this kind of model, depending on the processes that are implicitely included in the snow thermal conductivity parameter. This is especially true if the snow thermal conductivity in the model is an "effective" thermal conductivity, that is increased compared to the actual one to account for latent heat transfers through water vapor displacements. Besides, we believe that the thermal conductivities studied in Sturm et al. (2002) were mainly for dry snow. Snow on sea ice is frequently wet and more conductive. Therefore, in a simple model where a single "bulk" value is used for thermal conductivity, this value might well be realistically larger than 0.15W/m/K.

For paragraph staring P1498 L16: (a-d below)

a. P1498 L16 Phrase 'in varied previous investigations of the net role of snow depth to the sea ice mass thermodynamics' is a bit awkward. Suggest removing 'varied'. In

same sentence, remove phrase 'in use and under continued development'.

b. Suggest moving Sentence/para starting P1498 L25 'Due to the coupled...' to before P1498 L19 sentence starting with 'As such...'. This sentence represents the reason that the GCM is used for this study and so it should be highlighted in the paragraph explaining the motivation.

c. P1498 L19: 'validates' may be the wrong word to use here, partly because its meaning in the scientific literature is not consistent, and partly because the paper later states there is a persistent bias in the measurements, and in fact the comparison is not very good. We suggest using the word 'compares' or 'evaluates' throughout the manuscript.

d. P1498 L24 Delete sentence 'Additional evaluations...' unless the results of these standalone simulations can be summarized in a phrase here.

P1500 L5 and 1508 L9 - This is repetitive with respect to neglecting evaluation of sea ice albedo.

P1503 L13 'hindcasts'. The CCSM4 runs used are not 'hindcasts' in the sense that they are not forced with some set of realistic observational conditions. It would be clearer to use the descriptors 'historical' or 'historical radiative forcing' consistent with the description of these simulations for CMIP5 throughout the manuscript.

P1505 L25 Numbers (54% and 49%) seem like they should be the same as stated later in the paragraph (51% and 48%) since the text indicates they refer to the same thing.

P1506 Paragraph starting with L3: The snow stake measurement versus model differences are compared to the transect measurements versus modeled differences. The model depths are said not to be significantly different from the transect measurements in the previous paragraph. The sentence 'The significance of these comparisons is very similar to the transect comparisons, with July failing the significance test.' Rephrase. It is unclear whether all differences are significant except July, or whether all differences are not significant except July.

P1507 L10 'Model produces snow that is too dense.' If the density is defined as a parameter then the density is pre-determined by this parameter. Restate as 'the use of a constant parameter for snow density results in underestimate/overestimate ... compared to observations.'

Paragraph starting P1508 L3 How does CCSM4 make use of the advanced treatment for snow albedo and the delta-Eddington scheme if the snow properties are treated so simply?

P1511 L6-8 Did the authors observe any changes in summer ice melt (surface or bottom) in the experiment?

P1511 L13 'As Fig 6 shows ...'. A statement is made here that the there is an offset to the perturbation in conductive flux at equilibrium by the increase in snow depth without quantifying it. It would be helpful to know how much the additional snow depth at equilibrium modifies the thermal flux. Perhaps you can show on the same figure the conductive heat flux for the same period in both simulations. (Or reference difference in heat flux in Fig 10).

P1511-1512 Figure 8 does not specify the area over which the surface temperatures or long wave energy fluxes are averaged. It would be better to make it explicit in both the text and figure caption.

P1512 L13 Paragraph for Fig 9. (a-e below)

a. Fig 9 would probably be clearer if the budget terms on the right axis were timeseries of each term instead of cumulative amounts initialized from the start of the experiment.

b. It is difficult to understand how the 'volume tendency' on the right axis relates to the volume curve on the left axis. Furthermore, the volume curve on the left axis is a difference between two simulations, while the budget terms are from only the bias simulation. In order to understand the volume difference between the simulations,

please explain why it is not necessary to look also at the difference in the budget terms.

c. What do the signs on the right axis mean? E.g. ocean melt should generally be a negative term in the budget, but here the cumulative curve becomes positive.

d. Holland et al., 2010 (Climate dynamics) seems to have much simpler figures associated with the ice mass budget terms, and Figure 5 in this paper may be a better model for how to show the data for this figure. http://www.cgd.ucar.edu/staff/mholland/papers/holland_etal_simba.pdf

e. P1512 L16 'When congelation ice forms...' This sentence suggests that the formation of congelation ice causes the flux of heat to the atmosphere to be greater than the flux from the ocean. Rephrase it so that the flux is shown as the driving mechanism for the formation of congelation ice.

f. It is not clear what is meant by 'relative volume tendency'. Volume tendency would be the slope of the volume curve. It changes from roughly constant to increasing at about year 25. The slope is reduced from about year 35. The ocean melt curve changes at about year 30, is this associated with either of the changes in the volume tendency? Cumulative ocean melt becomes less negative (implying decreasing melt?) at about year 30. Is the line supposed itself to indicate the contribution to volume tendency or does the slope of the line indicate the contribution to the volume tendency? Again - it seems it would be easier and clearer if the time series of the budget terms were shown rather than the cumulative budget terms.

e. It seems it would be easy using the surface shortwave fluxes to demonstrate the it is indeed the ice-albedo feedback changes that explain most of the ocean melt tendency changes.

P1512 L28 The first line of this paragraph doesn't make sense as a comparison 'less variable' is drawn between Fig 9 volume annual mean timeseries to a 20 yr average climatology of fluxes. On inspection, the climatology of the conductive flux in Fig 10 is positive in Nov Dec Jan, but a comparison to Fig 8a shows it to be negative in those months. Are the signs switched in one of these plots?

Paragraph starting P1514 L3: Say something here about the effects of a transition to a thinner ice regime in the future.

Paragraph ending at P1515 L3 Can a comment be made here about the high ice volume and especially high summer ice extent in CCSM4 compared to other models? Can you estimate the effect of the snow depth bias on this quantity?

P1516 L12 'This study demonstrates ...' This sentence does not necessarily add to the conclusions here.

Conclusions: This section can be tightened and focused in response to the General Comments (1-3) suggested above.

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Technical comments

There are a fair number of obvious technical/grammatical/usage errors in this manuscript. We have included the errors we caught in the following list of technical suggestions though it is not likely complete. It is strongly advised that the authors check carefully and thoroughly for such errors before submitting a (revised) manuscript.

P1496 L9 'conductive flux': Specify that this is through the ice-snow column or from the ocean to the atmosphere.

P1497 L15 Delete 'historic' - it does not necessarily apply to the studies cited below.

P1497 L27 Move sentence starting with 'Later studies...' to the next paragraph.

P1498 L1 paragraph: Need parentheses around several reference dates (eg Brown and Cote (1992)). There are two such errors in this paragraph. Also need 'et al.' with Lecomte and Cheng references in this paragraph. Check on all references for these

errors throughout manuscript.

Paragraph starting P1500 L3 : 'In addition' or 'additionally' is used three times in this paragraph. Revise phrasing.

P1500 L3 Rephrase 'As CICE continues to be developed' to 'Since CICE is under continual development' for clarity.

P1500 L7 Brigeleb should be 'Briegleb'.

P1501 L9 Move and change first sentence to start with 'In order to save computation time, the fully active ocean component POP2 is replaced with a SOM in the sensitivity experiments described in Sec. 4 and 5.

P1502 L25 Change 'CRREL managed' to 'IMB buoys managed by CRREL'

P1503 L13 Change 'assert' to 'expect'.

P1504 L10 ... daily (the) snow stake ...

P1504 Sec 3.3: 'Unsurprisingly' is used three times in three paragraphs. Rephrase.

P1504 L26 Change the word 'significant', as it is not used in a quantitative context here, and the statement is inconsistent with the statements on P1505 paragraph starting with L3.

P1505 L7 Change 'any' to 'all ice' to be consistent with description on P1503 paragraph starting from L21.

P1507 L2-3 Add 'given the assumption that the density parameterization is correct in the model' to the sentence ending with '... would imply' for clarification.

P1508 L1 Delete comma and replace with 'and'.

P1508 L3 Delete 'simulation'.

P1508 Paragraph starting L12: Please add the units to the variables U, h, k discussed in this section for clarity. Additionally, we think that the written definition for thermal transmittance should have the 'h' added after 'given thickness, h'; The 'h' should be deleted after the definition of thermal conductivity k since it is already a quantity per unit thickness. The sentences should therefore read:

'To better understand this process, it is useful to consider thermal transmittance, Utotal in Eq. (1), which is the rate at which energy is transferred through a barrier of a given thickness, »> h «<. Thermal transmittance is useful to consider here because there are two barriers present in the sea ice system, ice and snow. Thermal conductivity, k, is the rate of transfer through a barrier per unit thickness, »>DELETE 'h'«<, and is a characteristic of the material.'

P1509 L13 'alternations' should be 'alterations'

P1509 Eq. (2) We think the "k_snow" below "h_ice" is a mistake (should be "k_ice").

P1509 L8 Should 'Eq.(3)' here actually be a reference to Eq (2)?

P1510 L6 Change 'is likely a lesser effect' to 'method likely results in a smaller effect' and add 'proper consideration of' after 'than' so it reads: 'This method likely results in a smaller effect on snow conductivity than a proper consideration of the more complex relationship between density and conductivity, but our results still reflect the role of the more complex relationship.'

P1510 Paragraph starting L12: Start para with sentence 'This Section focuses on ...'. And then describe other sensitivity tests performed and a synopsis of the results.

P1510 Paragraph starting L18 Change verb tenses from future to present.

P1510 L25 'Following... ' Rephrase this six-line sentence.

P1512 L7 Should ref be to 'Figs 6 and 7' instead of '5 and 6'?

P1513 L4 Missing symbol for statistical significance.

P1513 L2-3 'flux in the summer ocean heat flux' and 'This results in an annual average 6 Wm2 lower mean flux' are unclear. State where the flux is from and where it is to.

P1514 L22 'Snow conductivity of the snow'. Fix.

P1514 L23 'thinner ice pack' should probably be 'thinner snow pack'.

P1515 L8 'We determined two checks' might be better stated as 'There are two processes that prevent a continuous increase in ice volume as the experiment approaches equilibrium.'

P1516 L10 'effected' should be 'affected'.

P1517 L2 Add 'on sea ice' at end of sentence to distinguish from the land snow model.

Figures:

General suggestions: a. Remove titles from the figures as they have unexplained abbreviations and are not always consistent with the captions. In Fig 8, they are not consistent with the caption which states that the plots are differences between the bias experiment and control.

b. Fix aspect ratios in Fig 2 and 7. Fix overlap at edge of grid (red lines passing south through Greenland).

c. Fix spacing on x-axis labels with months.

Fig. 3 Error bars : What are these? Standard deviations?

Fig. 5 Simplify legend - label each of the four curves explicitly.

Fig. 7 Caption for (B) panel : 19%, not 90%; Put colorbars below plots. Reverse colorbar for B so that positive differences are red.

Fig. 8 Caption for (A) panel, "Surface conductivity" is wrong with respect to the units given after (W/m^2, units of a flux). '... indicates greater flux to the surface' from where to where? Colorbar of (A) needs additional unit of precision. Why are colorbars positive to negative?

Fig. 10. Direction of fluxes is not clear. Please clarify in the legend and/or caption: Make 'LW flux' legend consistent (e.g. 'up' and 'down') and state where is measured (atmosphere ice surface?). Make clear what is positive flux for each legend item. Make clear where 'ocean' flux is - e.g. 'ocean to ice flux'. Title should be 'equilibrated' (spelling). Over what area are these averaged? Again - Fig 10 sign of longwave flux out does not agree with sign suggested by Fig 8 (c).

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End of review

Interactive comment on The Cryosphere Discuss., 7, 1495, 2013.