The reviewer contributed a rather extensive review. We respond first to the general concerns.

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 recentmeasurements 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.

The first major concern raised by the reviewer is the omission of other sources of in situ measurements. The authors did consider the Ice Mass Buoys, and discarded this data set due to the high fraction of measurements reported in Fram strait. Because this region has a high variability when considering the presence or absence of ice, there is potentially a high distance between the buoy locations and the matching model grid cell. While it would be possible to simply remove the Fram strait region to include the IMB data, I believe this leads to cherry picking data in comparison to a 70N limit which is satisfactory when used with the drifting ice stations. As it stands, a comparison of CCSM & the IMB is available in appendix A of Blazey 2012. Figure R1 below is from Blazey 2012.

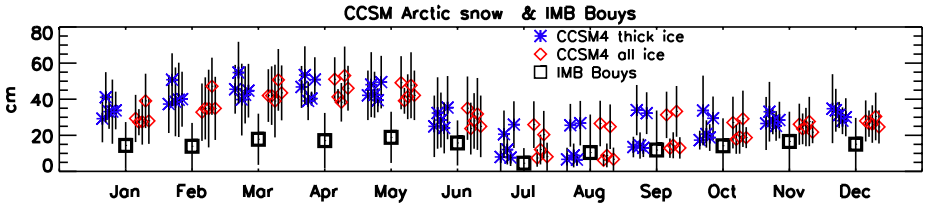

Figure R1. CCSM Arctic Ocean snow depths for the period 1993-2007 validated against IMB buoy measurements. Black squares indicate the *in situ* values for the period, red diamonds indicate the snow depth overlaying the nearest ice of any thickness, blue asterisks indicate the snow depth overlaying the nearest ice with greater than 1.49m thickness. (Blazey 2012)

The reviewer also cited the IceBridge data as another possible source of snow depth measurements. We believe that the IceBridge data would be another good source of snow depth measurements for validation. However, we assert that the extent of the Russian in situ measurements is sufficient for purposes of this validation. In any case, due to the matched snow depth and ice thickness measurements we believe the IceBridge measurements may be of great use for building a parameterization between ice thickness and snow redistribution as suggested in the conclusions section. However, that effort presents a significant effort and is beyond the scope of this study.

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.

The reviewer takes issue with the title "Arctic Ocean Sea Ice Snow Depth Evaluation and Bias Sensitivity in CCSM" implying that albedo effects will be considered. The authors were careful to include the "Depth" in the title. Beyond a few cm, the depth does not profoundly impact the surface albedo. The authors would be willing to make a change to the title such as "Arctic Ocean Sea Ice Snow Depth Evaluation and Bias Dependent Thermodynamic Sensitivity in CCSM", but would prefer to maintain the title as it is.

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.

The reviewer points to a confusing discussion and explanation of figure 9. I have made changes to the discussion and figure caption, which I believe improves the impact, usefulness, and clarity of the figure in the original form. In general, in the original text we did not make it clear that both axes are the difference (Bias experiment less control) I think with this clarification many of the reviewer's concerns are resolved. Some of the specific comments on this figure were also addressed in the text. We did consider both the cumulative and time series budget terms (as in Holland et al 2010) for this figure, but found that the comparison between volume difference and cumulative mass balance differences is more illuminating. We hope that the changes to the text make the comparison more clear.

Attached is a figure from the thesis Blazey et al 2012, which demonstrates the late summer ice area and was originally used alongside figure 9, but was removed before submission. I think with the comparison to figure 5 equilibrated ice area the figure is unnecessary, but provide it here for consideration.

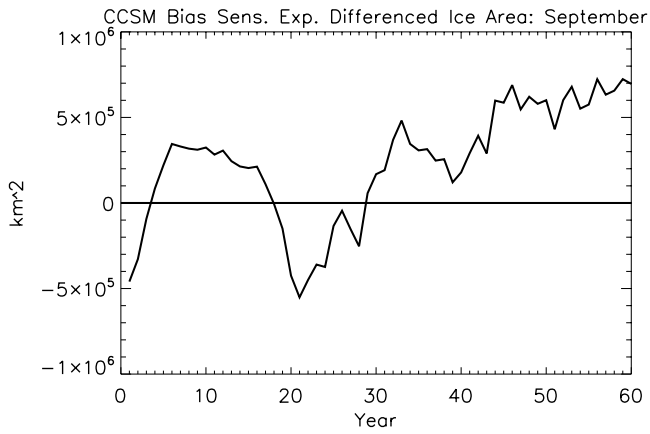

Figure 17. September ice area difference between CCSM bias sensitivity experiment and control, with a 5 year smoothing.

General reviewer suggestions:

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.

These suggestions have been incorporated to highlight the impact of the work, however I think some of the work suggested would better comprise a separate study. The lead author has done much of the work regarding the impact of future snow changes, see chapter 4 Blazey (2012), or Hezel et al (2012). Also, while precipitation in the arctic does increase, snowfall does not, at least within CCSM Blazey (2012) http://132.246.11.198/2012-ipy/pdf-all/ipy2012arAbstract02431.pdf

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, densi-fication, 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.

As outlined in the response to the reviewer's general comment, the in situ data used presents a spatially and temporally extensive data set. Some of the other data sets suggested were ruled out for specific reasons, explained in depth here and in Blazey 2012. I think the reviewer's concern that many processes and biases in the model are unexplored is valid, however I think that exploring all of them is beyond the scope of this

study. Rather this study tries to highlight a bias identified and the impact said bias has on the model. Hopefully this study serves to motivate future model improvements and processes studies.

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

There was one major issue with the figure (lack of clarity that both the volume and mass balance terms are differenced experiment from control). This clarification resolves many of the individual issues. Some additional text and clarification were added as detailed below.

1. Abstract: P1496 L12-17

The text was reordered and reworded :

"Our results indicate the importance of an accurate simulation of snow on the Arctic sea ice. Consequently, future work investigating the impact of current precipitation biases and missing snow processes, such as blowing snow, densification, and seasonal changes is warranted."

2. Treatment of thermal conductivity/resistance.

Reviewers #1 and #4 requested a bit of clarification and simplification in this section as well. In general, I tried to clarify this section (experimental design) to be more illustrative of the method.

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.

The timestep is 30 minutes, and the timestep is made explicit. The timestep is now included in the CICE description, and frequency of adjustments to conductivity was added to the experimental description.

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

The slab ocean model is serves as a heat reservoir and is largely independent from the POP. However, I believe terms like density, specific heat, and albedo are unchanged. Text was added to indicate the simplicity of the SOM

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

Agreed. This change was incorporated throughout the text.

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.
&
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? …"

Added text and modified following passage slightly: "As sea ice thins, the snow is potentially a larger component of the snow-ice column, increasing the impact of biases in snow depth. Given the bias and model reaction described in this study, correction of the current bias may result in slightly slower decline in sea ice than currently projected."

Specific Comments:

P1498 L15, 0.15W/m/K may not necessarily be "more physical"

Changed "more physical" to "the measured value"

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

A-C. Made requested changes

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

I would like to retain this passage as it indicates this foundational work was preformed, if published only in thesis form. Added text: "These simulations found consistent ice sensitivity to decreases in snow depth and a mean state dependent response to increases in snow depth."

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

One of the other reviewers requested more text relating to the albedo treatment. The two passages are well separated and remain in the revised text for emphasis. Given the reviewer's concern with the potentially misleading title, I believe this is appropriate.

P1503 L13 'hindcasts'.

The revised text uses historical forcing simulations (hindcasts) at first mention, but uses hindcast for passages such as "the hindcast snow depth" I feel that historical forcing simulations tends to be unwieldy, but can make the change throught the text if necessary. In the revised form I think it is clear what is meant without unnecessary verbage.

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.

There are separate sample sets for comparison to the Russian state and transect. I clarified the text here, and additional description of the statistical method was added at the request of another Cecilia Bitz.

P1506 Paragraph starting with L3: … Rephrase. It is unclear whether all differences are significant except July

Clarified in the new text (All months expect July are significant)

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.'

This change was incorporated in the new text.

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?

The intent here was to indicate that the DE is perhaps over capable relative to the snow treatment, as the reviewer suggests. An emphasizing phrase was added.

Added the text: "This is partly in light of the fact that CICE currently has a fairly advanced treatment of snow albedo ***despite a simple treatment of the snow mass balance***"

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

Figure 9 is now clearer and shows small changes in surface melt, with reductions in bottom melt. Presumably these occur in summer, which is corroborated by figure 10.

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).

Text modified

"and also inhibits conductive flux in the later period of model integration **as elaborated in the following discussion**"

P1511-1512 Figure 8 does not specify the area…

Added this text where relevant: "in the study area,70N poleward"

P1512 L13 Paragraph for Fig 9. ….

I believe there was a fundamental lack of clarity in the figure description, which has been remedied, see response earlier.

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?

I do not agree with the reviewer's interpretation here. I think trying to compare the two plots in this way is unnecessary, as they were both included to make the interpretation easier.

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

I would prefer to leave the summary paragraph here succinct, and have added some implications text in the conclusion.

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?

I would prefer to leave discussions of the CCSM4 mean state to other studies, and in the case of this bias, correcting it actually makes the CCSM4 ice extent worse. I prefer referencing the physical motivation for determining and simulating the correct snow depth

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

The sentence is included to motivate future investigations of snow processes, as that is one of the primary impacts of this study.

Technical comments….

Most of these edits are incorporated in the edited document, some are stylistic more than technical and have been modified either per the reviewer's suggestion or rewritten entirely for better clarity.

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.

Some captions have been changed, in general I think they are helpful and remain in the current text. In Fig 8 the captions were changed to match the text

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

The aspect ratios of 2&7 more closely match in the updated draft. Removing the artifact without negatively impacting the figure is beyond my ability. In the updated draft I can remove it via brute force, but it generally looks more unprofessional.

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

I am unsure what the issue is. Months seem to match correctly with the points on the plot.

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

The reviewer is correct, they are standard deviations, added to text.

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

Simplified and labeled independently

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

Corrected, The colorbar is in the same position as fig 2, I prefer positive differences to be blue (more ice = blue is intuitive)

Fig. 8 Caption for (A) panel, "Surface conductivity" is wrong with respect to the units given after (W/m2, 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 positiveto negative?

The confusion was addressed in the captions. The orientation and precision can be corrected if necessary.

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).

Clarified, as per requested. "As in Figure 10, positive anomalies indicated more flux to the ice **or less flux from the ice**"

Blazey, B A, 2012: Snow Cover on the Arctic Sea Ice: Model Validation, Sensitivity, and 21st Century Projections, Ph.D. Dissertation, University of Colorado, USA

Hezel, P. J., X. Zhang, C. M. Bitz, B. P. Kelly, and F. Massonnet (2012), Projected decline in spring snow depth on Arctic sea ice caused by progressively later autumn open ocean freeze-up this century, Geophys. Res. Lett., 39, L17505, doi:10.1029/2012GL052794.