MOSAiC SnowModel

Reviewer 1

This article presents an application of the use of snow and sea ice models (SnowModel-LG and HIGHTSI) with assimilated observations from the MOSAiC campaign to produce a continuous time series of snow and sea ice data at the location of the MOSAiC Central Observatory. As the article discusses, although MOSAiC has many high-quality observations, the campaign nevertheless occasionally experienced unavoidable data-collection interruptions. In this work, SnowModel-LG, a model used to produce snow on sea ice, is run in a 1D configuration and is used to provide input to HIGHTSI, a 1D thermodynamic sea ice model. The result is a 3-hourly time series of simulated snow and sea ice properties which helps fill in observational gaps during the MOSAiC campaign. The residual term in the SnowModel-LG budget, *D*, is found to correlate well with sea ice deformation.

In my view, this work is of interest to the scientific community, and I believe that the methodology of this study is sound. MOSAiC has a suite of measurements which are very well-suited to be used as assimilation for a model in a 1D configuration. SnowModel-LG is a widely-used snow-on-sea-ice model with detailed representations of snow processes, and the use of HIGHTSI enables the modelling of sea ice in conjunction with snow. I find it encouraging that even after interrupted observations, SnowModel-LG and HIGHTSI show high fidelity in representing snow and sea ice conditions during MOSAiC once observational corrections are applied. This study also provides some very scientifically relevant insights relating to fine-scale climate-relevant processes, and how climate-model representations of such processes could be improved. The manuscript is well-structured and generally clearly written, and the scientific conclusions follow clearly from the results. The one outstanding point for me is the availability of the data, which has not been provided with the preprint, though I recognize that the authors have stated that it will be available following publication. There are also just some minor points where I think some additional clarification would be beneficial, which I list with my comments below.

General comments:

- The snow density observations are linearly fitted before being used as input to SnowModel-LG. Although I understand the necessity of such a fit (given the high spatial variability relative to the seasonal evolution, as discussed in this work) and I agree with the use of it here, I would appreciate seeing some discussion of the possible biases which may be introduced from using this approach.
- Data availability: This section is currently incomplete, please include specific references for all datasets used. Also, is the model source code publicly available?

Specific comments:

1. Figure 5: I appreciate the authors wanting to convey all possible information and very much understand the difficulties in presenting a variety of information in a single plot, but Figure 5 is somewhat difficult to read for me. Some of the lines obstruct the points in such a way that it is difficult to see the points themselves, particularly if they’re overlapped by a dotted/dashed line while also containing a white dot. I suggest possibly removing the grid (or moving it to the background behind the points so that it doesn’t obstruct them). Possibly also the fit lines could be rendered as solid lines and moved behind the data points? Since

the bulk densities calculated from the individual cutter measurements are shown, perhaps the individual cutter values could be left out and shown in a supplement instead. Regardless of other possible changes suggested here, I do strongly suggest extending the plot vertically or otherwise adjusting the plot so that the legends do not overlap any data points. Otherwise, I will ultimately leave the choice of what to do here to the judgement of the authors.

2. Figure 7: I appreciate seeing the time series and scatter plots here, but regarding the scatter plots, it's not surprising to me that the assimilated values correlate well with the model. However, I am curious about the performance for the melt season (non-assimilated) values. Could you provide correlations for the melt period values alone? (Or would n be too small for this to be meaningful?)
3. Figure 7b): Snow density in mid-late July appears to fluctuate rapidly; is this due to artefacts from intermittent periods of bare ice? Would appreciate a brief comment on this. Also, just to be clear, is this the corrected density modified by the density correction parameter (line 313-315)? I am curious how large the correction was, here.
4. Figure 9: Since you make reference to some of the events shown in coloured shading in Fig 8 while describing this figure also, possibly consider adding the coloured shading to this figure as well. I would also appreciate more specificity in the captions and/or legend what is observed vs. what is from the model.
5. Figure 10: "some observations during quiescent periods are skipped in the Nloop" could you elaborate on why was done?
6. Line 170: How many measurements were included in this average?
7. Line 183: When you say 243 measurements were selected, do you mean to say that you applied some selection criteria? Or is this just all the bulk snow density measurements in the sites you're examining.
8. Line 309: I know you say that the plots for other ice types are similar, but I would appreciate still seeing them, perhaps in a supplement.
9. Line 340: Could you clarify how 1 standard deviation of snow depth is defined here? E.g. is this one standard deviation with respect to the average over the entire season?
10. Line 432: The reasoning as to why precipitation observations have low enough errors for this to be detected is not entirely clear to me. Are you saying that this follows from the fact that there is a strong correlation between the derivatives of D and total sea ice deformation?
11. Line 577: If around 50-60% of D can be explained by deformation, could you comment on what could be attributed to what remains of this residual? In particular, do you expect it to be attributable just to error or are there additional processes not being considered?

Technical corrections:

- Reference section: Several of the DOIs in the references appear to have formatting errors (e.g. the doi.org URL is repeated twice), which occasionally also breaks the hyperlinks in the article.
- Line 35-36: This sentence is confusing to me as it's currently phrased, did you mean to say that the drift of the expedition is shown in Fig 1?
- Line 140: survided -> survived
- Line 185-186: either should be "SMP measurement sites." or "SMP measurements."
- Line 377: SnowModel-LG misspelled
- Line 416: "affect" should be "effect"
- Line 461: phenomena -> phenomenon

Author’s Comment 1

Dear referee,

Thank you very much for your review, comments and suggestions. All your comments will be addressed, references improved and simulation data and code provided/published accordingly in the revision.

Response to the general comments:

- Both reviewers requested more discussion on the point of the snow density in-situ measurements. In the revision we will provide more discussion also on the potential biases, as requested here. This section will however remain short and we prefer not to add more information as supplements.
- A documented version of the identical code used here was published as part of the Mower et al, 2024 paper (https://gmd.copernicus.org/articles/17/4135/2024/). We will use the same Zenodo code repository as them. In addition, we will provide the model simulation results and python analysis scripts in separate Zenodo publications.

Response to the specific comments:

1. Figure 5 which represents the above mentioned in-situ snow densities is of high interest to the audience and it will be improved also based on the reviewer’s recommendations
2. Figure 7 has melt period correlations excluded from the statistical analysis. We will verify the corrections are statistically significant and report on that in the revision.
3. Figure 7b and late July fluctuations: after 7 May no more observations are assimilated and there is no bias correction used here. The fluctuation is a result between bare ice situation (zero density) and density estimated for a small amount of snow that has fallen on a given day (typical value 230 kg/m^3) – which promptly melts away under high atmospheric temperatures soon after the snowfall.
4. Figure 9 will have the event shading added.
5. Figure 10 and exclusion of the Nloop measurements: Nloop was sampled so often that it had measurements in between any weather or sea ice dynamic event. Those measurements were not used. This will be better explained in the text.
6. Line 170: This number depends on the individual transect. For example, Sloop had about one third area covered by level ice. The entire transect had at every sampling between 1000 to 1500 measurements (depending on the sampling distance by Magnaprobe). This would result in about 300 to 500 snow depth measurements on the level ice. The mean and standard deviation from these measurements were used in this paper to guide the model simulations. This also explains the reviewer’s question under point 9.
7. Line 183: These were all measurements used in this paper. Some measurements did not pass the quality control due to missing values, unique sampling locations etc.
8. Line 309: We will add the other location calculations to the response to reviewer.

9. Line 340: This is now explained under point 6 above. We will improve the manuscript description so that this will be clear to the readers.
10. Line 432: We are convinced about the quality of the precipitation data because of the analysis done by Matrosov et al 2022 (basic assumption in line 62) AND because the difference is small, has interchanging sign (positive and negative bias) and that coincides with how deformation processes could affect the local snow mass balance. This will be made clear in the revision.
11. Line 577: We expect that deformation is the last major missing process.

Thank you for all the technical corrections, they will be all applied!

Thank you also for your work and time. Your comments will greatly improve this manuscript.

On behalf of both co-authors,

Polona Itkin