

Interactive comment on “Atmospheric winter conditions 2007/08 over the Arctic Ocean based on NP-35 data and regional model simulations” by M. Mielke et al.

Anonymous Referee #2

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The manuscript reports on meteorological observations from Arctic winter obtained at the NP35 drifting station and uses these observations to evaluate the regional climate model HIRHAM. Both the new data and the model evaluation address substantial gaps in our understanding of Arctic weather and climate, making the manuscript a very valuable contribution. However, some conceptual weaknesses in the model evaluation and unclarity in the presentation of results are yet to be addressed. I therefore recommend the manuscript for publication after major revision.

Major issues

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1. Model runs

The authors use different model runs alongside each other without always making clear what to expect from each type of run. Essentially, the model run in forecast mode is a weather prediction model, which can legitimately be asked to reproduce the observed day-to-day variability, whereas the climate model version cannot be expected to accomplish that. Comparing, for example, the correlation between both types of models and observational data for individual variables is therefore confusing and does not lead to a clear assessment of model quality. I was also surprised by the weight given to the different initialisations – I would expect a purely atmospheric model in climate mode to ‘forget’ about its initial state relatively quickly. Is the “climate” run the only ensemble member that represents the two atmospheric circulation states, or do individual other realisations also capture this? Why are the climate runs initialised every month instead of using a continuous run for the entire winter?

2. Use of monthly means/synoptic situations

Most of the model-data comparison is based on monthly means, despite the authors noting the existence of distinct states of the Arctic boundary layer for both observations and model output. It would be more enlightening to plot, for example, atmospheric profiles conditioned on the surface net longwave radiation. This might also help to connect the discussion of temperature profiles, deficits in cloud representation and low-level inversions, which at present appear somewhat unrelated to each other.

3. Explanation of model biases

The authors claim that biases in temperature profiles are caused by vertical mixing and explore this with a sensitivity experiment using different stability functions. It is indeed known that models tend to overestimate turbulent fluxes under stable stratification and therefore struggle to represent sharp gradients in temperature inversions, but the authors do not address why this should lead to too much mixing in some and too little in other months, nor do they explore alternative explanations, which could for example be

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linked to the cloud issues mentioned in the paper and /or surface conditions.

4. clarity of presentation

Greater care must be taken to ensure the reader is always aware which type of run is being compared to which, what data over what timescales is being used for an analysis etc. (e.g. Is RH always w.r.t. water? Is Fig. 7. based on daily averages? What are the definitions used to count inversions and low-level jets? Is inversion height the depth of the inversion or the height of the inversion base?)

Specific comments:

5. p. 11856 ll13 ff: This sentence doesn't quite work – should it read “impact” rather than “strength”?

6. At least one of the original papers reporting on the SHEBA measurements should be referenced in the introduction, eg. Persson et al. 2002

7. I recommend to refrain from using past perfect in the description of data acquisition and handling (mostly pp 11859-11860).

8. p. 11861, l.1: For which surface are the cited roughness lengths used? What about other surface types?

9. p. 11861, ll.8ff: As the stability functions are unity by definition at $Ri=0$, I would rather write: decrease more strongly with increasing stability.

10. l 10: suggestion: 1.5-order (instead of “higher order”)

11. l 20: How is snow on sea ice handled in the model?

12. ll.22 ff: Having read the paper and especially this section several times, it is now clear to me which run corresponds to which setup. I am afraid this might not be the case for the first-time reader yet. Please also make sure to explicitly state which setup is used for HIRHAM b10 (the same as for HIRHAM clima, if I understand correctly).

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Throughout the paper, it sometimes remains unclear how b10 is compared to other runs – my understanding is that it should consistently be compared to HIRHAM clima while checking whether the difference is greater than the ensemble spread in HIRHAM ens.

13. p. 11862, l. 8: At which times is the forecast model initialised?

14. p.11864, ll 10 ff: How would an atmospheric model fail this test? And what are the units of the colourscale in Fig. 4?

15. p. 11865 l. 19: I recommend to not use 'significantly' other than for statistical significance tests.

16. p. 11866 l.11 'have a clear impact' -> please describe and explain the impact of changed stability functions of your profiles.

17. ll. 12 ff “Based ... months” The purpose of this statement is unclear to me.

18. p. 1167 I strongly believe these are very important results, but unfortunately it is not clear to me what is being shown in Fig. 9 (cf. Point 4). It might also be helpful to coarse-grain observations to a vertical resolution comparable to that of the model.

19. p. 11868 The occurrence of two states as described for SHEBA by Stramler (and earlier by Persson et al. 1999) seems to be a robust feature of the boundary layer in Arctic winter – it is interesting that it is neither captured by the forecast mode nor the ensemble mean. Do you have an explanation for this? Would the picture change if you restricted this analysis to DJF/NDJF?

20. p. 11869 I am not sure I understand Fig. 12. SHF is obtained by multiplying u , ΔT , Ch and a constant, and then divided by u to be plotted against ΔT , such that we are essentially looking at variations of Ch ? How is Ch obtained in Zilitinkevich's bulk parameterisation and how does intermittent turbulence enter SHF as plotted here?

21. p. 1171 ll. 16 f: “due to overestimated vertical mixing of heat in the stable ABL”

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I do not find the presented evidence convincing enough to draw this conclusion (cf. point 3). As pointed out by Tjernstrom&Graversen (2009), the boundary layer in Arctic winter isn't always stable, and biases in the partitioning between strongly stable and near-neutral profiles could also affect the mean.

22. L 25: What is the problem with cloud cover and how could this be connected to the inversion biases described?

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