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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 #1

Received and published: 16 June 2014

The manuscript addresses boundary-layer processes over the Arctic sea ice. The work is based on winter observations from the Russian drifting station NP-35. Such observations from the Arctic Ocean are a rarity. The observations are complemented by extensive set of model experiments, applying various simulation strategies and including sensitivity tests for parameterization of the stable boundary layer. A further strength of the manuscript is that it presents very interesting results on the interaction of boundary-layer and synoptic-scale processes. As a summary, there is really a lot of material for a single paper. This is a strength of the manuscript, but I must also say that I sometimes got a bit disappointed because some of the very interesting issues were

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discussed so briefly. Also, parts of the text do not appear carefully written. I suggest that substantial revisions are needed before the manuscript can be accepted for publication. I do not request more analyses or model experiments, but clearer and, in some places, more extensive explanation and interpretation of the results.

Major comments / questions:

1. The sensitivity tests for the stratification effect are interesting, but the results remain a bit unclear. In the end of page 11870, it is stated that the enhanced vertical stability in HIRHAM b10 reduces the baroclinic scale variability over the Barents and Kara Seas. Looking at Figure 14, it is not so clear for me. This is the case also for the statement on page 11871: “The use of increased vertical stability in the model simulation leads to diminished planetary-scale variability over the Arctic Ocean.”, which is not so evident from the figures. Presenting quantitative numbers for area-averaged values would help.

2. Some of the sentences in the Summary and Conclusions section appear contradictory or at least difficult to understand, or are not enough explained: - Page 11871, lines 8-16: “The observed near-surface temperature, ... are satisfactorily reproduced by the simulations. Significant temperature differences between observations and the simulations occur near the surface” - Page 11871, lines 19-24: “the frequency of surface inversions is overestimated. ... HIRHAM simulates too many elevated inversions compared to the NP-35 data, ..” - Page 11872, lines 6-7: “This feedback changes the synoptical cyclone tracks ...”. The sentence is not enough supported by the rest of the manuscript. Very little is written about cyclone tracks. - Page 11872, lines 18-19: explain more

3. In Section 5, the authors should better summarize what is new in the results.

4. Very little attention is paid on the role of snow/ice scheme in the HIRHAM model. Errors in the heat conduction through snow and ice may have a large impact on the simulated surface and ABL temperatures.

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Detailed comments

Page 11856, Line 13: unclear sentence

P11857, L16-19: Too complicated sentence

P11858, L15: small- and mesoscale features

P11858, L16-18: clarify the sentence.

P11859, L6-7: improve the sentence (“St. Petersburg on a sea ice flow over the Arctic Ocean”)

P11860, L1: for clarity, add “near-surface meteorological parameters”

P11860, L9-14: some repetition of the text on the previous page

P11861, L14: I am concerned of albedo parameterizations based on a linear dependency on the surface temperature. The albedo will give a positive feedback on surface temperature errors. But perhaps this is not critical in the present study where the main focus is on winter conditions.

P11862, L17: what is the positive direction of other fluxes?

P11863, L10-14: clarify what kind of humidity observations were made. Were the relative humidity and dew-point temperature measured independently? Dew point temperature measurements are not mentioned in Table 1. Further, I wonder if the relative humidity is accurate within $\pm 3\%$ also in the lowest winter temperatures (Table 1).

P11864, L6-9: In addition to relative humidity, I would like to see the results for specific humidity. They are directly related to atmospheric moisture budget and therefore easier to interpret than dew-point temperature.

P11864, L19: Clearly explain what is meant by the transient scale. Most of baroclinic cyclones are transient, i.e. non-stationary.

P11865, L25-26: replace “for stronger turbulence” by “when z_0 is large”. Are you sure

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that the dependency is not taken into account in any climate models?

P11866, L4-5: Perhaps comment that these inversion top heights are much lower than the statistics presented by Serreze et al. (2002) for winter. Ideas on the reasons?

P11866, L12: what is meant by largest? Do you mean that the scatter is equally large for all months?

P11866, L24: I would stress more the finding that HIRHAM-f12 beats here the ECMWF model. Any ideas why?

P11867, L2: Reformulate the sentence. ABL can be stably stratified without any temperature inversion.

P11867-11868: It is interesting how similar the problems in simulation of LLJs are to those reported by Tastula et al. (2013)

P11868, L8-9: The sentence is a bit out of the place here, or at least requires some more explanation.

P11868, L12-18: Why did f12 fail?

P11869, L20: Make it clear that Z-1979 is only applied for observations. How sensitive the results based on observations are to the selection of the parameterization?

P11869, L25: add surface roughness in the list

P11870, L15-17: I guess the results presented in Figure 13 include a lot of transient cyclones larger than 50-400 km in diameter. If so, make it clear.

Table 2: How one should interpret the RMS errors of observations? What is the reference?

Figure 4: Explain the colour scale, and briefly explain what is meant by edge effects.

Figure 5: The legend is not carefully written.

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Figure 7: Not clear enough to identify the different colour bars (blue and violet look too similar).

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 11855, 2014.

ACPD

14, C3674–C3678, 2014

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