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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Arctic summer atmosphere: an evaluation of reanalyses using ASCOS data" *by* C. Wesslén et al.

C. Wesslén et al.

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We would like to thank the reviewers for carefully reading the manuscript and providing constructive suggestions and comments. According to the reviewers' comments we have now added one picture, showing the surface conditions during the ASCOS campaign and the spacial distribution of sea ice (close to 90-100%). These conclusions where also made from web-cam photos of the sea ice during the expedition. In the following section, we give our responses to each of the comments or questions from the referees and when applicable, refer to an updated version of the manuscript. We have also included a slightly different version of figure 2 in the new manuscript.

Referee #1

1. Point measurements over sea ice are compared against grid-averaged values of reanalyses. The impacts of this practice should be evaluated for different variables. The impact is probably small for the turbulent and longwave fluxes, because in late summer the thermal differences between sea ice and open sea are so small, but it may be an important issue for the analyses and conclusions on shortwave radiation and surface albedo. This problem is mentioned on page 16504, lines 1-2, but only from the point of view of time averaging, which I did not fully understand. Information on sea ice concentration should be provided, with estimates on its effect on the comparison of point measurements and grid-averaged values. For the momentum flux, the authors already mentioned that observations were made both over ice and open water and the result did not differ significantly.

Reply: We understand that the comparison between grid-average values and point measurements will have different impacts depending on what variable you are analyzing and the chosen time averaging. However, we know that the surface was to 90-100% covered by sea ice and therefore the difference will not have significant impact, when averaging over the area. To show an example, we have included a picture (figure 3) of the area where the ship was drifting along with the ice. This is also a reason why we use statistics. The spacial variations will not have a significant influence on the overall bias, but can have an impact on the correlation on the surface variables. Moreover, the higher frequency data was time-averaged and the averaging time was selected to roughly represent an area average in a Taylor hypothesis sense corresponding to the 30-km grid and a typical wind speed. Additionally, for some of the data, especially the bulk cloud and vapor data, this was so noisy that we selected a longer averaging time. Finally, the statistical analysis is also an attempt to alleviate this problem, assuming that averaging over a large sample implies a convergence of time and volume averages, such that differences in individual samples due to this problem would appear in the results as "noise".

2. It appears that the authors did not apply the full vertical resolution of the reanalyses

(Page 9, lines 19-21), but only the data from isobaric levels. The reason for this selection should be explained, as also the high-resolution model-level data are available, at least from ERA-Interim. It should also be evaluated how the application of the coarse resolution isobaric level data has affected the main results, such as the errors in the cloud thickness.

Reply: We did apply the full vertical resolution of the reanalyses. However, we used the isobaric levels when interpolating from the pressure levels measured by the ASCOS radiosoundings and to gain the geometric height. This has been clarified in the manuscript.

3. I am somewhat confused about the analyses of longwave emissivity. I suggest the authors to check that they have correctly derived the surface temperature from pyrgeometer data, also taking into account the longwave radiation reflected from the surface (see e.g. Vihma et al. (2009), eq. (4)). If taken into account, the results should not be very sensitive to the emissivity used. A second issue is to make sure that exactly the same variables from observations and reanalyses are compared (longwave radiation emitted from the surface or the total upward longwave radiation). Also, clarify the sentence on Page 16516, lines 10-11 (the surface temperature of snow/ice should never exceed 0 deg C).

Reply: We completely agree with this comment, however, we show no results from the surface temperature, only 2 meter air temperature. The longwave radiation is a direct result from the measurements.

As background information, the actual surface temperature was observed during ASCOS with three independent methods: i) Thermocouple observations at several locations immediately below the surface of the snow; 2) Radiometric temperature from the M-AERI radiometer onboard Oden; 3) The method this reviewer refers to, using the upward and downward broadband longwave radiation.

In using 3), an assumption must be made on the surface emissivity, exactly as the

reviewer mentions. In comparing the three methods it was however impossible to reconcile the results using an emissivity much below unity; hence we concluded that the surface emissivity had to be higher than the value used for all the model results, perhaps because new snow or riming and/or frost on the surface might have a higher emissivity than more aged snow.

4. The manuscript remains isolated from previous work on evaluation of atmospheric reanalyses over sea ice. Such studies have been carried out in the Arctic also in the same region and season as in the present study, and some of the results presented in the present manuscript are not new. At least Lupkes et al. (2010) and Jakobson et al. (2012) have observed the warm and moist biases of ERA-Interim in the ABL. Also the results for the vertical profile of the wind speed are worth comparing against Jakobson et al. (2012). The previous evaluations of Polar WRF - the basis for ASR - are mentioned in the Introduction, but a reader would also be interested to see at least a brief summary of the differences/similarities between those and the results of the present study. If the authors are interested in evaluating their results in a broader context, the studies by Tastula et al. (2013) over the Antarctic sea ice zone may also be of interest.

Reply: We thank the reviewer for noticing this. We have now evaluating our results in a boarder context. This is included in the revised manuscript.

5. It would be good to explicitly mention that there is a large positive bias in ERA-Interim near-surface specific humidity.

Reply: This is now mentioned in the conclusions.

6. Beginning of the Abstract and Introduction: are you sure that the climate changes in the Arctic have been larger than those in the Antarctic Peninsula – Bellinghausen /Amundsen Sea region?

Reply: Yes, at least when you consider a larger region, like the entire Arctic or Antarctic,

which was our intention. This has been clarified in the manuscript. We think the jury is out on whether the Antarctic Peninsula has warmed more than the Arctic, or not. Still it has experienced significant warming, which is not the case for “Antarctica” as it is for the “Arctic”.

Referee #2

1. Abstract: The first two paragraphs can be condensed into a single paragraph, and I would like to see the third paragraph expanded to provide more details on the assessment of the new dataset. Also, it seems like it should be stated in the abstract that more improvements need to be made before ASR-Polar WRF can be recommended instead of ERA-interim.

Reply: We thank the reviewer for this suggestion. The first 2 paragraphs have been condensed. However, we leave it to the reader to decide weather ASR can be recommended instead on ERA-Interim, since it sill depends on what region and variables you are interested in.

2. p16496 line 2: "...any region on earth.." define "region".

Reply: In this case, a region is comparable to an entire continent; for example North America, South America, Europe, Antarctica or Arctic. The Arctic region is defined as the area north of the Arctic Circle (66°N).

3. p16497 line 12: Suggest you cite more recent papers too; e.g. (Holland and Bitz, 2003; Karlsson and Svensson, 2013; Liu et al., 2013).

Reply: We thank the reviewer for noting this. The results from Holland and Bitz, 2003, Karlsson and Svensson, 2013 and Liu et al., 2013 have now been included.

4. p16497 line 18: "..at least partly responsible." This seems a bit too strong, as models are supposed to be physically based. I would suggest you phrase as lack of observations inhibits a more thorough evaluation and improvement of model parameterizations.

Reply: The sentence has been corrected according to the reviewer’s recommendation.

5. p16498 line 29: "..most of the data were not assimilated". Which data was?

Reply: The mean sea level pressure (MSLP) and 10m winds. This has been clarified in the manuscript.

6. p16499 section 2.1: are there citations for reanalysis?

Reply: The citations can be found in table 1.

7. p16508 lines 22 and 24: what are the "systematic" errors you refer to? Would they bias the results in one direction or another?

Reply: This was an unfortunate choice of words; there are errors in soundings that are systematically present, but that are not systematic errors; the wording has been changed in the revised manuscript.

We are giving three examples: 1) Low-level wind speeds are affected by input surface winds and assumptions on wind speed profile until the GPS system has dithered sufficient amounts of data to by itself provide a wind estimate; 2) High altitude temperatures can be biased due to radiation effects on the bare sensor; 3) Low temperature (i.e. high altitude) RH is always difficult to measure, and the accuracy of the sensor goes down, but also its sensitivity. If the sonde passes a cloud at high altitude, there is a risk that RH above the cloud will be overestimated, and vice versa. The magnitude of these errors varies with conditions and is therefore difficult to quantify.

8. p16511 line 20: there are numerous other refs that suggest the cloud microphysics scheme you use converts liquid to ice too quickly; e.g. Liu et al. 2011.

Reply: We thank the reviewer for noting this. The reference from Liu et al., 2011 have now been included.

9. p16511 line 22 define "noisy".

Reply: In this case, noisy means that the time series are a mixture of low and high values, with several peaks due to instrumental errors, while other could be explain by synoptic scale features. It is close to impossible to distinguish between these. This is mentioned later in the paragraph.

10. p16515 line 10: are you sure these fluctuations in ASR2 indicate cloudy and clear? It is possible that the model predicts clouds but they are thin ice clouds and don't perturb longwave. e.g. Cesana et al. 2012.

Reply: Yes, I believe it is possible. This is now stated in the manuscript.

11. p16515 line 20, it may be useful to say that it is well known that the cloud micro-physics scheme produces too little liquid (Liu et al. 2011, Prenni et al 2007, Cesana et al 2012).

Reply: We thank the reviewer for noticing this. It is now stated in the manuscript.

12. p16522 line 8: Sub-grid scale processes are at least loosely related to spatial resolution; you might want to re-phrase this sentence.

Reply: This sentence has now been re-phrased.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 16495, 2013.

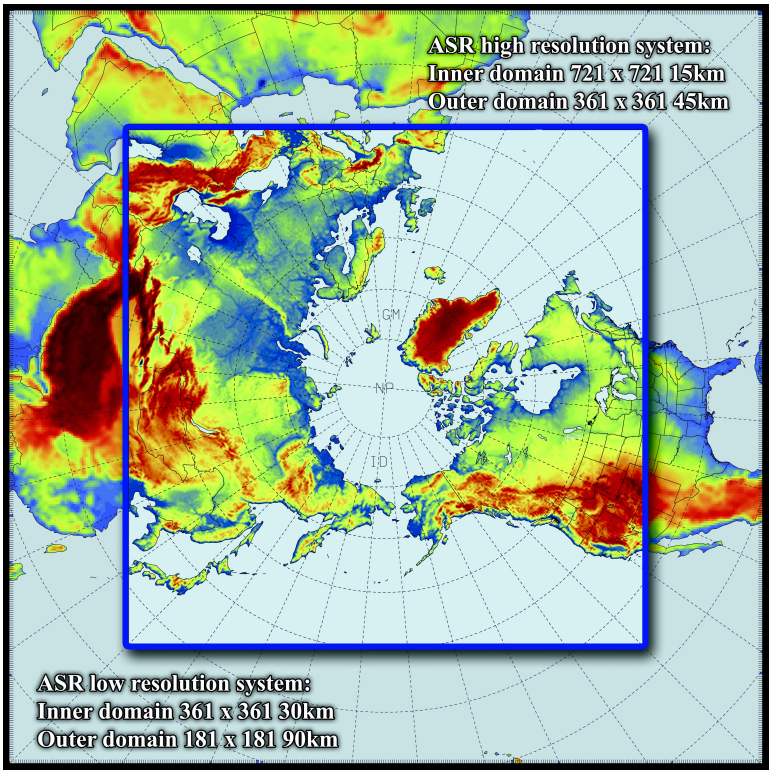

Fig. 1.

Fig. 2.