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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Arctic clouds and surface radiation – a critical comparison of satellite retrievals and the ERA-interim reanalysis" *by* M. Zygmuntowska et al.

M. Zygmuntowska et al.

marta.zygmuntowska@nersc.no

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We would like to thank the reviewer for the useful comments and suggestions which help us improve the quality of our paper. Before replying to the specific points raised by the reviewer we wish to clarify a few general points.

In our study we compare Arctic clouds and their influence on the radiation budget over the Arctic ocean. For this purpose we analyze three popular data products, the Era-interim Reanalysis, AVHRR data from CM-SAF and the 2B-FLXHR and 2B-GEOPROF-LIDAR data derived from CloudSat/CALIPSO. Apart from the modification of the sea ice albedo for the 2B-FLXHR dataset, we did not modify the datasets but

used them as they are, off the shelf. We believe that such a comparison will be useful for understanding the Arctic system, and may also help reconcile contradicting process studies and model evaluations based on different data sources. Our attempt was not to modify or develop the datasets to the extent that they provide the best possible results, but to present them as they are and highlight the strengths and weaknesses in each case. In particular the use of the 2B-FLXHR has been criticized as only clouds detected by CloudSat are included in the radiation transfer calculations, hence the important low level clouds are missing. We agree with the reviewer that it is indeed a very strong caveat to use the 2B-FLXHR dataset within the Arctic. However, to quantify the limitations of the datasets is one of the results of this paper.

The second concern raised by the reviewer regarding the 2B-FLXHR data is the choice of values for the surface albedo in our correction. Not shown in the manuscript is a more sophisticated approach of describing the surface albedo (α) with:

α = 0.15 +0.6 * ice concentration (in a 200x200km grid box) or α = 0.15 + 0.7* ice concentration

Averaged over the analyzed area positive and negative tend to cancel, hence the net shortwave fluxes are mostly bounded by the assumptions based on fixed albedos of α= 0.45 and α=75 for ice-covered pixels. To keep the figure simple only the area enclosed by these values was shown. Assumptions were based on various measurements published by Perovich (1996), as well as results from coupled models (Gorodetskaya, 2007).

Further, the reviewer requests more information about the longwave fluxes at the surface, which is only mentioned briefly in our manuscript. We would like to thank the reviewer for pointing out this issue as our description is indeed misleading. The lower boundary condition (surface) for the longwave upwelling flux is based on the surface temperature and an emissivity of 1.0 (i.e. black within each infrared band). The surface temperature is taken from ECMWF (L'Ecuyer, 2007). The manuscript will be edited to clarify this point.

The next issue raised by the referee addresses the chosen area of study. We used the ocean north of 68 N, but excluded the Atlantic sector south of 78 N. The main focus was set on the part of the ocean which is at usually seasonally covered by sea ice . However, as the Atlantic sector is dominated by the inflow of warmer water masses which differ substantially from the rest of the Arctic Ocean this region has been excluded. On the other hand, we felt that a easily defined region in terms of longitude and latitude boxes is preferable over a more complex definition. We shall attempt to be more precise in our language and hence we will rewrite the formulation 'central arctic conditions' as the use of the word 'central' might indeed confuse the reader.

The reviewer is also missing more explicit cloud definitions to assert if the cloud amount from ERA -Interim can be compared to the observational data sets in this context. We try to address this issue and will give a more detailed description of the cloud definitions and algorithms.

The 2B-GEOPROF-LIDAR data set is based on the level 1B Cloud Profiling Radar (CPR) data product from CloudSat and further the Vertical Feature Mask (VFM) from CALISPO. The CPR product contains the measured return power sampled at vertical range bin of approximately 240m. The 2B-GEOPROF algorithm reads in these profiles of reflectivity and produces a 'cloud mask' containing values which indicate the location of likely hydrometeors. In the 2B-GEOPROF-LIDAR data set this cloud mask is combined with information from CALIPSOs Vertical Feature Mask. This merged data set provides a hydrometeor fraction for every vertical level and additionally up to five cloud tops and bases for every profile. A cloud layer boundary is hereby defined as the first encounter of a cloudy range level followed by a cloud free range level, either radar or lidar . For more information about the algorithm we refer to Marchand (2008), Mace (2003) and the CloudSat Standard Data Products Handbook. In our study we only use information about the cloud top height which is available in the 2B-GEOPROF-LIDAR data set. For low-level clouds all profiles with a cloud top below 3000m were defined

as cloudy (1) and those with no cloud top below this height were regarded as cloud free (0). This has been used to calculate a cloud fraction within a 200km x 200km grid cell. For the total cloud fraction all altitudes have been taken into account.

The used AVHRR data is embarked on NOAA and EUMETSAT Meteorological and operational Weather Satellites (Metops) and the underlying algorithm is the NWCSAF PPS (Polar Platform System). The algorithm uses various spectral and texture features, such as differences in brightness temperatures in different wavelengths or local variability of bidirectional reflectances. Information about the integrated water vapor, surface temperature and the temperature at various pressure levels is obtained from numerical weather prediction models to classify the cloudy pixels. However, in our study we only used information about the cloud fraction provided on a grid with a resolution of 15 km x 15 km. The algorithm is described in detail by Dybbroe et al (2005) and Karlsson et al (2008).

In ERA-INTERIM clouds are described by prognostic equations for cloud liquid water/ice and cloud fraction based on the scheme published by Tiedkte 1993, which is widely used in GCMs. The evolution of cloud water content and fraction is determined by convection, boundary layer turbulence, large scale lifting, adiabatic cooling, as well evaporation and precipitation. Low level clouds are defined as clouds within 1.0 > sigma > 0.8 in a sigma coordinate system.

We acknowledge that clouds and cloud fraction are defined differently in the three datasets simply by the nature of the varying methods used. However, the aim of our study is not to adjust the algorithms or definitions to be more consistent, but rather compare them as they are and understand the resulting differences.

The last comment addressing the conclusions about ERA-INTERIM is well taken. Even though Era-Interim agrees surprisingly well with the measurements from SHEBA, results need to be taken with caution. In particular the resulting cloud radiative effect is very likely high-biased due to the documented dry-bias. We shall rewrite this sentence to better reflect our findings.

Line 5 on page 31504; results from over land are reported, yet on line 17 on page 31500, it is stated that only data over oceans and sea ice are analyzed.

→ the displayed annual cycles are based on data only over the ocean. Values over land are included in figure 3 for completeness, but are not further analyzed and discussed. Therefore it seems reasonable to emphasize that only data over oceans and sea ice are analyzed.

It is mentioned on line 22–24 on page 31497 that clouds might occasionally be warmer than the surface, due to the semi-permanent inversion in the Arctic. I think this is not as rare as the sentence implies, especially not during the Arctic winter when the inversion is very strong.

→ this is right, inversions are a indeed very common. We will rephrase the statement accordingly.

line 12-14, p31503. I suggest to add some suggestions/explanations to why the dataset based on passive instruments detect so few clouds in December and January here.

→ In this paragraph we simply describe the results displayed in figure 2. Concerning the structure of the paper it is not useful to include a discussion at this point. A discussion of the result can be found on the next page, p 31504 line 10-19.

line 23 on page 31506, short wave cloud radiative effect depends to a lesser extent on the presence of “mixed-phase or ice clouds”. I would say on “cloud particle phase”, otherwise it tends to read that ice clouds or mixed phase clouds have little effect on the short wave radiation.

→ We agree and shall revise the text accordingly.

Line 2 on page 31509; A bit harsh I think, the dataset based on AVHRR measurements

does not fail to detect clouds completely as in implied here, albeit it does leave many clouds undetected.

→ We agree and will rephrase this sentence in the suggested way.

Line 2–4 on page 31510, This doesn’t make sense to me: From that sentence I read “CloudSat detects clouds that are too optically thin”. Do you mean the opposite?

→ We agree, of course we mean that CloudSat has difficulties detecting optically thin clouds.

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