Response to Reviewers of: "Near-surface meteorology during the Arctic Summer Cloud Ocean Study (ASCOS): Evaluation of reanalyses and global climate models"

Reviewer 1 Comments:

This paper exploits the observations from the Arctic Summer Cloud Ocean Study (ASCOS) to evaluate different reanalysis products and different climate models. To be close to the synoptic situation during the one month observational campaign and to allow "deterministic verification", the climate models are run in short range forecast mode from reanalyses or are relaxed towards them. Interestingly, most of the observations were not assimilated in the reanalyses, so the verification is really independent. This aspect makes the paper particularly valuable as it provides independent verification of reanalysis in an area where data is very sparse, and where reanalysis is very popular because other data sources are very limited.

The paper is very well crafted, makes many sensible choices (e.g. on interpolation, averaging, spinup, sampling) and presents a nice collection of tables and figures. The study goes beyond the computation of basics error statistic in the sense that it tries to give a physical interpretation of the errors by relating different observables. A key result is the relation between cloud liquid water, radiation and near surface temperatures. I am sure the paper will become an important reference for any study of the Arctic that uses reanalyses. A few suggestions for improvement and correction:

1. At a few locations, the spectral truncation of models is mentioned. It would be good to add the associated grid point resolution.

An estimate of the horizontal resolution, in degrees, has been added in the description of the reanalyses.

2. On page 19429 line 19, reference is made to YOTC as a reanalysis. Strictly speaking the ECMWF YOTC data is not a reanalysis. The ECMWF/YOTC data is from the ECMWF operational analysis (i.e. at the highest resolution) supplemented by products that are not available operationally (e.g. process tendencies from short range forecasts).

You are correct and we have replaced "reanalysis" with "analysis".

3. Models can show spinup in the first few days in short range forecasts, particularly when the model is different from the one that was used in the data assimilation system. With CAM5, this problem was addressed by selecting concatenated 24-48 hour forecasts. The GISS model was kept close to realistic synoptic conditions by nudging of wind towards the R2 reanalysis, which can potentially push the model into a continuous state of spinup, i.e. it can influence the systematic errors. My question is: what is the time scale of the relaxation towards analyses and is anything known about the consequences for systematic errors?

This is a good point. To date, no detailed evaluation of the influence of nudging on

systematic errors has been performed. GISS researchers believe that the impact of the nudging is small and that biases are mainly the result of model physics (vs. errors in the product used for the nudging). While we can not quantify this, we have included a statement in the text that reads: "It is important to recognize that this nudging may introduce systematic errors in these runs tied to any biases present in the analysis used for the nudging. However, it is believed that because the nudging only impacts horizontal winds, that most biases are connected to the physics of the GISS-ModelE2 itself".

4. The description of the observations is short as it has to be in a paper of this type. However, it would help the reader to have a feel of the level of accuracy that can be expected from some of the observations. Most readers will be familiar with standard observations like temperature, humidity and wind, precipitation, but accuracy of more advanced observations like surface energy fluxes and cloud liquid water path is less obvious. Questions are: to what extent does the surface energy balance close? Was Qsfc "observed" directly or derived as a residual of the surface energy balance? What are typical errors in the energy fluxes? What are typical errors in liquid water path observations; is it 100%, 50% or 10%?

It is certainly important to include error estimates. The LWP error estimate was already included in the manuscript (25 g m-2). Additionally, the manuscript included information on what was represented by Qsfc ("Qsfc represents the residual flux, including subtracted conduction terms"). Shortwave radiative flux measurements were corrected to avoid issues with riming and cleaning. Additionally, sensible heat estimates were screened for bad data through vertical velocity and surface temperature variance calculations. Error estimates for the energy balance measurements are included in Sedlar et al. [2011, Climate Dynamics] and we have included a reference to this work in the current paper.

5. The authors make a very good point about the relation between cloud liquid water, radiation and near surface temperature, which is very relevant and interesting. I was wondering whether wind speed is another factor that could have some control on near surface temperature through turbulent mixing processes? The early strong wind period has indeed high temperature, but there are perhaps too few strong wind periods to draw any conclusions?

It is possible that turbulent mixing of the boundary layer would contribute to warmer temperatures since it would eliminate surface-based temperature inversions. Having said that, there does not appear to be a clear correlation in the recorded data. For example, August 18th featured some very low wind speeds (1-2 m/s), but was also one of the warmest days of the entire campaign. September 2, conversely, featured much colder surface air temperatures, but had relatively high winds (5-12 m/s), comparable in speed to winds observed early during the campaign when temperatures were much higher.

6. Figure 3b does not seem to have an insert as advertised in the caption.

Correct – this was revised at the last minute and we forgot to update the caption. This has been corrected. Thank you for bringing it to our attention.

7. Figures 2, 3, 4 and 6 have right hand panels with distributions of errors. The captions refer to these errors as "differences between observed and simulated values". It should be "differences between simulated and observed values".

Yes, you are correct, and we have made this revision, as suggested.

Reviewer 2 Comments:

General Comments

This is an impressive and well-written paper that addresses an important issue: how well do various reanalysis and models perform in the Arctic? There is a large amount of information presented, but the authors do a good job of making this understandable to the reader through the use of color figures of times series and bias/range displays.

The authors appear to have done the best they can at reconciling the various gridspace and time resolutions of the different models. The results are well-described, and the authors' knowledge of the various models is apparent as they try to explain the various results. The summary/discussion is appropriate. The emphasis on cloud characteristics is appropriate given their importance to the energy balance in the Arctic.

Specific Comments

Despite a careful reading, this reviewer did not find any problems with the paper, nor did he disagree with any of the conclusions.

We thank the reviewer for the time spent reading through the document and appreciate his support of this work.

Technical Correction

P 19433 line 20 represent » representing

This change has been made, as suggested.