

Interactive  
Comment

***Interactive comment on “Quantifying the clear-sky temperature inversion frequency and strength over the Arctic Ocean during summer and winter seasons from AIRS profiles” by A. Devasthale et al.***

**Anonymous Referee #1**

Received and published: 23 March 2010

Devasthale et al. analyze AIRS satellite data to quantify inversion frequency and strength over the Arctic Ocean. They document seasonal (winter vs. summer only), year-to-year (2003-2008), and geographic variability. While the data analysis will be useful to those studying the poorly sampled Arctic Ocean, the analysis and physical interpretations are limited in scope and novelty. I also have several specific comments below. If addressed, the paper may be appropriate for publication in ACP.

1) Data sampling concerns. More details on the conditional sampling of this dataset in the Arctic should be included, especially if this dataset is to be used for model evalu-

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ation as suggested on page 2845. Perhaps the greatest limitation of the AIRS data is the clear sky sampling. This issue needs more attention in this paper. How frequently are AIRS observations considered to be cloudy? Does this change as a function of season and year? Can you assess the clear sky bias in the results presented here? Can models compare their results to a clear-sky retrieval without conditional sampling? In addition to the clear sky sampling issue, what is the temporal sampling of the AIRS data? What is the difference between the ascending and descending passes? Why are data from only the summer and the winter analyzed?

2) Physical interpretation. Explanations of what controls geographic and temporal variations in inversion frequency and strength were limited and/or not new. Why are the inversion strength PDFs broader in winter than in summer? Why are there weaker inversion strengths and less frequent inversion occurrences in the near-coastal environment as compared to the central Arctic?

3) Following on from 2). While this paper does provide additional quantification of the atmospheric temperature structure during 2007, it does not add much new information beyond what is already present in the literature. As noted by the authors, work has already been done to assess the 2007 inversion strength anomaly as seen in the AIRS dataset. The monthly resolution adds more temporal detail, but as presented and discussed, is not compelling. Kay and Gettelman (2009) discuss both large-scale circulation and sea ice loss related mechanisms for year-to-year variations in inversion strength as assessed from AIRS. For example, they attribute the strong inversion strength in 2007 to warm air advection: "Warm air advection aloft produced by the southerly winds also enhanced near-surface static stability over the Pacific marginal seas." A similar situation is likely present in 2005, which also had a strong anti-cyclonic Beaufort High as discussed in Kay et al. (2008).

Minor comments

Page 2838: I was not familiar with the work of Liu et al. (2006), but am skeptical about

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the utility of this data. What can you learn about the Arctic inversion with channels peaking at 650 mb and the surface? The 650 mb level does not seem appropriate.

Do the authors know the following papers? They seem particularly relevant.

Pavelsky T, Boé J, Hall A, Fetzer E (2009) Atmospheric Inversion Strength over Polar Oceans in Winter Regulated by Sea Ice, Submitted to Clim. Dyn

Boé J, Hall A, Qu X (2008), Current GCMs' unrealistic negative feedback in the Arctic, J. Clim, 22: 4682-4695, DOI:10.1175/2009JCLI2885.1

Page 2839. "discussions" to "discussion"

Figures. Is it necessary to include both the ascending and descending orbits? If both are included, it would be more useful to provide one orbit, and a difference map between the orbits.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 2835, 2010.

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