

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.

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Comments on “Quantifying the clear-sky temperature inversion frequency and strength over the Arctic Ocean during summer and winter seasons from AIRS profiles” by Devasthale et al. This is an interesting and fundamental description of Arctic temperature inversions. The ubiquity of these features described here are sufficient justification for publishing the results. The manuscript is well organized and well written

My first concern is with possible sampling effects (also brought up by the first reviewer),

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though I believe this is a minor concern and can be resolved with some careful book-keeping and careful wording of the text. The occurrence rates cited in this study are conditioned on at least three criteria. The first criterion is the AIRS quality flagging, which reject a certain fraction of cases before they are averaged into the L3 products. Second is the requirement that cloud fraction be identically zero. Third is the definition of an inversion. The effect of each of these can (and should) be better stated in the manuscript. For example, what fraction of total AIRS observations do the PDFs in figure 3 represent if they are highest quality retrievals, stringently clear, and meet the criterion of a temperature inversion? This is a more representative measure of frequency than the fraction of inversions in Level 3 data. That said, this is a really minor point: it is difficult to imagine a serious sampling issue when the phenomenon of interest occurs 70-90% of the time. (Note that Gettelmen et al. (2006), The global distribution of supersaturation in the upper troposphere from the Atmospheric Infrared Sounder, *J. Clim.* used AIRS data to describe supersaturation in $\sim 1\%$ of the data.) The results shown in this study are some of the most robust I have seen in any AIRS study.

Another concern is the use of descending and ascending as proxies for daytime and nighttime. A longer discussion of the diurnal sampling is needed. Using day and night instead of ascending and descending (or the other way around?) is much easier on the reader unfamiliar with the Aqua orbit.

Here are other comments:

p. 2838, line 20. More discussion of diurnal effects is needed here.

p. 2838, line 25. The requirement of zero cloud fraction is a bit draconian. Figure 1 of Fetzer et al. (2004), Satellite remote sounding of atmospheric boundary layer temperature inversions over the subtropical eastern Pacific, *JGR*, shows that physically plausible inversions can occur in the AIRS data under visibly obvious cloud cover. The identically zero cloudiness requirement here could be increased to 10 or 30% cloud fraction without adding significant uncertainty. This should also increase the inversion

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occurrence rate.

p. 2839, line 1 forward. Something should be said about the relative frequency of surface based versus elevated inversions, since they presumably form under different conditions.

p. 2842, line 3 forward. This sentence is readable but runs on, and the 'also not expected' construct is unnecessarily complicated.

p. 2843, line 2. The "was" can be deleted.

p. 2843, line 10 forward. More articles should include sections like this. The limitations are not always obvious.

p. 2844, line 15. Delete "the" so it reads "in every AIRS profile."

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