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Interactive Comment

Interactive comment on "Humidity observations in the Arctic troposphere over Ny-Ålesund, Svalbard based on 14 years of radiosonde data" *by* R. Treffeisen et al.

L. Miloshevich (Referee)

milo@ucar.edu

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SUMMARY

This is a well-written and informative paper that describes the frequency of occurrence and characteristics of ice-supersaturation (ISS) and ice-supersaturated layers (ISSLs) in the Arctic troposphere, based on analysis of a long time-series of radiosonde relative humidity (RH) measurements. The authors have made an admirable effort to minimize the many shortcomings of radiosonde RH data, especially in the upper troposphere. Studies of the occurrence of ISS in the atmosphere are relevant to a variety of research areas, including water vapor and cloud parameterizations for models and radiative transfer calculations, for satellite validation, and for contrail prediction. This paper provides a useful characterization of ISS at arctic latitudes.

GENERAL COMMENTS

1. Some sort of time-series assessment (perhaps annual averages) of ISS and ISSL characteristics would show something about annual variability and possibly trends (though I'm not suggesting that the data are suitable for climate monitoring). Also, for purposes of data assessment, time-series analysis might identify possible instrument-based discontinuities caused by changes in the sonde type (RS80-A vs RS90/92), or by the elimination of the RS80-A contamination problem by Vaisala in June 2000 (see specific comments).

2. In addition to the radiosonde measurement errors that are discussed and addressed with existing corrections, there are two more that bear mentioning and possibly investigating. First, the RS80 is affected by sensor icing, where a coating of ice can form on the sensor in liquid water, and to a lesser extent in prolonged ice-supersaturated conditions, causing the measurements to remain near ice-saturation sometimes into the stratosphere for severe cases (M01). At a minimum the data should be screened for severe sensor icing. My approach is to reject the sounding if the mean RH in the altitude range 3-7 km above the tropopause is greater than 13% RH (this is arbitrary, but is based on looking at many soundings). Icing is less of a problem with RS90/92, but can still occur if there is prolonged ice-supersaturation above the level where the alternating heating cycle terminates.

Vaisala radiosondes are also affected by solar radiation error, where solar heating of the RH sensor leads to a dry bias in the measurements. The mean solar radiation error in the lower troposphere, from microwave radiometer PW measurements, is 6-8% for daytime RS90 soundings (M06), and 3-4% for daytime RS80-H soundings (Turner et al. 2003, JTech, probably similar for RS80-A). (Note that these are percentages of the measurement, not %RH values.) The RS90/92 solar radiation error is probably

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greater because there is no protective cap over the sensor. There is some evidence that the RS80 solar radiation error doesn't vary much with altitude, presumably due to the cap, but there is a strong altitude-dependence to the error for RS92 (Voemel et al., in press, available at http://cires.colorado.edu/˜voemel/, "Radiation dry bias..."). The solar radiation error varies with the solar altitude angle, and not much is known about the error at low solar altitude angles. I calculate that for the lat/lon of the measurements at 11 UTC, the solar altitude angle is less than 10 degrees and can safely be ignored during Sept. to March. Then the angle increases to a maximum of 34 degrees in June, and there is the possibility of solar radiation error during the summer of uncertain magnitude. The possibility of solar dry bias during the summer should be mentioned, especially for RS90, and you might consider looking at the day vs night data separately (although it might not be possible to distinguish solar radiation dry bias from natural diurnal variability in the water vapor field).

SPECIFIC COMMENTS

p 1265, end of sec 2.1: Regarding the GC, it would be helpful to mention that 0% RH is the (assumed) RH when the sensor is placed in a container of desiccant, and the sonde measurement while in the desiccant is used in the data processing to adjust the calibration to read 0% RH. I think it is optional to apply the results of the GC as a correction...was the GC only used to reject sondes (at what threshold?), or was it applied to the data as a calibration adjustment? The GC is subject to "operator error" if the desiccant is not fresh, because the actual RH in the desiccant chamber may be $>>$0% RH. If available, it would be instructive for evaluating the radiosonde data to see a PDF of the magnitude of the GC correction, and it would be even more instructive to see a time-series that would allow identification of instances where the GC correction increases as the desiccant becomes contaminated.

same paragraph: Can you also provide a PDF and/or time-series that shows the results of the check at 100% RH? Was this information used to adjust the data in any way?

p1266, sec 3.1, line 8: Specify that these are correction factors (not RH or some other values).

p1266, sec 3.1, line 18: Please clarify what "parallel" means. Were the sondes on the same balloon or different balloons, and if the latter were they launched at the same time? If on the same balloon, were the profiles aligned based on 'time' rather than 'altitude' (which minimizes alignment errors caused by differences in the pressure measurements that are used to calculate altitude).

p1267, line 5, re contamination correction: If radiosonde serial numbers were recorded with the data then the production date can be determined (M04), and the age is launch_date minus production_date. If this can't be done, please give some indication of how much the contamination correction varies between, say, 1 month and 1 year, as a measure of uncertainty. The contamination correction only applies to radiosondes produced before June 2000, when Vaisala fixed the contamination problem with a sealed sensor cap. So the correction shouldn't be applied to radiosondes produced after June 2000, or if applied it may overcorrect by a magnitude that should be given in the paper.

p1268, first full para: It seems like the first sentence belongs with the previous paragraph about time-lag correction.

[Aside: although the concept of the time-lag correction is relatively simple, its numerical implementation is fairly complicated, and I know that some people find it difficult to get right. I'd appreciate feedback on your experience with the time-lag correction and the adequacy of M04 in conveying how to implement it. Thanks. (milo@ucar.edu)]

Same para: You might mention that the M06 method for RS90 involves time-lag correction plus an empirical calibration correction that removes mean bias error relative to simultaneous measurements by the CFH (the CFH is the newer version of the NOAA hygrometer and is described by Voemel et al. (in press, available at http://cires.colorado.edu/~voemel/, "Accuracy of..."). Also, please be more specific in the last sentence when summarizing which corrections were applied to which sensor type.

p1268, final para of sec 3.2: Please clarify that this correction is not applied IN ADDITION TO other corrections, but rather is a separate statistical approach that implicitly includes all sources of measurement error by removing the RS80-A mean bias relative to the NOAA hygrometer as a function of T. You found that this approach can overcorrect in the UT, and we now understand that this is because the correction was derived from daytime RS80-A/NOAA comparisons where the sensor cap had been removed thus increasing the solar radiation error, so this correction will certainly overcorrect nighttime soundings.

p1268, second sentence of sec 3.3: Consider being more explicit, e.g., "The RS80-A was corrected for contamination and calibration errors (W02) and time-lag error (M04), and RS90 was corrected for time-lag error (M04) and calibration error (M06)." It should be noted (here and perhaps elsewhere...see General Comment) that the M06 correction refers to nighttime soundings, so one would expect especially RS90 measurements to still contain a solar dry bias for daytime soundings in the summer.

sec 3.3 in general: You might consider showing an example comparison in the form of altitude profiles from both sondes, before and after corrections are applied.

p1269, sec 4.1 and Fig. 4: Is there enough data to construct figures like this for each year (or two years), to see if there is an anomalous discontinuity when the sonde type changed?

p1270, line 20: Is this 70% of all ascents, or 70% of those that contained some supersaturation?

p1272, re exponents: I suggest also specifying in Table 2 the coordinates of the peaks in Fig. 7. This would allow one to construct a more accurate parameterization of the RHi distribution using the 2 equations and the peak, thereby including a crude representation of the cloud processes.

p1273, line3, and Table 2: What is a "non ice-supersaturated layer"? Are these the broad regions between, above, or below ice-supersaturated layers, and would it be the entire profile if there were no supersaturation in the sounding?

p1273, line 27: It is implied by the wording of the sentence that all ISS occurs in clouds, whereas a large amount of especially the higher supersaturations probably occurs in clear air.

p1274, line 5: Does this mean that layer thicknesses are always multiples of 200 m? If so, this seems rather course, and I wonder if you couldn't just calculate the actual layer thicknesses from the soundings before averaging to 200 m?

p1274, line 22: 400 m?

TABLES AND FIGURES

p1283, Table 2: standard deviation of b = 0.0 suggests these numbers weren't multiplied by 100 as stated. Also, does 98th percentile mean that 2% of layers had mean layer temperature greater than these values? It would also be useful to see the 98th percentile on the cold side of the mean.

p1284, Table 3: The 98th percentile on the dry side of the mean would also be useful to see. Suggest moving the ratio column to the end, or else defining it in the caption.

p1285, Fig. 1 (and related text): It would be helpful to show the ice-saturation curve in color so it can be seen and so the prevalence of ISS can be seen. Also, please specify the saturation vapor pressure formulation used to calculate the ice-saturation curve (Vaisala calibrations assume Wexler). Also, it appears that there must be data below -70C...can these data also be shown?

It seems odd that RS90 never measured very dry conditions at the lowest tempratures shown, especially since conditions up to 2 km above the tropopause are considered.

Does inspection of the actual RH profiles suggest any reason for this? Also, it is unexpected that RS90 was always well below the Koop HN threshold...does this suggest that either RS90 has a dry bias (e.g., see General Comment about solar radiation error), or perhaps Koop is too high?

In order to judge the effect of the corrections relative to the curves, it would be very helpful to see these same plots after the corrections were applied.

Finally, although Fig. 1 is a good representation of the datasets, it would also be very informative, especially for comparison purposes, to see PDFs of the RH for both sonde types (before and after correction).

p1286, Fig. 2 (and related text): It's unclear from the caption whether this is an RH difference (%RH) or an absolute percentage difference (%). Also, is it a correct interpretation that the mean differences shown in this plot represent an expected discontinuity in the ISS results when the sonde type is changed from RS80-A to RS90, and do you see such a discontinuity? It seems to show that the corrected RS80-A (red) is generally ˜5% RH lower than RS90 in the lower and mid trop, and ˜5% RH moister than RS90 in the upper trop (is there data at lower temperatures for panel B?). I have also seen moist bias in corrected RS80-H in the UT, which I suspect is due at least in part to sensor icing (see General Comment). Also, the font size requires using 200% zoom to read it.

Since the Vaisala calibration is a function of both RH and T, errors and differences will also depend on both RH and T. You might consider also looking at the absolute difference as a function of both RH and T, i.e.: percentage difference = (RS80A - RS90)/RS90 * 100.

One problem with using corrected RS90 as the "reference sensor", in addition to the possibility of solar radiation error discussed in the General Comments, is that early RS90 radiosondes (produced before June 2001) used a less accurate temperature-dependence calibration. There is a correction for this in M06 (Appendix B), if you have

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serial numbers to determine the production date.

p1287, Fig. 3: Axis labels are in the wrong place, and equation font size is too small, and a 1:1 line would be helpful.

Fig. 5 caption: repeated phrase.

Fig. 6 caption: "frequencies" –> "frequency of", and grammatical errors.

Fig. 8: Font is way too tiny. Remove "adjacent" from caption?

TECHNICAL COMMENTS

p1265, top, line 3: use "to analyze" or "for analysis of"

p1265, sec 2.2, line 24: "at" 1.02 um?

p1266, sec 3.1, line 18: "stated out" –> "described"?

p1268, line 25: Do you mean "temperature dependent"?

p1270, line 6: Should "clean" be "clear"?

p1274, line 10: sentence repeated.

p1274, line 15: remove "that"

p1274, line 26: change "the both"

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1261, 2007.