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7, S706–S716, 2007

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.

R. Treffeisen et al.

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We first like to thank Mr. Miloshevich for his fruitful comments on our paper. We like to take the opportunity to comment on suggestions and how we changed the manuscript for improving the paper and its content. We will resubmit an overworked version of the manuscript including the new figures and tables. We also like to mention that due to the overwork we analysed now as well the year 2006 in the data anlysis.

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 IąÇm not suggesting that the data are suitable for climate monitoring). Also, for

EGU

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).

Actually, we are working on a time series assessment in terms of trends but we believe this does not suite into the intention of this paper. We will address this in another paper in preparation in detail and with different statistical tools in order to see weather we can identify a trend in the data or not. There are authors (Box, J. et al., Upper-air temperatures around Greenland: 19642005 or Seidel D., et al., 2006, Variability and trends in the global tropopause estimated from radiosonde data) who both used radiosonde data to perform such a study.

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.

Yes, we did screen our data for icing processes using a criterion of rejecting all profiles for which a specified ppmv (mean plus two times standard deviation) was exceeded in the altitude range of 4 to 6 km. We think this match very good with your suggestion of the 13% RH in an altitude range of 3 to 7 km. Anyhow to take your suggestion into account we used the mean of RH in the altitude range between 3 and 7 km above tropopause height and one sigma (around 9%). Therefore, the profiles are cleared for

ACPD
7, S706–S716, 2007

icing events. We add this in the treatment of the data base in the manuscript. Indeed this to mention did not occur in the text.

3. There is some evidence that the RS80 solar radiation error doesnąÇt 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).

Thank you for this helpful comment. We add some phrases in the manuscript to address the questions but I think we are not able to provide a day/night comparison. On the other hand such a correction would only be possible if there is a straight forward published formula available to apply for us. We believe this is a thing to work on in future and it might be even interesting to look at the soundings in this perspective. We will perform an intensive sounding (every hour) for one day this year and this might be then a possibility to look at such an interesting issue.

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

ACPD
7, S706–S716, 2007

Interactive Comment

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?

We add some sentences here to be more precisely. The GC is used to ensure quality prior to the launch. First the GC is used to reject faulty sondes when the following limits are not hold: 7% humidity difference for RH and 1.0% as stability criterion. These criteria are following the international standards of radiosondes launches. If we are inside the criteria the difference is used with the Vaisala routines to correct for the difference. Therefore, the GC was used to apply a calibration adjustment to the data following the standard international rules for radiosonde launches. There is no possibility to show a time series as you mentioned as this is not recorded in the routine work at the station.

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

We changed this sentence.

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 ąÇtimeąÇ rather than ąÇaltitudeąÇ (which minimizes alignment errors caused by differences in the pressure measurements that are used to calculate altitude).

The sondes were launched on the same balloon on the same horizontal level. They are aligned on height. Using a height average of 200m we believe it is not really necessary to align data on time. But we performed some checks to as you suggested but the

ACPD

7, S706–S716, 2007

Interactive Comment

results did not change. As we are further on only looking on height dependent analysis we kept our figures as they had been.

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 canąÇt 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 shouldnąÇt be applied to radiosondes produced after June 2000, or if applied it may overcorrect by a magnitude that should be given in the paper.

As mentioned in the paper we are not able to determine the production time of the sondes we used the estimated storage time of normally not more than one year for the calculations. Following the publication of Wang et al., 2002 page 988 this error is around 2% at saturation for a one year old sonde. The correction was not applied for the sondes used after 2000.

p1268, 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.

Thank you for the comment we add a sentence as well as the reference to the paper. We also rephrased the last sentence and hope this will be more precisely now.

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

ACPD

7, S706–S716, 2007

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.

We add some sentences to clear this. Yes, it was of course not an additional correction. But we agree that the wording was not straight forward here. As the parallel sounding was taken in March/April the sun was already up.

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.

You are right we were not very precisely in the wording here and did change the sentences so that this should be much better now. We also add some sentences on the solar dry bias for daytime soundings in 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.

In principle your suggestion is right but we think it is not really necessary to bring such an additional figure as we already have three figures for the comparison included in the manuscript. If you still like to have one of course we will provide it. We think we would not gain additional information on such a figure.

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

ACPD

7, S706–S716, 2007

Interactive Comment

changed?

We have investigated the both sondes types separately. The principle structure and features are kept and no break could be identified. But the RS90/RS92 data present a rather short time period we put the data together in order to have in terms of statistics a more reliable data base. We add a sentence in the manuscript to take this into account.

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

Thank you for this comment. We did some rewording here to be more precisely as we meant that the percentage is related to those that contained 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.

We did not explicitly add the peak. It is around 100% and due to the measurement uncertainty we think it does not make too much sense to put there a number.

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?

Thanks for the comment. We believe that the word layer bring up some misunderstanding here and misleads the reader. We changed the wording. What we actually did was the following: For altitudes above 4 km, data was divided into two sets, one where the humidity was at or above ice saturation and one were the humidity was below ice saturation. Therefore if there is no ice-supersaturation layer the profile from 4 km up to 2 km above the tropopause would be considered.

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

ACPD
7, S706–S716, 2007

clear air.

The analysis of the vertical extent of the ISS layers was done because it relates to the potential thickness of clouds. We are aware of the fact that just because the humidity is supersaturated with respect to ice this does do not automatically set a cloud in this layer and that the very high relative humidityąÇs are present because clouds did not form yet. As soon as a cloud forms it will start consuming available water vapour. We did some rewording here and hope this is now better expressed.

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 couldnąÇt just calculate the actual layer thicknesses from the soundings before averaging to 200 m?

We have performed our analysis first with a much higher resolution and finally decided to choose the 200m as being most suitable for not implying to be more precisely as one can be after all the correction and smoothing. Looking at 100m steps keeps the entire analysis the same that was the version before we came to the conclusion of the 200m steps. All shapes and features are the same and some did look more accentuated, but the results did not change. Therefore, the layer thicknesses are always multiples of 200 m. As we are heading for general features in the upper troposphere we are believe that the chosen resolution is good for our purposes.

p1274, line 22: 400 m?

Thanks. This was a typing error. We changed the number to 400.

Tables and Figures:

p1283, Table 2: standard deviation of b = 0.0 suggests these numbers wereną Çt 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.

The b values and its deviation are multiplied with 100. If we get a standard error of

0.000 and we multiply this with 100 it is still zero. We have added a 2% Percentile for completeness in the new table and hope this is fine now. Yes, it is a normal 98% Percentile to characterise the distribution.

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.

It is included in the new table 2.

p1285, Fig. 1 (and related text): It would be helpful to show the ice-saturation curve in colour 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 temperatures 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?

We changed the colour of the ice saturation curve hope this is now better to see. The ice saturation curve was calculated using: Buck, A., 1981. New equation for computing vapor pressure and enhancement factor, J. Appl. Meteorol., 20, 1527-1532. The reference is added in the subtitle of the figure. As we are not interested in stratospheric influences we stopped the plot at -70°C. In order to show that there are of course dry conditions at low temperatures in the measurements we now plotted the original data without the limitation to the 2km above the tropopause height, which gives you the dry values as expected. Hope this will be cleared then.

p1286, Fig. 2 (and related text): ItąÇs 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 dis-

ACPD

7, S706–S716, 2007

continuity 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.

We changed the font size of the figure to make it better readable. We plotted the RH difference (%RH) and did add this information to the subtitle. Concerning your comments on the observed differences we like to mention that we checked the 12 profiles for sensor icing and we believe that this is not the reason for the observed differences. As we do not have serial numbers of the sondes to determine the production rate we are not able to apply the suggested correction for this from M06 (Appendix B). Looking at the entire data base separated into RS80 and RS90/92 we do not get a general change in the features as we see when we analyse the data as whole data block. As our intention was from the beginning to make profit out of the length of the data base we spend analysis on being sure to be able to connect them. Furthermore,12 sondes is not a large data base at all. But we used this comparison balloon launches more for a general indication of uncertainty.

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.

Axis labels have been in the right place but the legend was wrong. The Font size is changed and a 1:1 line is added for better reading.

Fig. 5 caption: repeated phrase. Double sentence is deleted.

Fig. 6 caption: "frequencies" $>$ "frequency of", and grammatical errors. Caption is changed.

ACPD

7, S706–S716, 2007

Interactive Comment

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

Technical comments:

All the following technical comments are changed, too. We are grateful to the very careful reading of the reviewer.

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"? 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.