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Atmospheric Chemistry and Physics Discussions

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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Answers to anonymous Referee #3:

We are also grateful to the 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 included now as well the year 2006 in the data analysis. Please also check our comments to the review of Mr. Milosevich.

The authors should at least discuss the impact of a solar heating bias on their measurements. This effect is not quantified, but may contribute to a dry bias for daytime

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measurements, which will affect the interpretation of the seasonal cycle.

Following the suggestions of review from Milosevich we add some sentences for the dry bias for day time measurements. We like to repeat here what we comment on this to Mr. Milosevich. We add the reference Voemel et al., in press, available at http://cires.colorado.edu/voemel/, "Radiation dry bias....ąÇ. 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. 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.

Section 3.3, comparison analysis and figures 2 and 3 are not that illuminating. What is actually ąÇtruthąÇ in this case? The corrections look smaller (10-15% RH) than other similar values at low temperatures from Miloshevich. Perhaps these are not that cold (some of the Milosevech values are tropical). Also, when were these 12 profiles taken? Does the solar heating bias affect them?

The capture of the figure and the section 3.3. clearly states out that the 12 profiles were taken in March and April 2000. As we have a review of Milosevich we think we can keep this figures as they deal for a general feeling of the uncertainty. The section does not serve as a reference to truth, but rather as the variations and deviations of different sondes for a very small subset of the data. Concerning the solar heating bias Milosevish mentioned as reviewer that between September and March this influence

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can be neglected and thus we believe that it might be event still a minor influence for measurements performed end of March and beginning of April.

There is also a recent paper by Gettelman et al that discusses supersaturation and shows some frequency maps in the Arctic. You might want to compare your values to these numbers (which may be high): A. Gettelman, E. J. Fetzer, A. Eldering, F. W. Irion, The Global Distribution of Supersaturation in the Upper Troposphere from the Atmospheric Infrared Sounder, J.Climate, 19(23), 6089-6103, 2006

Thanks for the reference. As main part of the paper more dealing with Tropic issues we not included any comparison on our manuscript. But it might be very interesting for future analysis to take data from AIRS to compare directly with the data in Ny-Ålesund.

Section 4.5: can you explain the vertical shift between supersaturation and sub-visible clouds from SAGE? What is the SAGE weighting function. Perhaps you could apply the vertical weighting function to the radiosonde data when you do the comparison and this would explain it. It would be useful to at least attempt this data matching before trying to use a dynamical explanation.

The vertical resolution of the SAGE II data is about a kilometre at these altitudes and so this will not explain the differences between the radiosonde and SAGE II SVC observations. Since clouds do not necessarily occur at the tangent point, cloud altitudes may be assigned an altitude below where they actually occur under some circumstances but not to altitudes above where they occur. We assume some of the ice supersaturations result in clouds that would terminate SAGE II events (no clouds).

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