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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ground-based water vapour soundings by microwave radiometry and Raman lidar on Jungfraujoch (Swiss Alps)" *by* D. Gerber et al.

D. Gerber et al.

Received and published: 15 December 2003

Anonymous Referee 1

Received and published: 19 November 2003

This paper is a quite interesting tentative to obtain a complete vertical profile of the atmospheric water vapour distribution by using measurements obtained by two groundbased instruments such as a Raman lidar and a microwave radiometer located at the same place, e.g. the ISS Jungfraujoch. Therefore, each instrumentation has its own limits, i.e. the Raman lidar profile stops at about 6 km while the microwave radiometer profile starts at 20 km and upReferee. In order to get some information about

the water distribution between 6 and 20 km, the authors present a methodology by adapting an a priori profile to match the tropospheric water vapour content measured by lidar and then they use the a priori information to retrieve the microwave profile. This idea is quite interesting and could be developed in other cases.

Therefore the paper suffers in clarity due to a laborious writing. By improving the English writing, the paper should be more concise and clearer (essentially about the used methodology). Some remarks about figures: Figure 1: The caption to Figure 1 does not indicate the used symbols as given in caption to Figure 2.

➥In Figure 1 the symbol-coding of the profiles for each month should not puzzle the reader because in this Figure the profiles for each month are held separate. But we will clarify the caption to this figure in the final revision of the manuscript.

Figure 4: It is suggested to indicate on the plot itself the instrumentation used for the different profiles, as the caption to figure is not so clear. Thus the reader would have a clear and immediate idea of the various profiles.

➥This has already been corrected in the first revised manuscript.

Figure 5: It is suggested to use different line symbols for the final combined profile: Raman lidar, adapted a priori profile and retrieved microwave profile.

➥We believe that this plot would not benefit from the use of line symbols since the individual profiles are very close to eachother and the use of symbols would obsctruct the view on each profile. If we are not overruled by the editor we will leave the plot as it is.

Typographic errors. They are very numerous essentially in parts 2 and 3: Lack of

ACPD

3, S2202–S2210, 2003

coherence to writing: backscatter. Present should be used for the verbs in 2.2 and not past. Some indexes for chemical species are missing. Dependent or dependant? Astronomic? Plural is missing in several cases. Curious way of writing H2O in part 4? ÿ Sa instead of ÿ SanÇT in several cases ISSJ not defined in the introduction.

➥These typographic corrections have already been given in the peer review process and are already corrected in the first revised manuscript.

In conclusion, this interesting work has to be published but the English writing and typing mistakes should be corrected in order to clarify the comprehension of the methodology and results for future readers.

Anonymous Referee 2

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The authors present a method for combining raman lidar measurements with microwave measurements to get a single water vapor profile from the ground to 60 km. This is an interesting idea not only because it is intrinsically appealing to have such a continuous profile, but also because, in order to retrieve the stratospheric water correctly, one needs to know the attenuation of the signal by the troposphere. There are, however, some serious issues which the manuscript needs to address:

4839 lines 15-22: The difference in peak altitudes is quite large considering that the average latitude of the HALOE measurement is not that far south of the AMSOS measurements. My guess is that even the HALOE profile for 50-60N has a higher peak than the AMSOS, which would invalidate the claim that this is a latitudinal effect. The

authors should certainly make this comparison if they continue to wish to make the claim that the difference is caused by latitudinal differences.

➥The suggested comparison could indeed proof whether our assumtion is correct or not, unfortunaltely this comparison is not feasible in such a way as it was suggested. The reason for this is that HALOE does not yield any data above 50N in the deep winter months November to January. This is due to the fact that HALOE measures solar occultations which, because of the low solar angle over the winter hemisphere, do not occur further north than 50N.
As an illustration we have evaluated all the available HALOE profiles for the winter months of 1999. Figures with the results are available at:

http://www.iapmw.unibe.ch/staff/gerber/authorComments.html

There one can see in Figures 1-4 that: (i) The peak altitude of water vapour gradually descends from the equator towards the pole for all months. (ii) The latitude range of the measurements is reduced for the months of November to January.
The fact that there are no measurements above 50N for these months can also be seen in Figure 5 on that webpage, the figure in which the latitude distribution of HALOE measurements is shown.

We can still gain some insight from the comparison. First of all we note that for the month of October, for which we have HALOE measurements up to 70N, the water vapour peak does not fall below 50km. This is in accordance with the results in our manuscript where we observe a lower peak altitude only for the months following October.
Afterwards, for the months of November and December, during which we would expect the sinking of the peak to take place over the area of the polar vortex, we have

ACPD

3, S2202–S2210, 2003

Interactive Comment

no HALOE measurements for said region so we can't use HALOE to contradict our assumption.
We can however note that for the month of January also the HALOE peak altitude falls to 40km, even though its values are still high at 50km. These observations fit with a scenario of a downwelling starting after October, manifesting in a decrease of the water vapour peak altitude first at latitudes greater than 50N and which then reaches a minimal value of 40km in January as much southward as 50N.

While all this evidence does not directly validate our assumption to have observed the downwelling of polar air over the vortex looking at the water vapour peak altitude, it is nonetheless in accordance with it as outlined the secenario above. The evidence gained from the HALOE measurements most certainly does not contradict the assumption.

The scenario given is realistic as can be seen in a study presented by one of our coauthors about airborne measurements with our AMSOS instrument (This study is also cited in our References):

http://www.iapmw.unibe.ch/publications/pdffiles/477.pdf

In Figures 3-5 on this poster the descending of the water vapour peak altitude due to downwelling of polar air in wintertime can be traced back to the period between August and February, while in August there is no sign of it at all and in February the peak altitude has already fallen to 40km at our latitude (47N). This study also shows that at 47N we are located just at the latitude border where minimal values can still be observed, thus ruling out the possibility that such low values could manifest in HALOE means for the latitude range 40N to 50N as pointed out correctly by Referee 2.

ACPD

3, S2202–S2210, 2003

4840 lines 16-18: I think the authors are trying to say that the peak moves up and down a lot so there is no consistent peak altitude , but I might certainly imagine that with enough averaging there might be changes in the profile shape with season. I'd just drop this sentence.

➥Actually we do not want to talk about peak altitude in this part about tropospheric variability at all, this line was just meant to denote a cross-comparison to the stratospheric processes. Since this seems to be confusing we will drop the line as suggested by the Referee.

4841 lines 22-24: Given that the entire lidar profile seems to be calibrated according to the MeteoSuisse ground measurements, it seems likely to me that the dominant contribution to the variation in the column is probably this calibration term, and not the actual lidar measurements. Please clarify whether this is or is not the case.

➥The Raman lidar water vapor mixing ratio profiles on this work are obtained from the average of typically 30min to 2h accumulation data (20-50 Hz repetition rate at 60-70 mJ/pulse). Each profile is first corrected from atmospheric transmission (Rayleigh, aerosols, etc) and then is calibrated with the correspondent average value calulated from MeteoSuisse ground measurements (P, RH, T) and this for each measurement data series (avoiding thus the system changes influence). The spatio-temporal variability given on the water vapor profile (1Sigma) is calculated based on Poisson statistics for the temporal variability at the same altitude and from standard statistics for the spatial average. Thus the average value at JFJ is calibrating the average profile. Ongoing studies of the influence of the variability of the calibration constant will show the accuracy of using this method of calibration.

4841 line28-4841 line 3: These 2 sentences are a bit confusing. I think all the authors

ACPD

3, S2202–S2210, 2003

are trying to say is that, because the lidar is only measuring up to 5500m it is not getting the total tropospheric column because the tropopause altitude is above this. If so, please just say that. Also, do the authors have a value for the tropopause height at this time?

➥This is indeed what we want to say. The point is that the value we might call the lidar 'penetration depth' into the troposphere is dependent on the humidity of the troposphere. Therefore if the troposphere is very dry, the 'penetration depth' is low and the column density given by the lidar consequently depicts a lower column than on a wet day. This has to be taken into account when comparing the column values. (The value we have for the tropoapuse height is from a climatology; We don't have a measurement exactly for Jungfraujoch and this day.)

4843 line 6: 15km? What is filling the gap from 15 to 20 km?

➥This is an inaccuracy in our text. The limit where we cut off our spectrum due to a priori contribution is not a fixed one, but we should certainly use the same value throughout the text. I will correct this in the next revision of the manuscript.

4844 line 21: It would be very interesting to see a quantification of how realistic variations in the tropopause water vapor profile with the total column constrained would affect the retrieval. My guess is that this will not have a significant effect.

➥This would indeed be an interesting research topic, but it requires a thourough sensitivity study well beyond the scope of this work.
Furthermore, when using the lidar profile to derive an a priori profile for the troposphere, we do not tie this profile to a given column density. On a dry day the tropospheric profile will feature less water vapour alltogether than it would on a wet day. Less water vapour in the tropopause will always increase the accuracy of the retrieval because the signal to noise ratio of the measured spectrum is better. This can

ACPD

3, S2202–S2210, 2003

be seen when the instrument is operated from the airplane at flight altitudes up to 12km.

Table 2: It is completely unclear from the text how the authors derived the water vapor values from 6.0 to 17.5 km. The authors mention the effect of the a priori profile, but how did they derive this a priori? Is there some functional form used here? If so, what is it? What happens at 12.5km? Presumably this is the tropopause, but where does this tropopause value come from? It certainly doesn't make sense to show results at 0.5 km increments in the upper troposphere when there aren't really any measurements there.

➥The altiude resolution between the first datapoint and the tropopause correspond to the inital a priori profile taken from US Standard Atmosphere which was afterwards fitted to the lidar datapoints between 3500m and 6km. The problem is that the microwave retrieval has a minimal vertical resolution of 5km, so any small scale feature like the exact location of the tropopause cannot be resolved.
So while we say that by using a validated a priori we do not have to cut off the retrieved values below 20km, we still cannot just print the result of the retrieval as it is because with the low resolution we would only have one, maybe 2 datapoints for the whole tropopause, which would make it impossible to get any feature like e.g. the slope of the tropospheric drying out.

So what we do (and what we have probably not explained clearly enough in the manuscript) is that we take the result of the retrieval down to the tropopause (i.e. 12.5km) instead of just 20km and from there on we use the values of the adapted a priori profile. The values are the same because the retrieval almost exlusively consists of a priori information at these altitudes, but the retrieval has such a low altitude resolution that it would just smooth out the tropopause region. We explain this procedure on page 4845 lines 7-13 of our manuscript, but since it seems unclear we consider to edit this section for clarity. Maybe the editor would like to comment on this.

ACPD

3, S2202–S2210, 2003

4847 line 23-25: The authors mention that there method is better than simple interpolation, and I certainly agree that their model looks not unreasonable, but once again I'm not sure how the a priori is derived.

➥See above.

Typos, grammatical corrections, minor comments:

4837 line 5: details should be detail

➥Will be corrected in the final revision of the manuscript.

4838 line 14: sentences should never end in for

➥Will be corrected in the final revision of the manuscript.

4838 line 21: cycles should be circles

➥Will be corrected in the final revision of the manuscript.

Figure 1: It is not clear what the gray range refers to. There can only be one 1999 HALOE monthly mean for the corresponding month , so what is meant by minimal and maximal? Same applies to Figure 2.

➥The minimal/maximal range corresponds to the lowest and the highest profile of the set of monthly profiles used to derive the mean value. We will set this straight in the final revision of the manuscript and captions.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 4833, 2003.