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Atmospheric Measurement Techniques
Discussions

Interactive comment on "UTLS water vapour from SCIAMACHY limb measurements V3.01 (2002–2012)" *by* K. Weigel et al.

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We thank referee #2 for the time and effort spent on reading the paper and providing the comments. Below please find the reply to every comment.

As a general rule, I would say it is generally not a good idea to include systematic errors in retrieval calculations, since this will reduce the observed variations below the ability of the instrument to measure them. But multiplying the SNR by 1.5 will certainly not have a large effect and to some extent this must be considered as a tuning parameter in any case.

For our retrieval setup the SNR is calculated from the residuals. This has the advantage, that it usually provides a better estimate of the varying SNR for SCIAMACHY than the instrumental L1 or any fixed or modelled SNR. A disadvantage is that the used residuals contain a mixture of noise and uncorrected systematic effects (especially a systematic but varying difference between "odd" and "even" detector pixels plays a role). Therefore, this scaling factor should provide a correction towards a pure noise error without these systematic effects. However, it is only an estimated value. Therefore we agree with the referee that it has to be considered as tuning parameter. To clarify that this is the reason why we use the SNR scaling, we will change the corresponding text (P7959, L8–13) to:

The signal-to-noise ratio (SNR) of the sun-normalized radiances is calculated from the spectral residuals multiplied with a factor of 1.5 after the wavelength shift correction is applied. The SNR obtained in this manner is not identical with the instrumental L1 SNR but does include additional, partly systematic errors, which are part of the residual. The multiplication by 1.5 shall account for these systematic features in the residuals, which should not be part of the noise error. The SNR is usually between 400 and 700. It is largest for lower altitudes.

How is the vertical resolution shown in Figure 1 defined? Presumably the reason that the vertical resolution is better than the vertical sampling (as has been mentioned by the other reviewer) is because there is information in the spectrum. If I understand this correctly, would it be possible to show a typical spectrum and fit in the UTLS?

The vertical resolution is calculated as inverse of the information content, i.e. the inverse of the diagonal elements of the AVK, as described at P7960, L15-17. That the resolution can be better than the sampling is because it is not directly dependent on the sampling and calculated independently for each height of the retrieval grid. It describes how the value at one particular altitude is influenced by the measurements at the same and other altitudes. The best possible resolution is about 2.5 km, limited by the size of the FOV. Theoretically one single LOS can lead to this resolution at one height. But together with to the regularization the vertical sampling plays a role of for the resolution at heights inbetween the measurement altitudes. With 3.3km the sampling is coarser than the vertical extent of the field of view. At the lowest altitudes the real difference between two measurement altitudes is increased to even more than 3.3km due to refraction. For most other limb instruments the vertical sampling is rather higher than the vertical extent of the field of view. In this usual case the result is always clearly influenced by neighbouring measurement altitudes. For the lower altitudes in this retrieval the influence of the neighbouring altitudes is rather low, therefore these higher resolutions are possible. The drawback is, that the resolution between two measurement heights is clearly influenced by both, leading to the zigzag shape in the resolution profile. Spectra and fits are shown in Rozanov et al. (2011) for V3 and vary more for individual profiles than for the different data version V3 and V3.01, therefore we do not show them in this work. Here, also the resolution calculated with the Backus and Gilbert approach (Backus and Gilbert, 1970), i.e. the spread function defining the averaging kernel width is shown and is comparable to the one calculated here. Because the description how the resolution is calculated is rather short, we will change it to:

In combination with the regularization, the extent of the FOV, and the vertical sampling yield the height dependent resolution of the retrieved water vapour profile, shown in the right panel of Fig. 1. As in Hoffmann et al. (2008), it is calculated as reciprocal values of the diagonal elements of the averaging kernel matrix multiplied by the retrieval grid spacing. This method to calculate the resolution is based on the concept of information density (Purser and Huang, 1993) and yields comparable results to the Backus and Gilbert approach (Backus and Gilbert, 1970) used e.g. in Rozanov et al. (2011) for V3 of the water vapour from SCIAMACHY limb measurements. The lowest part of the profile shows the best vertical resolution of about 2.5 km. Here, the result is mainly influenced by one measurement, therefore resolutions close to the

FOV size are possible. The resolution becomes coarser with increasing height and between the measurement heights at about 11.6, 15, 18.3, 21.6, and 24.9 km, where local minima in the vertical resolution are seen. For all SCIAMACHY limb profiles the measurements are taken at about the same heights, leading to a systematic variation of the resolution with altitude in the data set. The coarser resolution at higher altitudes is caused by a lower SNR combined with the increased smoothness coefficient. At 22 km, the resolution is clearly coarser than the 2.5 km FOV width also at the grid point closest to measurement height because due to the regularization and the lower SNR at 22 km this height is influenced by the measurement below at about 18 km.

In Figure 2 the grey and orange lines are very difficult to see except where they are on top of the black and red lines. It would be easier to see if these lines were replaced by symbols and overplotted on the black and red lines.

We will change the figure to increase the visibility of all lines.

What is the primary cause of non-convergence for 27% of the profiles and how does this bias the results?

The two primary causes for non converging profiles are a low SNR and difficulties in the retrieval to fit the albedo and the tropospheric contribution parameter, which can influence each other. We observed that most non-converging profiles are found at high northern and southern latitudes and often in cases with high albedo or high effective elevation. There can be systematic effects but these should be smaller than the effect of the cloud filter (for high clouds). At P7961,L21 we will include the sentence:

Non-converging profiles are mainly due to cases where the SNR is low or where the retrieval of the tropospheric contribution parameter and the albedo is difficult and are more frequent at high northern and southern latitudes.

*Figure 3a shows a very large (off-the-scale) positive difference from 12-14km for the H2O*0.5 (12-14km) case, while the smoothed version (Figure 3b) shows a small negative difference in this region. This seems very surprising. Is there a way to explain this?*

The smoothing acts on a more than exponentially decreasing profile (like the one shown in Fig. 1). Therefore the (in the relative terms) apparently smaller negative region below the large positive difference leads to a negative smoothed value (the opposite happens with the green dashed line).

Why are saturated measurements specifically excluded for FISH but not for other comparisons?

FISH is the only instrument is the comparison, which measures total water, i.e. water vapour and ice crystals. All other instruments in the comparison measure water vapour. To compare them to water vapour measurements the FISH measurements are usually filtered for saturated cases, assuming that this excludes cases where cloud ice crystals are part of the measured total water.

Figure 1 shows that the measurement response is near 1 everywhere, which would seem to imply that the a priori doesn't matter. This seems inconsistent with the claim that in 3.2.1 that the reason for the better agreement with the balloons in midlatitudes is that "the a priori water vapour profile is closer to the expected real profile".

The value of the a priori has nearly no influence on the result, therefore the measurement response is usually near 1. But the shape of the a priori profile matters because of the first order Tikhonov constraint. To clarify this, we will change the text at P7977 L24–26 to:

The reason is most probably, that in the mid latitudes the shape of the a priori water vapour profile is more similar to the shape of the expected real profile.

Figure 15 – It's obvious, but it would still be nice in the first sentence of 3.3.1 to explicitly say "V3.01 limb measurements and the lunar and solar occultation . . ."

We will add this.

Figure 17 – The text says that the annual cycle of the differences when compared to the SCIAMACHY time series is better in the mid-latitudes than at the polar latitudes. But it seems to me that the relative differences at the polar latitudes show a much smaller annual cycle. So doesn't this mean that the annual cycle is better matched at the polar latitudes?

We agree with the referee that the sentence is confusing. It was meant that all other instruments but SCIAMACHY agree in their annual cycle in the mid latitudes. To clarify this we will change P7980 L3-5 to:

This is similar for the NH mid latitudes (Fig. 17, right panel). Additionally, here a concordant annual cycle is found in the differences between SCIAMACHY and the other instruments which is not seen as distinctly in the polar regions.

It should be pointed out that an increase in water vapor in the tropical lower stratosphere is consistent with increasing 100 hPa tropical temperature anomalies and with the increase observed near the stratopause in the tropics, both of which are discussed in Nedoluha et al. ("Variations in middle atmospheric water vapor from 2004 to 2013", JGR 2013).

Following this comment and a comment from referee #1 we will change the text at P7986 L13–14 to:
Therefore, our study indicates that the increase of water vapour mixing ratios in the tropical lower stratosphere is real. It agrees qualitatively with an observed temperature increase in the TTL after 2001 and an increase of water vapour near the stratopause between 2004 and 2013 (Nedoluha et al., 2013).

In several places "decent" is incorrectly used instead of "descent"

We will correct the text.

Literature

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