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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "A self-adapting and altitude-dependent regularization method for atmospheric profile retrievals" *by* M. Ridolfi and L. Sgheri

Anonymous Referee #1

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General:

The paper presents a new method for the determination of an altitude-dependent regularization strength applicable to atmospheric parameter retrievals from remote sounding observations. The approach is successfully applied to simulations and to real observations of MIPAS limb emission spectra. This new technique is definitely worth being published. Since it concerns rather technical retrieval aspects, the paper would ideally be suited for the ACP sister journal AMT. Within the scope of ACP I would rank it between a 'Technical note' and an ordinary paper.

Specific:

p.18009, l.19...:

In addition to the oversampling of MIPAS observations starting from 2005, possible reasons for oscillations in MIPAS profiles retrieved with the ESA processor should be mentioned (e.g. systematic errors in calibrated spectra or in the radiative transfer modelling like non-LTE or spectroscopic data). E.g. the oscillations above 55 km in the water vapour profiles shown in Fig. 8 cannot be due to oversampling which is only applied at lower altitudes.

p.18010, l.10: 'In the case of the retrieval of vertical atmospheric profiles from spectroscopic limb measurements, ill-conditioning produces oscillations in the retrieved profiles beyond the error margins defined by the mapping of the measurement noise into the solution.'

I doubt that this correct when only measurement noise is considered. In that case, oscillations in the profile should be described properly by the estimated error bars. Additional instability of the profiles might be due to systematic errors which are not covered by the error estimation. In any case, the role of regularization for smoothing of systematic error-induced oscillations should be mentioned in the paper.

p.18010, l.23: 'When i=0, Tikhonov regularization is equivalent to the optimal estimation (OE) method introduced by Rodgers (1976).'

Please mind that this is the case only when the OE a-priori Sx-matrix is diagonal. In general, isn't OE is a kind of combination of different orders of Tikhonov regularization?

p.18012, l.13:

It should be explained why for the retrievals shown in the actual paper the inversion of $K^T S_y^{-1} K$ is unproblematic. However, for other applications, like nadir-retrievals on a fine altitude grid the calculation of x_{LS} is not possible. As I understand, you suggest to use an OE regularisation before applying your scheme. Could you explain this a bit more in detail?

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p.18018, l.7:

Is the a-priori profile used for regularization the same as the initial-guess profile or is it set equal zero? Could you show the effect of a different a-priori (e.g. zero instead of the initial guess) on the results?

p.18018, l.20:

Could you comment on a possible reason for the relatively large difference between the 'Reference' and the 'Retrieved VS' values at the top end of the profile (70 km).

p.18018, l.20: 'Only below 18 km the regularization introduces a noticeable smoothing error,...'

I doubt that the differences between 'Reference' and 'Retrieved VS' at around 15 km altitude is really a smoothing error. When you look closely at the profiles in that region, the 'Reference' is much less structured than the 'Retrieved VS' which indicates that there are some remaining oscillations in the 'Retrieved VS' profile which are not described correctly by the estimated error bars. What might be the reason for these instabilities? The sharp gradient of the profile at 20 km or the sudden increase of the regularization vector by several orders of magnitude (Fig. 1d) just above 15 km? It is important to understand such kind of errors in the UTLS (upper troposphere, lower stratosphere) since this is a region of great importance e.g. with respect to climate change issues.

p.18019, l.10...:

In Fig. 1c the vertical resolution at around 60-70 km altitude is 10-15 km, however, as one can see from Fig. 2 there are no averaging kernel rows peaking at these altitudes. Thus, here the FWHM criterion applied seems problematic. Could the authors comment on possible implications on the VS regularisation scheme which uses also at those heights the FWHM criterion.

p.18021-18022, chapter 4.2 and 4.3:

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It is problematic to demonstrate the advantages of the new regularisation approach compared to other approaches on basis of measurements where the true profile is not known. E.g. the structure at 25-30 km in the CH4 profile might be real, however, it is smoothed out by the new regularization, as well as structure in the H2O profile at 15-20 km. Thus, for the intercomparison of different approaches I strongly suggest that the authors either use observations where the real atmospheric state is known from independent validation measurements. In case this is not possible, the comparison should better be performed on basis of simulations.

p.18022, l.5-10:

As mentioned above, Fig. 8 shows that some structure of the H2O profile at 15-20 km is smoothed by the new regularisation resulting in a vertical resolution of 5-7 km. In my opinion such a degradation of the vertical resolution at these altitudes is not acceptable regarding the small estimated error bars of the LS solution in that region.

Also in this paragraph, I suppose the authors wanted to point to the profile 'above the stratopause' instead 'above the tropopause'.

p.18022-18024, chapter 5:

Which a-priori profiles are used? Is there an effect of the a-priori on the results?

I agree with the comment by Susan Kulawik, that it would be nice to see results of a simulated entire orbit also for better comparison with the EC method.

Technical:

p.18025, l.10: 'extention'

should read: 'extension'

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