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Atmospheric
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Discussions

Interactive comment on "Performance of the line-by-line radiative transfer model (LBLRTM) for temperature, water vapor, and trace gas retrievals: recent updates evaluated with IASI case studies" *by* M. J. Alvarado et al.

Anonymous Referee #1

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Referee Report

Performance of the line-by-line radiative transfer model (LBLRTM) for temperature, water vapor, and trace gas retrievals: recent updates evaluated with IASI case studies.

M.J.Alvarado et al ———

GENERAL COMMENTS

The paper describes an analysis of spectral residuals from a set of IASI spectra from two different versions of the LBLRTM in order to establish the impact of modifications between the two versions both on spectral residuals and on retrieved profiles.

LBLRTM is an important tool in the interpretation of IASI data, and this paper attempts to address scientifically useful questions. Unfortunately, with the approach used, I do not believe the conclusions are robust enough to be publishable.

The ideal way to approach such a task is to use the 'true' atmospheric profile for each of the IASI scenes, run the LBLRTM, and compare the results with the IASI measurements. However, the authors argue that the best estimates of this 'true' profile, from ECMWF and climatologies, are insufficiently accurate for these purposes and instead modify this profile using a retrieval based on the LBLRTM itself. This has two undesirable effects: it can mask spectroscopic errors which are simply absorbed into the retrieved profiles, and also leads to complications in propagation of, particularly, temperature and water vapour errors into residuals of other species.

SPECIFIC COMMENTS

My main criticism, then, is that results are not presented on the basis of residuals using the a priori profile. This would have provided a simple, and direct, comparison of the performance of the two models. If there are uncertainties in the true profile, these should be characterised and propagated through the forward model as uncertainties associated with the residuals. If there are clear deficiencies in the a priori, then these should be carefully identified and adjusted, and examined a posteriori to verify that the adjustments are reasonable.

Here we do not see what impact the retrievals have had on the a priori estimate, or if the resulting profiles are physically reasonable (not, for example, something with wide oscillations or unphysical surface-lower atmosphere temperature discontinuities). And, for the analysis of residuals, there should still be a single true profile for each scene, not one which depends on the model. However, by using the two different model versions, two different versions of the 'true profile' are created which appear to differ significantly from each other (but I could see no example of how they differ from the a priori). So, the spectral residuals analysed here are some complicated combination of spectroscopic errors, forward model errors and retrieval artefacts. The paper itself becomes vastly more complicated seems necessary, just from attempting to untangle these effects.

If the resulting residuals are clearly (ie visually) improved, then one might overlook this. But in most cases the improvements can only be identified by examining the RMS or bias (incidentally, unclear which is preferred, and why).

So, of the various conclusions, I am inclined to believe that the CO2 modelling in v12.1 is better than v9.4 - although even here it is not clear whether this is due to the underlying spectroscopic data or the inclusion of P,R line-mixing in v12.1. However, nothing else seems to point unambiguously to v12.1 being more accurate than v9.4.

I would also be wary of assuming statements on the impact of these changes on temperature and water vapour retrievals are generally applicable. Most assimilation or operational retrieval schemes do not use the complete spectral range as used here but just a small selection of channels. For these, the retrieval characteristics, hence impact of LBLRTM differences, may be quite different.

Other comments:

HDO Depletion The argument that HDO depletion is the cause of some of the spectral residual differences seems entirely plausible but, following the rest of this paper, I would have expected the argument to be made by demonstrating that a reduced HDO concentration results in a reduced RMS. Instead, it is argued that the HDO depletion causes a bias, but since I assume the retrieval attempts to minimise the RMS of the spectral residual it is not obvious to me that the result would be a bias rather than an enhanced RMS - it is, after all, just equivalent to another spectroscopic error and the argument elsewhere is that these increase the RMS difference.

H2O R-band If HITRAN data produces a larger bias while keeping the RMS unchanged, doesn't this mean that the scatter (SD) about the mean must also have been

reduced with HITRAN? Otherwise you would expect RMSˆ2 = SDˆ2 + Biasˆ2 In fact, coupled with the results of Table 4 I would suggest that the HITRAN data for the R branch is better than using AER, ie that v9.4 is better here than v12.1

TECHNICAL CORRECTIONS AND MINOR COMMENTS

p80 l14: "below 10hPa" is ambiguous. Presumably "altitudes below the 10hPa surface" is what is meant.

p81 l26: "MeteoSat" should presumably be "Eumetsat"

p83 l3: suggest adding RTTOV to the list of fast RTMs that are trained on LBLRTM spectra.

p83 l19: the heavy molecule cross-section data supplied with HITRAN is generally both temperature *and* pressure-dependent.

p87 l12: "3.1 p/m 3.3 x 10ˆ{-7}" is presumably a typographical error

p88 l23/24 "below 10hPa" and "above 0.1hPa" also ambiguous.

p89 l19: "a priori spectral surface emissivity" implies that the surface emissivity was also a retrieved parameter. I assume it wasn't since there is no other mention of it.

p89 l21: there is no mention of the number or location of the retrieval levels in each profile.

p89 l24: was surface pressure retrieved?

p89 l12: was any a priori correlation assumed between T_srf and T_atm for the lowest temperature profile level.

p91 l11: any particular reason that the CO correlation was 2 km while 1 km seems to have been assumed for everything else?

p91 Eq1: it would be useful to have an approximate figure for how much of this RMS total can be ascribed to instrument noise.

p92 l12: it is claimed that the reduction in residual is due to the inclusion of P and R branch line mixing, but how do you know that it is this and not simply the difference in the basic CO2 line parameters between the two versions?

p92 l24: it seems highly unlikely to me that these residuals are affected by water vapour - more likely the atmospheric temperature structure.

p93 l20: could it be that the residual at 667cm-1 represents poor vertical resolution of the stratopause in your retrieval?

p93 l13: convert this figure to brightness temperature.

p93 l24: there is an implication in this statement that the oscillations in Fig.6 come from the v9.4 retrievals. However, the real test would be to compare the retrievals against the a priori profiles to see which one has the best RMS difference - but that's not shown.

p94 l10: Fig 1 suggests that a version of the T retrieval *is* performed using the v3 band, but here it is stated that residuals plotted are based on the v2 band T retrieval. So what was the purpose of the T retrieval using the v3 band? This is actually explained later (p95) but I was confused at this point.

p121 Fig3 (and elsewhere) it seems odd to describe a spectral region of more than 100cm-1 width as a 'microwindow' (which are more typically ∼1cm-1 width).

p125 Figs7-9 y-axis should be in Kelvin for consistency with other plots?

p142 Fig23: radiance units rather than BT?

p142 Fig24: optical depth for convolved spectra is an ambiguous quantity I assume this represents -ln(convolved transmittance) rather than convolved (-ln(monochromatic transmittance)).

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 79, 2013.