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Atmospheric Chemistry and Physics 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 #2

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General Comments:I have read with interest the paper by Alvarado et al. that deals with a thorough investigation of the performance of the LBLRTM forward model. The paper covers aspects that are very relevant for the users of satellite data. I think it is a good paper based on sound science although no new concepts or ideas are introduced (with thepossible exception of the investigation of the mis-specification of the water vapour isotopic ratios). My main criticism is that sometimes (probably due to the vast amount of material presented in the paper) the discussion of the results is lacking in detail. Possible alternative interpretations of the results are overlooked or discarded

altogether. The number and the quality of the figures is adequate. To summarise, I think the paper should be accepted for publication with minor modifications. I invite the authors to take my comments constructively and pursue the publication of the paper.

Specific Comments:

Abstract

First paragraph: I do not entirely agree with the use of "critical" in the following statement "Reducing the uncertainties in our knowledge of spectroscopic line parameters and continuum absorption is thus critical to improving the application of satellite data to weather forecasting". Instead of "critical" I would suggest the use of, for instance, "an important factor". Forward modelling is a key element in the assimilation of satellite data but it is not the only factor controlling the impact of satellite data on weather forecasts. To my knowledge, it has never been demonstrated that the use of an improved spectroscopy has critically improved the quality of weather forecasts.

Introduction

Page 81, Second paragraph: strictly speaking, NWP centres (or at least the vast majority of them) do not retrieve atmospheric profiles. They assimilate satellite radiance data using variational data assimilation techniques. Rather then "retrieved", the authors should speak of "analysed" profiles.

Page 81, line 16: again, I do not agree with the use of "critical".

Section 2.2

Page 86, line 18: what is the justification for the use of empirical scaling factors in the CO_2 continuum model?

Page 86, line 20: this sentence can be misleading. Do you actually mean the temperature dependence of CO_2 continuum? More in general, could you please clarify the nature of this correction? What about the CO_2 continuum in the nu2 band? Could you please give any detail?

Section 3

Page 87, line 7: "1/e" should be defined explicitly in the text.

Page 87, line 15: Could you please clarify what criteria did you use to assess the cloudiness of a scene? How confident you are the spectra are not affected be residual cloud contamination?

Page 87, lines 15 to 20: why do you think 9 profiles failed to converged and one profile showed biased residuals?

Section 4

Page 88, line 23: I wonder why the authors have chosen to use a priori profiles obtained from an older version of the ECMWF model. I think it is very likely that a more recent version of the model would have produced more accurate a priori profiles. Is this choice in any way related to the Matricardi (2009) paper often quoted by the authors? If this is the case, could you please clarify why? Could you please also clarify what do you mean by ECMWF model output? Have you used analysed (i.e. retrieved) profiles or background (i.e. forecast) profiles?. I am asking this because it could have implications for the accuracy of the profiles.

Page 89, line 3: the authors quote that water vapour profiles are taken from the ECMWF model as discussed in Matricardi (2009). I doubt this can be the case. If I am not mistaken, Matricardi (2009) uses very short range (3 hour) forecast profiles obtained inside the 12 hour 4D-Var assimilation window and, to my knowledge, this profiles are (usually) not publicly available. The use of 3 hour forecast profiles has the obvious implication that these profiles can be closely matched in time to IASI observations, an aspect that is of particular relevance for humidity fields. Could you please provide any clarification?

Page 89, line 20: the authors state that spectral emissivities have been computed at

zero viewing angle. Does this mean that they have used only IASI spectra at Nadir (i.e a Zenith angle of zero degrees at surface)? Please clarify.

Page 90: could you please clarify what is the basis for the choice of the error covariance values used in the retrievals? How do you justify that choice?

Section 5.1

Page 92: lines 19 to 25: the authors provide evidence of a dependency of the residuals on PWV (at least in some spectral regions). Why do they think residuals should increase in warmer and wetter regions? What mechanism should be responsible for that? Is there any inherent weakness of the LBL model that could be responsible for that?

Page 93, line 7: the authors mention Fig.12 of Masiello et al. (2011). I think it would be very informative if , following Masiello et al, the authors could also plot the error in mean residual due to noise.

Page 94, line 16: in contrast to the previous Figures, in Figure 7 the residuals are expressed in terms of radiances rather than brightness temperatures. I fully understand the reason for that (i.e. the strong non linearity of the Planck function in the short wave) and had like to have a sentence in the text explaining it. This said, the authors should notice that it is very difficult to check the consistency of the results in the long wave and short wave regions if results are expressed using different units.

Page 94, lines 20 to 25: I must admit I am not fully convinced by the N2O argument. Does the supposed day to day stratospheric N2O variability account for such a large signal? After all, the weighting functions of many of these channels peak in the middle to lower troposphere. Why have the authors totally discarded any issue related to the CO2 spectroscopy in that region?

Page 94, line 26: it seems to me that the residuals in the 2200 cm-1 to 2250 cm-1 region have a strong dependence on PWV in both versions of LBLRTM. Yet, there is no mention of this in the text. Why have the authors chosen to ignore this aspect of the results?

Page 95: ljnes 1 to 11: what the about the contribution of foreign water vapour continuum in this region? It has been suggested in the literature that models like MT_CKD can underestimate its contribution by an order of magnitude.

Page 96, lines 1 to 4: I agree that profiles retrieved with LBLRTM v12.1 are more consistent. However, I cannot fail to notice that in the middle troposphere differences between profiles retrieved using the nu2 and n3 bands can still reach 2K. Do the authors have any opinion regarding the possible source of this discrepancy?

Section 5.2:To what extent the residuals can be affected by errors in the (foreign?) water vapour continuum? This aspect of the discussion is conspicuously absent in the paper. If the authors think that water vapour continuum is not relevant in this spectral region, then they should state it explicitly in the paper.

Section 5.4: How do the authors intend to modify the foreign continuum?

Section 5.5: Are the authors suggesting that an improved spectroscopy is potentially degrading the residuals?

Conclusions: this section should be updated based on the response to the comments above.

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