Atmos. Meas. Tech. Discuss., 3, C1425–C1429, 2010
www.atmos-meas-tech-discuss.net/3/C1425/2010/

Atmospheric Measurement Techniques Discussions

Interactive comment on "

A new optimal estimation retrieval scheme for carbon monoxide using IASI spectral radiances – Part 1: Sensitivity analysis, error budget and simulations" *by* S. M. Illingworth et al.

Anonymous Referee #2

Received and published: 10 September 2010

General Comments

This manuscript describes the development and testing of a new optimal-estimation based retrieval algorithm for IASI observations of CO. Validation of the resulting products is not addressed in this paper. Such retrieval algorithms have many facets, each

of which is very important to the quality of the final product. While several sections of the paper require significant additional work, most of the paper is scientifically sound, fairly well written and appropriate for AMT.

Specific Comments

Introduction

Authors should add references for SCIAMACHY and MOPITT NIR CO products.

Sect. 2.

In second paragraph, authors report radiometric accuracy of IASI, but not noise characteristics. Both quantities relate to instrument performance and should be discussed in this section.

Third paragraph should note that clouds greatly reduce the actual daily retrieval coverage from the ideal.

Sect. 3.1.1.

The description of the forward model requires greater detail, since even small deficiencies in the forward model can result in large retrieval biases. Specifically, the following questions should be addressed. Does the longwave radiation term account for atmospheric emission reflected by the earth's surface? How is the reflection modeled (e.g., specular vs. Lambertian)? The representation of the solar term (Eq. 2) seems overly simplistic, since it neglects the dependence of the optical depth on wavelength, CO concentration, water vapor concentration, etc. What evidence is there that this equation is appropriate? Has this parameterization been validated? What is the radiative uncertainty associated with this parameterization? How does this radiative uncertainty quantitatively affect the retrieval errors?

What is the uncertainty in the forward model due to uncertainties in the spectral database (HITRAN)?

Sect. 3.1.3.

The Levenberg-Marquardt method is often used for highly nonlinear problems which are not suited to simpler iterative methods (e.g., Gauss-Newton iteration). Why were simpler methods not considered?

Sect. 3.2.1.

Authors should clearly state how each state vector parameter is quantified (e.g. VMR vs. logVMR for CO, specific humidity vs relative humidity for water vapor, etc.).

Sect. 3.2.2.

Arguments in first paragraph concerning grid selection are relevant to visualization and interpretation of K and A, but not to their validity. Using an irregular grid by itself is not a source of retrieval error. This point should be made.

Sect. 3.3.

The meaning of the last half of the sentence 'Being an OEM retrieval scheme, the ULIRS makes use of a priori knowledge relating to the quantities that are to be retrieved, with the a posterior retrieval weighted by the choice of the inputted data.' is unclear.

Sect. 3.3.3.

The development of the CO a priori profile is unusual and will clearly lead to retrieval biases. The authors state that '... to avoid the a priori being heavily biased by background concentrations of CO, only proïňĄles where the surface concentration of CO was greater than 100 ppbv were considered.' This seems backwards. By excluding all the profiles that represent background concentrations of CO, the a priori profile will be valid only for polluted conditions. When such an a priori profile is used for retrievals in relatively clean areas (near background concentrations), retrievals at levels with low sensitivity will be positively biased. Preferably, the a priori profile should be recalcu-

lated without discarding any profiles, since this will lead to a much better estimate of the true climatological CO profile. At the very least, the authors should clearly discuss the expected positive biases resulting from the method used to calculate the a priori profile.

Sect. 3.5.1.

Should the text 'where m is the number of measurement vectors' really be 'where m is the number of measurements'? Isn't there just one measurement vector?

Sect. 3.5.2.

The argument in the first paragraph (concerning radiative effects of scattering) considers the effects of absorption, but not emission. These arguments are valid for near-infrared wavelengths, but not thermal infrared (which is more complex). I would suggest deleting this paragraph, since it's already clear that the radiative transfer model is only valid in clear-sky scenes.

Sect. 4.2.1. Maps of DFS over the study region (daytime and nighttime) would be interesting and would clearly demonstrate the geographical dependence of the retrieval performance.

Sect. 4.2.2.

The first paragraph requires more detail concerning the calculation of forward model parameter error (epsilon_param). For example, what assumptions were made regarding the variability of the non-retrieved trace gases?

Sect. 4.3. This section clearly demonstrates that the retrievals are sensitive to the presence or absence of the solar term, but does not demonstrate that the solar term is properly represented (see comments for Sect. 3.1.1. above).

Sect. 5.

The four profiles selected for simulations are somewhat limited. In particular, the inclu-

sion of a CO profile exhibiting a clean lower troposphere and polluted mid-troposphere would be an interesting case.

Inspection of Figs. 20 and 21 shows that the retrieval uncertainty is reduced (relative to the a priori uncertainty) at all levels, including the surface. This is somewhat surprising for the over-ocean retrieval where the Jacobians are likely to be very weak at the surface. This likely indicates the 'information projection' effect discussed in Deeter et al. (2010) and should be discussed.

Conclusions.

In the second paragraph, simulation results for retrieved total column are reported (e.g., total errors ranging from 18 to 34%). These results do not seem to appear in the main body of the paper. These results should be provided with more detail somewhere in the main body of the paper.

The authors should discuss the practical limitations of the new retrieval method. Specifically, is the computation time a factor in processing large amounts of data (e.g. months or years) over large areas (e.g., globally)? What steps would be required to allow its use to produce operational retrieval products?

Technical Corrections.

Right parenthesis missing at end of line 22 on p. 3752.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 3747, 2010.