

Interactive comment on “Tropospheric CO vertical profiles deduced from total columns using data assimilation: methodology and validation” by L. El Amraoui et al.

Anonymous Referee #2

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The main message of the paper is summarised on page 6538: "The aim of this paper is to show the benefit of using satellite CO total column data with no associated error covariance matrices and averaging kernels within an assimilation system. " After reading the paper I arrived at several fundamental objections which are detailed below. As such I cannot support publication of this paper in AMT.

On page 6521 the authors write: "The proposed method has the advantage of allowing fast computation of the vertical profiles and the analyses of CO. It will be particularly useful in the future when there will be many missions providing large volumes of data for which level 2 retrievals with their corresponding characteristics (covariance matrices

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and averaging kernels) will be very expensive in terms of computer resources (i.e., IASI onboard METOP-A and METOP-B or future geostationary missions). " The authors argue that the removal of information stored in the kernels and covariances is valid and useful for future applications. According to the book of Rodgers, the information stored in these kernels and covariances is non-trivial and important. It provides the details of the sensitivity profiles of the satellite observations and the uncertainty, which may vary considerably from one observation to the next related to changes in thermal contrast, albedo etc. Data volumes are huge, but this is not generally considered to be a major problem. Computer capabilities (storage, processing speed) are also growing in time. If needed, data thinning or grouping into "superobs" may be considered. Furthermore, as shown by Migliorini (doi: 10.1175/2007MWR2236.1) there is an efficient way of storing satellite information without losing information. To my opinion such approaches are to be preferred, and the paper was not able to convince me otherwise.

Conclusions: The authors conclude by mentioning that the approach will be applied to other satellite measurements (in particular IASI). This more or less implies a recommendation that the approach of neglecting kernels and covariances is more generally applicable. To my opinion such a statement should not be made without justification, and the approach of simplifying observations should be extensively tested for each dataset and assimilation system separately.

Abstract: "Second, for chemical species that can be measured only as the total column, this method provides an attractive alternative for estimating their vertical profiles in the troposphere." Conclusion: "The total column analyses permit us to give relevant information on the vertical structure of the CO field at global and regional scales." The authors claim that total column measurements can constrain profile shapes. This does not make sense. With one piece of information one can only constrain one aspect of a model. A local column measurement can not constrain the local profile, apart from an overall scaling.

What is a typical Degrees of Freedom for Signal (DFS) for the MOPITT CO obser-

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variations used? This is crucial information but this information is not provided in the paper. When the DFS is close to = 1 the profile observations cannot be viewed as independent of the total column observations! Very similar assimilation results may be expected when furthermore the a-priori is not too different from the model simulated profile.

Concerning the chi-square analysis: I do not see how this can uniquely constrain the observation error. This is only possible when B is accurately known (or known to be small compared to R). There is no proof of this in the paper. Normally, the chi-square test is used to constrain B based on a fixed R provided in the retrieval dataset.

The background covariance B optimisation and structure should be discussed in much more detail for the reader to appreciate the meaning of the results. The B matrix will determine how the total column measurement affects the profile shape.

Because chi-square is a single number it can at best constrain only the average error, and does not provide information on the variability from one measurement to the next. How large is the variability of the observation errors in the retrieval product? Is this variability reflected in OMF? This aspect is not discussed.

For other data assimilation approaches with an other chemistry model and a different formulation of the B matrix there may possibly be much larger differences between the column and profile analyses. I am not at all convinced that the results can be generalised to other assimilation systems and other satellite datasets.

I would like to stress that the objections given above concern the conclusions and objectives of the paper. The MOCAGE-PALM assimilation system seems to be working very well, producing high quality analyses.

In addition I have a few related detailed remarks:

To my taste there are too many plots in the paper. All the details on the global distributions, zonal means, regional means and vertical profiles are not needed for the main

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conclusions, and the results sections may be shortened.

6522, l25: What is a typical value and range of DFS ? This is crucial information.

6527, l20: "Chi-square close to 1 indicates consistency between both error covariances." I would dispute this. It shows that the sum of both covariances is consistent with the observed OMF.

6528, fig 1: First I would like to see evidence that B is well specified. The optimal value of R will depend on the construction of B.

6533, fig 10: It is important to know what the (average) DOF is in these cases. If this is small there could be a lot of a-priori information mixed in, and a good comparison of the profiles is not very meaningful.

6535: It would be interesting to know why the assimilation of total columns and profiles (with kernels, covariances) gives very similar results. Are the a-priori profiles used in MOPITT similar to the MOCAGE profiles?

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