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Interactive Comment

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 #3

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Review of "Tropospheric CO vertical profiles deduced from total columns using data assimilation: methodology and validation" by L. El Amraoui et al.

The main subject of this paper is a comparison of total columns and vertical profiles as observational input for data assimilation of CO. The paper is a struggle to read because it provides excessive detail on uninteresting things while giving little or no attention to the real issues. For example, the specification of the observation error is not controversial or new, so it could have been covered in a few sentences. At the moment it requires an entire section, one table and three complex figures. In contrast, the specification of the background error covariance matrix is crucial to the data assimilation problem, particularly for assimilation of total columns, yet it is not described.

The main result of the paper is that assimilation of either columns or profiles gives similar results. That contradicts an awful lot of research over the years, as well as common sense, so the unanswered question is: why does this happen? The answer may lie in the way the vertical correlations are specified in the background error matrix (B), or in the information content and number of vertical degrees of freedom available from the retrievals - this comes from the averaging kernel matrix A. Neither of these aspects are properly covered in the paper. To understand what is going on, the authors need to look at how the A and B matrices combine to project information from the observations (column or profile) into model space. Ideally they need to compute this explicitly, but if this is difficult they could show examples of the vertical patterns of increments resulting from the assimilation of single observations on single levels.

In summary, I would like to see a lot of the current paper compressed, to be replaced by a major effort to properly explain why assimilation of columns or profiles makes little difference to the results.

Major points

1) p6521, lines 15-26, provide the justification for this work, which seems weak. It seems to imply that assimilation of columns is a necessity to avoid computational performance problems. However, many groups assimilate profile data from multiple satellites in an operational context, for example the MACC project. The real justification, perfectly valid, is revealed only in the conclusion: the idea is to assimilate CO columns from IASI, where presumably there is not enough information to provide a vertical profile.

2) The description of the DA system in sections 3.1 and 3.2 lacks some essential details, principally a description of the B matrix, and it seems confused in some of the notation (that will be covered in minor points).

AMTD

6, C2777–C2781, 2013

3) In section 2, the chi squared method is presented in excessive detail yet it ignores the real issue. The time variation of chi squared would be uninteresting for one of the three data categories (land day, land night, ocean) let alone all 3. It would be enough just to present table 1 without showing figures 1-3 at all. The real issue is that the chi squared method assumes the background error variances are correct in magnitude. The quantification of the observation errors makes sense only if this is true.

4) Sections 4 and 5 could easily be compressed to make for a lighter and more interesting read, and to make room for an explanation of what causes the results. For example, figures 5, 6, and 7 could be combined. There is no need to always split the observations into the different groups of land/day, land./night and ocean. The authors should only split things out when it reveals a feature of real interest.

Minor points

1) Abstract. This is over-detailed (is there any need to give details of the chi squared errors?) and does not need to include labels like "LAND_DAY", "LAND_NIGHT" and "TOTCOL_ANALYSES" and "PROFILE_ANALYSES". These capitalised labels are in general a bit of a distraction and make the paper harder to read, not easier.

2) Page 6519, line 23 to 6520 line 5 gives a long list of instruments and acronyms that have nothing much to do with the current paper. Such a long list is quite hard to read. It would be more accessible if it were given in a table, but really I don't think it is necessary at all.

3) Page 6520, lines 6-14. A list of the constituents retrieved by IASI and a description of the METOP satellites is pointless in the context of the current paper.

4) Page 6523, lines 10-14. This very long list of papers is not useful for understanding the current work; it should be pruned or removed.

5) Page 6524, lines 13-15. Again, this is a long list of citations that brings nothing to the current work. Things should only be cited if they have a direct relevance.

AMTD
6, C2777–C2781, 2013

6) Page 6525, lines 8-18. Again, a list of citations that adds nothing to the understanding of the current work, and should be removed.

7) Section 3.1: notational problems and confusing description:

a) In equation 1, do you really mean H(x_i), not H(x)? In other words, where does the forecast model come in, how is information propagated from the analysis time to the observation time?

b) Is p really the number of degrees of freedom? Conventionally i=1,p represents the different time-steps in the model. See Ide et al. (1997, J. Meteor. Soc. Japan) for standard DA notation conventions.

c) Equation 2 seems to have lost some subscripts compared to equation 1. Where have they gone? The text should explain.

d) p. 6525 line 21 to 26. It's not clear what these lines are attempting to say. Whatever it is, it could perhaps best be explained with mathematical notation, or else with a much more carefully explained piece of text. As I read it now it is wrong, anyway. The increment is not just a projection through HˆT but also through the background error covariances.

8) Section 3.2, as mentioned before, a section on "error specification" needs to include the background errors.

9) p. 6529, line 16; "this indicates .. the bias .. is reduced". Standard deviation says nothing about bias, this cannot be true as written.

10) Section 4.1 seems to have a strange viewpoint in which the analyses provide the reference against which the MOPITT vertical profiles are being validated! How can the authors justify this, given that the analyses are a combination of CTM and total CO observations? Are they saying that the CTM is more accurate than observations? Examples:

AMTD

6, C2777–C2781, 2013

Interactive Comment

a) Page 6530, line 21: [biases in the lower troposphere] "could be explained by the reduced sensitivity of MOPITT .. at lower levels".

b) Page 6531, lines 4-7 similarly

c) Page 6531, lines 16-19 similarly

Really, the biases between the analyses and the MOPITT vertical profiles are a diagnostic of how the assimilation system is working, not an absolute verifcation of the quality of MOPITT. As an aside, some independent validation of the CO fields would have been nice in this paper; everything is self-circular at the moment.

11) Page 6532, lines 21-23. "the spatial extent..." This sentence doesn't make sense.

12) Page 6532, lines 17-28. This introduction/justification seems a bit excessive; it could easily be cut down.

13) page 6533, line 12: Vertical profiles "agree within the standard deviation of both datasets". This is spurious; the t-test for the difference of two populations is based on the standard deviation divided by sqrt(population size), a much smaller number. Statistically, these pouplations are probably very different; this does show in the rather low correlations exhibited in Figure 12.

14) page 6533 line 17: "mean RMS" - this is confusing and needs some explanation

15) page 6534 lines 5- 15: analyses and observations are "generally in good agreement" - this is unconvincing, given the low correlations in Fig. 12. Also, this section is a further example of the curious attitude where the analyses are seen as verifying the MOPITT retrievals. Here, the lack of sensitivity of MOPITT to the lower troposphere is blamed. This is another place where a look at the A and B matrices would help illustrate what the authors are trying to say.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 6517, 2013.