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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the extraction of wind information from the assimilation of ozone profiles in Météo–France 4D-Var operational NWP suite" *by* N. Semane et al.

Anonymous Referee #1

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General Comments

In this paper 4D-Var assimilation of EOS MLS ozone data is shown to reduce analysis and forecast errors in lower stratospheric wind fields. Degrees of freedom for signal (DFS) and error variance reduction diagnostics show the high information content of lower stratospheric EOS MLS observations relative to tropospheric humidity-sensitive radiances, and the large contribution to the reduction of the horizontal divergence background error variance from these observations is also demonstrated.

This is a generally good paper which contains results of interest for the general meteorological community. It is quite close to being acceptable in ACP as it stands. However,

I would like to see some changes related to the paper layout and to the discussion in parts of the paper, which, if implemented, would improve the paper. These are listed below.

Specific Comments

Paper layout: For the general meteorological community, I think the headline result is the fact that assimilating EOS MLS ozone data reduces lower stratospheric wind errors. However, this result (Figure 7) is stuck at the end of the paper almost as an afterthought! This result needs to be more prominent and the way to do this is to move Figure 7 ahead of Figures 5 and 6 and to discuss the wind errors prior to the DFS and error variance reduction diagnostics. These diagnostics are a very useful way of deducing the impact of the EOS MLS ozone data, but this analysis should appear after the key results (ie Figures 4 and 7). Other parts of the paper (eg the abstract) also need to be changed to reflect the reorganisation of the paper.

pp 16479-16480 and Figure 1: The use of MOCAGE and ARPEGE fields interchangeably in the ozone assimilation is probably largely beneficial, but there are issues related to different biases and other errors in the two models that could lead to difficulties. Two extra pieces of work would be useful in this regard:

a) Since the EOS MLS data are highly effective at constraining ozone analyses in the lower stratosphere, and the ozone photochemical lifetime is long here, it makes sense to perform another experiment where the MOCAGE step is missed out all together and the 6 hr forecasts are produced using ARPEGE only. This would test out the hypothesis at the top of p 16480 that is doesn't matter whether MOCAGE or ARPEGE is used for background error calculation (and, by extension, the 6 hr forecasts). If the authors have already done this experiment, it may be useful to report the results of it in the paper. If not, it's worth discussing in more detail exactly what extra MOCAGE gives you compared to ARPEGE in a 6 hr assimilation window (eg without MOCAGE is there drift within the 3 month experiment period?)

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b) The background error is calculated using 12 and 36 hour ARPEGE forecast differences. The hypothesis on p 16480 (see a) above) may actually be weak here, since Figure 1d shows that over a 24 hour period MOCAGE and ARPEGE fields differ by a relatively large amount (often over 10%) near the tropopause. It would be useful to recalculate the background errors using MOCAGE forecast differences and compare them with those calculated from ARPEGE forecast differences. What impact might this have on the results presented?

p 16480 l 15-18. Using an ozone climatology in the assimilation of HIRS 9 and AMSU 18 radiances may be problematic because this may lead to a degraded assimilation of these data, compared to the case where background ozone is used in the radiance assimilation. This is particularly important given that both channels have ozone Jacobians that peak in the UTLS, and since assimilating EOS MLS data seems to give a more accurate analysis in this region (see Figure 4: this Figure could be extended to plot departures of the Fortuin climatology from observations to confirm the above). Thus, using background rather than climatological ozone in the radiance assimilation could lead to even larger positive impacts of wind errors than those shown in Figure 7. Again, if the authors have already run an experiment like this, they should report the results from it in the paper. If not, the points raised above should be discussed.

p16481/Figure 4: The results shown are not surprising since the observations (ie EOS MLS) used to produce the plot are not independent, ie they are being assimilated by the system (at least, that is the impression given by the text). So this Figure only shows that the assimilation system is performing properly. To be more meaningful, Figure 4 needs to be repeated using independent observations (eg from ozonesondes).

Minor comments:

p16475, l 19-23: I think at least some of the papers listed (eg Holm et al) take the approach of directly specifying correlations between wind and ozone in the background error covariances. It should be mentioned that this approach has been used in many

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Interactive Comment

of the papers referred to, and clarified that in the study here, the ozone-wind relationship is represented differently, via the tracer transport equation (and its adjoint) and its evolution within the 4D-Var assimilation window.

p16476 l 9 and l 14: "On the one hand" and "on the other hand" do not aid the clarity of the text here. The former should be deleted and the latter should be changed to "In addition".

p 16476 l 21: I don't think you are attempting to "nearly" do anything! Better to rephrase this as "..in this study attempts to meet these requirements as closely as possible through.."

p16477 Sect 2.1: Numerous other EOS MLS ozone assimilation studies have recently appeared and these should be referenced - Jackson (2007, QJRMS); Stajner et al (2008, JGR): Feng et al (2008, JGR)

p16481 / Fig 4: More detail is needed in the discussion of these results: Why is northern hemisphere ozone overestimated? (I know a reference to El Amraoui et al is given but a summary of the relevance results from this paper should reappear here); why is southern hemisphere and tropical ozone below the 46 hPa level underestimated?

p16483, l7: Add "if" ("In fact, if it is assumed..")

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16473, 2008.