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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The MACC reanalysis: an 8-yr data set of atmospheric composition" *by* A. Inness et al.

Anonymous Referee #2

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This manuscript presents an overview of an 8-year reanalysis simulation performed under the auspices of the European Union funded Monitoring Atmospheric Composition and Climate project. Under this project the authors assimilated satellite observations of a number of atmospheric trace species, and reactive gases in particular, into a coupled model system using the ECMWF integrated forecast system and MOZART-3 chemical transport model. The authors have performed a very thorough evaluation of the reanalysis, comparing the CO, ozone, and NO2 outputs against independent observations from a number of in situ, ground-based and other satellite observations not used in the assimilation. The authors indicate that while the assimilation of the different datasets improves the modelled fields relative to the independent data, there are caveats in the reanalysis which are attributed to data quality and availability. The manuscript is well written and generally very clear throughout and I recommend it for

publication in Atmospheric Chemistry and Physics subject to addressing the following comments.

General comments:

Section 2.2: I suggest including equations that describe the data assimilation process (i.e. the cost function). This will be useful in the next section when the equations for the observation operator is presented. Retrieval diagnostics such as averaging kernels could do with some clarification. In particular I would point out that these come about due to the optimal estimation approach to retrieving data from the satellite measurements - this may also be helpful in explaining why the NO2 observation operator is different later in the manuscript (I assume NO2 isn't retrieved using optimal estimation).

Section 4: I think the manuscript would benefit from some general comments on how improvements could be made in a future reanalysis of this type. The appendix goes some way to describing some of the relevant issues, which are vitally important to potential users of the reanalysis product, but it strikes me that the authors could link this to similar future efforts in a couple of short statements. In light of some of the issues described throughout the manuscript, a statement in the conclusion on how these issues may affect the usability of the reanalysis as a research tool will be helpful to the reader.

Figures: Labelling of the figures is very minimal. Much of the information is included in the figure captions but I would prefer to see clearer labelling of the axes including units on the figures themselves.

Specific comments:

Page 31249, Line 7: please clarify that this is horizontal resolution - it may also be worth indicating the vertical resolution as the reanalysis spans the troposphere and stratosphere.

Page 31250, Line 4: rearrange the sentence so that the definition is before the acronym.

Page 31253, Line 3: OH has not been here although it is further down the paragraph - suggest moving to here.

Page 31253, Line 11: in what respect is tropospheric ozone harmful? one or two references might be useful.

Page 31254, Line 19: has GRG been defined previously?

Page 31256, Line 5: what are the differences, if any, in the time resolutions between IFS and MOZART-3? some information of the dynamical and chemical timesteps would be useful for the reader.

Page 31256, Line 12: suggest changing “had already” to “has been”.

Page 31258, Line 10: suggest using “photochemical” rather than “chemical”.

Page 31259, Line 9: it would be helpful to the reader if a formula for the data assimilation process could be referred to here.

Page 31261, Line 25: while the averaging kernels have to be provided by the data producers I would point out that the averaging kernels come out of the optimal estimation approach to the retrieval.

Page 31262, paragraph starting at line 26: some of the terms in this paragraph read like technical jargon. I think it would helpful to the reader to explain what the bias correction is to begin with rather than assuming knowledge of the ECMWF system. I would understand that the bias correction is applied to retrievals of the same parameter from different instruments but this isn’t particularly clear here. Also it isn’t clear what the term ’anchor’ refers to and the bias correction description may help with this.

Page 31263, Line 26: suggest changing “within” to “throughout”.

Page 31264, Line 18: the description of the GFED emissions should also make it clear

how non-carbon species emitted by biomass burning are determined - i.e. I assume emissions factors are used, from which source?

Page 31269, Line 23: change “apart” to “apart from”.

Page 31270, Line 5-6: a lot of recent work has been done to better understand issues related to the model errors when assimilating CO data (e.g. Jiang et al., 2011). In particular the convection scheme used in the model transport can lead to quite large discrepancies which would also impact on the long-range transport as the authors state. It would be useful to the reader if the authors could make a stronger statement on this and put the MACC reanalysis in the context of other studies looking at this.

Page 31270, Line 28: suggest replacing “only little” with “limited”.

Page 31271, Line 8: I appreciate that relatively coarse model resolution would not capture fine-scale structure in the MOZAIC profiles, I would at least expect it to get the background CO mixing ratios right - a comment from the authors on this would be helpful.

Page 31272, Line 11-12: there is also higher insolation over the equator - could this also help to explain the lower ozone columns?

Page 31272, Line 15: please clarify that UV instruments can’t measure anything (not just ozone) in the polar night because there is no backscattered solar radiation.

Page 31273, Line 1: are these Brewer-Dobson spectrometer observations?

Sentence beginning on Page 31273, Line 29: I thought this would be fairly fundamental atmospheric science but it is poorly written here. I thought that net ozone production occurs in the tropical upper stratosphere and is transported poleward and downward by the meridional branches of the Brewer-Dobson circulation. The large-scale ascent in the tropics brings other chemical species into the stratosphere such halocarbons and N2O which can further influence stratospheric ozone photochemistry and it isn’t clear to me if ozone is transported from the troposphere to the stratosphere as the authors state. I could be wrong but this should be clarified before publication (a reference could also be helpful).

Page 31276, Line 1-2: this isn't particularly clear from the figure. It will be helpful if the authors could quote some numbers to clarify how large is "large".

Page 31277, Line 2: is the model horizontal resolution the only issue that could contribute to this? could vertical transport also play a role as with the CO assimilation? As with the CO, I would expect the model to at least capture background ozone mixing ratios.

Page 31277, Line 23: include numbers to clarify what "small" means.

Page 31277, Line 26: clarify the "good agreement" between the two datasets - from the figure the bias appears to be fairly persistent throughout most of the reanalysis period at +20-30%. The subsequent sentences clarify the discrepancies but it only appears to be "good" at particular time periods.

Page 31279, Line 20-23: could the differences also be due to no ozone observations being available at nighttime in the assimilation? does the sensitivity, information content and data availability change as a function of season in the assimilation? What about vertical mixing between the free troposphere and PBL? Previous studies (Parrington et al., 2009 and Foret et al., 2009) have looked at this and could be useful to cite here.

Section 3.2.4: it isn't clear if this section is all that necessary and distracts a little bit from the flow of the manuscript in describing the MACC reanalysis - isn't the perspective inherent to the comparison against independent observations? I would recommend removing this section prior to publication.

Page 31281, Line 3: change "as" to "such as".

Page 31282, Line 3: the NOx/CO emission ratio should be described in the model set-up section as I pointed out above.

Page 31283, Line 6: what about large positive biases over Scandinavia in DJF/SON? I assume these are small relative biases? some numbers to quantify the magnitude of the biases would be helpful in the text.

Page 31284, Line 20-27: could this be related to data availability? the authors have already alluded to the challenges of assimilating a species like NO2 with a short photochemical lifetime. does SCIAMACHY observe less in the winter?

Section 3.4: is the section describing the HCHO analysis really necessary? after all this is a paper describing the MACC reanalysis and there are small differences between the control and reanalysis HCHO.

References

Foret, G., L. Hamaoui, C. Schmechtig, M. Eremenko, C. Keim, G. Dufour, A. Boynard, A. Coman, A. Ung, and M. Beekmann (2009), Evaluating the potential of IASI ozone observations to constrain simulated surface ozone concentrations, Atmos. Chem. Phys., 9, 8479–8491. Jiang, Z., D. B. A. Jones, M. Kopacz, J. Liu, D. K. Henze, and C. Heald (2011), Quantifying the impact of model errors on topâĂŘdown estimates of carbon monoxide emissions using satellite observations, J. Geophys. Res., 116, D15306, doi:10.1029/2010JD015282. Parrington, M., D. B. A. Jones, K. W. Bowman, A. M. Thompson, D. W. Tarasick, J. Merrill, S. J. Oltmans, T. Leblanc, J. C. Witte, and D. B. Millet (2009), Impact of the assimilation of ozone from the Tropospheric Emission Spectrometer on surface ozone across North America, Geophys. Res. Lett., 36, L04802, doi:10.1029/ 2008GL036935.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 31247, 2012.