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Interactive comment on “A tropospheric chemistry reanalysis for the years 2005–2012 based on an assimilation of OMI, MLS, TES and MOPITT satellite data” by K. Miyazaki et al.

Anonymous Referee #2

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This manuscript presents a multi-year reanalysis of tropospheric O₃, CO and NO₂, assimilating satellite observations of these species from different sensors into the CHASER chemistry transport model. The performance of the data assimilation in terms of improving both the model initial conditions and surface emissions is thoroughly evaluated for the reanalysis period using ground-based and in situ measurements in a global context. The manuscript is well written with a very clear methodology for the work that was done, and the authors provide a very concise and clear summary as to where studies such as this could go in the future for improving our understanding of atmospheric composition and associated uncertainties. I recommend that the

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manuscript is published in Atmospheric Chemistry and Physics subject to my general comments below.

General comments

Page 8690, Line 24: suggest clarifying what is meant by “bottom-up” in line with the description of “top-down” in the following sentence.

Page 8693, Line 20: clarify that the 2.8 degree resolution is for longitude and latitude?

Page 8696, Line 15: the context for this sentence isn't very clear – suggest linking better to the previous sentence and clarifying that the optimization will lead to the reduction in the initial bias in the a priori emissions.

Page 8696, Line 18: suggest changing to “The EnKF approach always has. . .”.

Page 8697, Line 1: suggest changing to “One difference to the study of. . .”.

Page 8697, Line 15: I'm not sure this statement on the averaging kernel is strictly true. The averaging kernels give the sensitivity of the retrieved state to the true state and the a priori dependence is only removed when the innovation is calculated, i.e. as in equation (4) and not in equation (7) which is what this statement appears to allude to. Please clarify this in the text.

Page 8698, Line 4: suggest using “simulation” or “model simulation” rather than “calculation”.

Page 8698, Line 5: the sentence beginning “Eight series of. . .” isn't very clear in its meaning, please rewrite and clarify.

Page 8699, Line 12: I think “onboard” can be replaced with “on”.

Page 8706, Line 7: Model underestimates of CO is a fairly persistent issue for a number of different models and the low bias relative to MOPITT shown in Figure 2 would appear to be consistent with this. It may be useful for the reader to acknowledge how the

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CHASER global CO field generally performs with respect to other models, and if this could also contribute to the OmF statistics in this study. This is commented on in Section 5.2.1 but could also be mentioned here.

Page 8707, Line 20: I think that this statement on the TES information content is consistent with the literature showing that the TES O3 averaging kernels have distinct peak sensitivities in the lower and upper troposphere – it would be useful to the reader if this was acknowledged/cited here.

Page 8709, Line 1: I think “measurement” here should be “retrieval” to indicate that it refers to the satellite data as the ozonesondes don’t have low sensitivity in the lower troposphere.

Page 8710, Line 28: Is it possible to back up the statement that the improvement in O3 relative to MOZAIC at cruise altitude is due to the MLS assimilation. Have the authors performed any sensitivity tests which quantify the relative contributions of the different assimilated datasets to the analysis? While it is clear that the MLS data could bring about this improvement, a supporting statement could be useful for the reader.

Page 8711, Line 8: “attributing” should be “attributed”.

Figures 9 and 13: I found each panel to generally be too small and could be made bigger using common y-axis titles.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 8687, 2015.

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