

Interactive comment on “Combined assimilation of IASI and MLS observations to constrain tropospheric and stratospheric ozone in a global chemical transport model” by E. Emili et al.

Anonymous Referee #2

Received and published: 15 October 2013

General comments.

Authors propose an original study where IASI and MLS ozone observations are assimilated within a global CTM using a linearized chemical scheme. They show how these ozone observations allow to improve the ozone simulations in the stratosphere and in the troposphere. Especially, errors are reduced in the UTLS and at low latitudes in the troposphere. In general, the paper is interesting and well argued. It is showing how recent satellite observations are able to improve the representation of ozone tri-dimensional fields. It shows also how it is possible to produce accurate global ozone analysis at low computational cost by using a linearized chemical scheme in

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synergy with observations. In that sense, it is of interest in the context of the operational monitoring of ozone. Globally, I think that the study deserves to be published but I propose some minor corrections and remarks that could improve the paper. Remarks and proposition of corrections are made following the plan of the paper.

Specific comments.

Abstract.

Never in the abstract, are author making mention of the fact that a linearized chemical scheme is used in this version of the MOCAGE CTM. Maybe it is more than just a mistake in that case. I have had sometimes the feeling that authors do not fully assume the use of this scheme but in fact this is one of the originality of the paper. Nevertheless, I must admit that the conclusions are going in this direction. Several remarks linked to this point will come all along the text. Line 23-25: “. . .the analysis is found to be little sensitive to the assimilation parameters and the model chemical scheme . . .”. Do I miss something about sensitivity analysis to the chemical scheme. I do not see any. Maybe we can suppose that the chemical formulation could be screened out by the assimilation but it is not demonstrated in this paper. It would have been very great to test a more detailed chemical scheme in this context. It would have made the paper much stronger.

Introduction.

P21457 Line 5-8: please give a reference about the tropospheric ozone chemical formation. The description of the processes authors are giving is very concise and a reference is needed there. P21459 Line 1-3: Maybe authors could be more precise about the results of the work using TES. To my knowledge there are two papers of Parrington with different findings. Is the sampling of TES an issue ? how does it determine its use ? There is a recent paper of Barré et al QJRMS (2013) using MOCAGE, IASI and MLS that should be discussed here. I think that this is a good occasion to come back on the utility of using a linearized chemical scheme.

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2. Ozone observations.

P21462 line 7-8: pixel with TOC DFS lower than 0.6 are removed. What amount of data does it represent ? Why do authors choose this particular value of 0.6 ?

3. Model Description.

P 21465 line 3: a coma is missing after "MOCAGE". Please give a reference for the CARIOLLE parameterization. P 21465 line 13-14: I m not sure that the Geer et al (2007) paper shows that linear parameterization for ozone have similar accuracy than full chemical models. It is much more an intercomparison between reduced schemes. Especially, in the troposphere it underlines large errors (especially biases) of linear models compared to soundings. Once more, you could have mentioned/quantify these weaknesses of the linearized scheme within the troposphere and latter show that the assimilation of IASI allows correcting it.

4. Results and Discussion.

P21469 line 12: "... since detailed ozone tropospheric chemistry ..." . Also dry deposition is not taken into account and probably explain some discrepancies. P21469 line 20: what represent "one vertical grid point"? how much kilometers ? P21471 line 13-25: You mention fire emissions as model deficiencies, is there also an issue with COV emissions over equatorial forest and also their interactions with deep convection ? P21474 line 14-19: I do not understand very well why authors are discarding the MOZAIC data ? Following your sentences, the reader can think that it is done because results are not satisfying. Is it the case ? P21475 line 15: robustness instead of robustness P21478 line 15: instruments were very different in these studies. What about the results of Barré et al 2013 ? For the authors, what could cause this specific difficulty to improve tropospheric ozone at mid and high latitudes? P21478 line 25-27: this low frequency would favor the use of MOZAIC data ... Concerning the surface stations, is it really possible to compare it with a 2°x2° model ? Are these stations impacted by dry deposition ?

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 21455, 2013.

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