

Interactive comment on “Global lightning NO_x production estimated by an assimilation of multiple satellite datasets” by K. Miyazaki et al.

Anonymous Referee #1

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This is a very interesting paper that assimilates data from multiple species and multiple satellite datasets to constrain lightning NO_x (LNO_x) and O₃ chemistry. The results look very credible. It improves understanding in the subject area in terms of providing a new estimate of the global LNO_x source with a relatively narrow uncertainty range that compares well to recent modelling results. It provides additional new insights into the peak heights of LNO_x as well as the effectiveness of the standard cloud top height parametrization over ocean. The sensitivity analysis performed in the paper considers a comprehensive range of possible error sources, and is used to produce an overall error estimate. The discussion is also comprehensive and tackles much of the uncertainty in the subject area. The paper is well written throughout. Figure 1 is a nice schematic of the methodology. I have only a few general comments (in addition to

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specific comments):

General comments:

- 1) The approach considers data assimilation of multiple species to constrain LNO_x and surface emissions sources as well as species concentrations. For Ozone (O₃) the results from the assimilated system are compared to observations in Fig 9. It would be useful to be able to quantify how important the correct simulation of the LNO_x source is in itself for O₃, since much of the improvement will emanate from assimilation of O₃ itself.
- 2) In fact "LNO_x –only" optimisation is discussed in section 4.4, but this text is confusing where it is currently placed since this section refers to Table 3 which shows the relative contribution of assimilation of each of the different satellite datasets on simulated O₃ chemistry including surface and LNO_x sources.
- 3) It is not totally clear, but it seems year 2007 was chosen for both model simulations and for assimilation with measurements? Have any other years been examined to see how well this approach performs in other years?
- 4) There is no validation with LIS/OTD lightning flash rates though this is discussed briefly. In particular, it would be useful to see if there is any seasonality in flash rates over the oceans in line with those found in Figure 6 (when data assimilation is included).
- 5) Figure 7 shows low clouds over oceans producing maximum amounts of LNO_x. Is this signal really due to low clouds or is it that the re-distribution of the LNO_x source towards the surface is greater with assimilation?
- 6) The conclusion regarding IC/CG ratios in the discussion (section 6.2.2) is rather confusing but may be insightful. Was it possible to obtain cold cloud thickness and hence ranges of z values from the satellite measurements, in order to comment on whether ratios of 1 or 10 were more likely?

Specific comments:

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P29204, Line 1 "assimilating observations" add "into a chemistry transport model".

P29204, line 14, "These estimates . . ." This sentence is confusing as Table 3 shows a lower value for the global source when using OMI NO2 alone. It is likely referring to results not shown trying to optimise LNOx production alone.

P29205, line 4, provide reference for 10-20% is it from Grewe et al. ?

P29205, Line 19: explain "the lightning parametrization" – either state which one or rephrase as "any lightning parametrization"

P 29205, Line 21: GC to ID flashes equals 10- there is more recent literature on this e.g. DeCaria, et al (2005), J. Geophys. Res., 110, D14303, 860 doi:10.1029/2004JD005556. Ott et al. (2007), J. Geophys. Res., 112, D05307, doi:10.1029/2006JD007365 Ott, et al. (2010), J. Geophys. Res., 115, D04301, doi:10.1029/2009JD011880, 2010

P29206, line 7-8, "errors in these processes . . ." – rephrase this text for clarity and provide references.

P29206 line 10 remove or rephrase "etc". This is a key point so it would be helpful to add an example to reinforce the text.

P29206, line 15, it is not obvious that the 4-D var method goes hand in hand with an adjoint approach rather than a forward running model being re-ran. Can this text be explained in more detail, as again it is a key point of the methodology?

P29206, line 18, define "CTM".

P29206, line 21, when discussing the 35 chemical species, it would be useful to relate those to the species that are directly measured: O3, NO2, NHO3 and CO.

P29206, line 23, "several "? 35 species are referred to in the line above?

P29206, line 29 "the while year 2007"? P29207, line 14, Define all terms in the equation

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here and provide reference/s. Relate this equation more clearly to the sub-sections that follow describing different satellite observations- or move this equation and text to 3.1.2 where this information is used and re-name the section?

P29208, line 8, briefly explain here what is meant by "the super observation approach". It is not clear how all the observations are considered together. Are all the datasets re-gridded onto a 2.5 by 2.5 degree grid or is it only for NO2?

P29208, line 12, rephrase "OMI scale" for clarity.

P 29208, line 24, rephrase "halfway the cloud"

P29209, line 20, are there any issues with MOPITT being on a different satellite to the other 3 instruments on AURA? The MOPITT CO contribution is not shown in Figure 8. Is this because the differences that feed through to the LNOx source from the CO corrections are too small?

P29212, line 11, H is the observation operator. In section 2.1 y was defined as the observation operator, please clarify.

P29212, line 18, change to "observations".

P29213, line 2, explain what the term "covariance localization" means.

P29214, line 6, What is the tuning factor and what is it based on? How does this scaling factor affect the LNOx error?

P 29216. Line 2, "super observation"

P29216, line 22, "provides".

P29218, line 7, it would be useful to show this figure.

P29218, line 8, in fig 5 right hand panels it is difficult to see any coherent differences over Africa, can the description be more precise and include the sign of change.

P29218, line 10, it isn't clear which are the model results "with and without assimila-

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tion".

P29219, line 13, add where at "240 hpa" (since this could be in the stratosphere at mid-latitudes).

P29222, line 5, It would be helpful to split Table 4 into different regions to accompany the text in this paragraph. It would also be helpful to remind the reader that the assimilation process influences the O3 distribution through the assimilated O3 as well as LNOx. It would be useful to comment if the improved O3 is wholly due to the assimilation of O3.

P29222, line 8-12, although the O3 bias in the upper troposphere is improved there seems to be a greater bias in the lower troposphere?

P 29222, line13 define TOC. This table caption discusses "global" but the text here discusses "in the tropics".

P29223, line 15, it would be useful to note that the LNOx parametrization is not based on cloud fraction but cloud top height although clearly in the GCM cloud top height must be related to cloud existence. It would also be helpful to remind the reader that this region encompasses the maritime continent where significant lightning activity occurs.

P 29224, line 15, it is hard to see the improvements discussed in Fig 11 from assimilation of TES and MLS O3.

P 29227, line 17, the text discusses an increase using SSTs for 1997, but Table 5 shows a decrease for year 1997 compared to the control. This experiment with SSTs for 1997 will have a number of differences besides cloud location and so should be interpreted with caution.

P 22928, line 18, change to "are" used. State what the chi squared test results given are actually measuring.

P22929, line 5, the text discussing LNOx a priori errors and a priori source estimates

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could be made clearer so the reader knows which rows in Table 5 to look at.

P22929, line 7, correct to "a priori"

P22929, line 19, explain "to some extent" more precisely, the value for GL for July is 10%.

P22931, line 12, add appropriate reference for 7% underestimation – Murray et al. 2012?

P29233, line 7, "overestimated by 1km in the tropics"- did Ott et al (2010) find any difference in the tropics?

P29234, line7, change to "most active".

P29235, line 6, other papers discussed earlier in the paper provide estimates of global LNOx constrained from satellite- Boersma et al. 2005, Bierle et al. 2006, Martin et al. 2007, Lin et al. 2012. It would be useful to add these ranges here.

Table 2: remove "are shown in brackets".

Fig 3. Why does the panel for TES O3 in Fig 3 (difference with and without lightning) show a large difference in northern polar latitudes?

Fig 5. "analysed sources" add "of LNOx".

Fig 6. The black and red lines and numerical values need to be explained.

Fig 11. Some of the caption is rather unclear. Explain what "inventories" mean. Are these the datasets used in the CTM? Re-phrase more clearly and give references. Rephrase "the data assimilation" to "the CTM simulation using data assimilation" or such like.

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