

Author comments in reply to the anonymous referee on “Global lightning NO_x production estimated by an assimilation of multiple satellite datasets” by K. Miyazaki et al.

We want to thank the referee for the helpful comments and suggestions. We have revised the manuscript according to the comments, and hope that the revised version of the manuscript is now suitable for publication. Below are the referee comments in italics with our replies in normal font.

Reply to Referee #2

1) Apparently, this study builds up on Miyasaki et al. (2012a). It is not clear whether you use the same data assimilation experiment than in Miyasaki et al. (2012a) or whether you had done some new developments compared to Miyasaki et al. (2012a) or performed a new data assimilation experiment. I think this has to be clarified in the Introduction section.

The following sentence has been added to the Introduction section.

“Compared to the system described in Miyasaki et al. (2012a), several updates have been applied to the data assimilation settings on the a priori emissions and the assimilated measurements.”

2) The goals of the sections 5.2 and 5.3 are not very clear. Please clarify. In addition, in Results, you can maybe first present the validation of the data assimilation and after present the LNO_x source estimation.

The following sentences have been added to describe the purpose of these sections:

“Lightning strongly influences the O₃ production and chemistry, especially in the tropical troposphere, as discussed in Sect. 3.2.2 and suggested by Sauvage et al. (2007a). Lightning activity and surface NO_x sources differ considerably among the tropical regions, reflecting variations in the meteorological conditions including cumulus convection activity. This section demonstrates the ability of CHASER-DAS to analyse the LNO_x sources and O₃ distributions in several tropical regions.”

We believe the structure of Section 5, first showing the general performance of data assimilation and then demonstrating detailed analysis results, is reasonable.

3) Section 5.1: Why do you perform the validation at only 4 Shadoz websites ? I think it will be more rigorous to have a comparison for all the sites otherwise one can think that you chose to show the sites for which it works well. You could show some of the comparisons and present the results of all

the comparisons in term of bias, correlation and rms for the LT and UT in a table. You can also refer to the extensive validation of the CHASER-DAS system presented in Miyasaki et al. (2012a), if this is relevant (see my question 1).

The following sentence has been added:

“Ozonesonde observations from 39 locations (9 locations in the tropics) have been used to validate the global ozone profiles (see Sect. 6.3). In the tropics, the data assimilation reduces the mean ozone concentration bias: by 11 % in the lower troposphere (750-450 hPa), by 63 % in the middle troposphere (450-200 hPa), and by 79 % in the upper troposphere (200-90 hPa) in January. Similar improvements were reported before by Miyazaki et al. (2012a).”

4) *Section 3.2.1: The parameterization of Price and Rind (1992) should be only applied to convective clouds. I wonder whether you apply it to every cloud. Indeed, LNO_x over oceans in figure 7 is maximum in the lower troposphere below 900hPa. This seems unrealistic. Please clarify this point.*

The parameterization was applied to convective clouds only. This is clearly described in the revised manuscript as follow:

“The global distribution of the flash rate is calculated in CHASER for convective clouds on the basis of the observed relation between the lightning activity and the cloud top height (Price and Rind, 1992) in the AGCM at each forecast step.”

The AGCM tended to produce low convective clouds over the oceans, and thus lower tropospheric source maxima are produced. Even when high convective clouds are simulated, lower tropospheric LNO_x source maxima could be present because of the averages of individual LNO_x profiles with different cloud top height (i.e., the lower maxima are always present in the lower troposphere but the upper maxima occur at various altitudes). The following explanation is provided in the manuscript:

“Over the oceans, persistent strong sources associated with the simulated low clouds and the occurrence of IC flashes are predicted in the lower troposphere. Data assimilation further increases the lower tropospheric sources by a factor of up to two.”

The following sentence has been added in Sections 4.3 and 6.1.1 to discuss the reality of the analyzed LNO_x sources over the oceans:

“We note that errors in the OMI tropospheric NO₂ column retrievals could cause large uncertainties in the analyzed LNO_x sources over the oceans, as will be discussed in Section 6.1.1.”

“It is emphasized that low NO₂ concentrations over the oceans are mostly smaller than the OMI noise

level. Errors related to the separation of stratospheric and tropospheric NO₂ could also cause errors in the OMI tropospheric NO₂ column retrievals (Lamsal et al., 2010; Boersma et al., 2011). These may cause large uncertainties in the analyzed LNO_x sources, especially over the oceans.”

5) *In Pickering et al. (1998), 3 vertical profiles of LNO_x are provided depending on the environment (land/ocean, tropical/midlatitudes). It is not clear if you used these 3 profiles or only one of them. Can you be more precise on this point?*

The following sentences have been added:

“The three profiles provided by Pickering et al. (1998) is averaged and applied in the parameterization.”

6) *Section 4.4 : I do not understand your explanation for the negative analysis increment in the upper tropospheric LNO_x obtained from the assimilation of TES (figure 8) due to the negative bias of TES in the UT. I thought TES had a general small positive bias in the upper troposphere according to Worden et al. (2006) and Nassar et al. (2008). In this last paper, the only systematic negative bias occur in southern subtropics. In figure 8, the negative analysis increment due to TES is for the southern tropics and also for the northern midlatitudes.*

Although the reason for the negative increment is not very clear from the analysis, the positive bias in the simulated O₃ concentrations in the UTLS region (Fig. 11 in Miyazaki et al. (2012a)) could be partly responsible for the negative increment. The sentence has been rewritten as:

“The negative analysis increments in the upper tropospheric LNO_x sources obtained from the assimilation of TES O₃ data likely arises from the TES negative bias (up to 20 %) from the upper troposphere to the lower stratosphere in the southern subtropics, see e.g. Nassar et al. (2008), whereas those in the northern mid-latitudes may be associated with the positive bias in the simulated O₃ (Miyazaki et al., 2012a).”

7) *section 6.1.4 : Could you explain the latest step in the calculation of the total error (p 29230 l 12-18) ?*

The explanation has been expanded.

8) *When speaking about lightning activity over the ocean in section 6.2.1 you can refer to Boccippio, Dennis J., 2002: Lightning Scaling Relations Revisited. J. Atmos. Sci., 59, 1086–1104. It is shown in this paper that the lightning parameterization of Price and Rind (1992) over the oceans is not*

consistent with observations.

The following sentence has been added:

“Boccippio (2002) also pointed out inconsistencies between the scheme of Price and Rind (1992) and satellite observations over the ocean.”

Minor comments:

Page 29206 line 10: ‘etc’ to be removed

Removed.

Page 29207 line 14: could you put the expression of the observation operator in section 3.1.2 ? Please also clarify the explanation of the expression. In particular, please better define the operators S and A and explain the utility of H.

The sentences have been written as:

“The observation operator (H) is constructed on the basis of the spatial interpolation operator (S), the a priori profile (x_a) and the averaging kernel (A), which maps the model fields (x : N- (the system dimension) dimensional state vector) into observation space (y: p- (the number of observation) dimensional observational vector) while taking into account the vertical averaging implicit in the observations as follows:”

The following sentences have been added:

“The spatial interpolation operator (S) is first applied to the model fields x in order to interpolate to the horizontal location of each observation and the height of each of the vertical layers. The averaging kernel (A) is then applied to define the sensitivity of the satellite retrieved state to changes to the true state. For weak absorbers, the a priori profile (x_a) does not, or only weakly, influence the relative model-observation difference (Eskes and Boersma, 2003). The averaging kernel (A) and the a priori profile (x_a) information provided for each retrieval is used in the data assimilation.”

Page 29212 line 14: the ensemble mean analysis is then Page 29213, line 13: typo

Corrected.

Page 29221 l 20-24, could you put the influence of the length of the assimilation cycle in the discussion of the errors in section 6.1.3?

The following sentence has been added to Section 6.1.3:

“The choice of the length of the data assimilation cycle could also influence the data assimilation result associated with distinct diurnal variations in tropospheric chemistry.”

Page 29224, line 4: 153 S -> 15S Page 29229, line 2: typo

Corrected.