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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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In this study, multiple satellite datasets are assimilated into a chemical transport model to constrain the source of NOx by lightning. The authors find that the analysed ozone field agrees better with independent ozone observations than the original CTM field, giving confidence in the performance of the data assimilation. The lightning NOx source estimate is discussed in term of total amount, regional and vertical distribution as well as seasonal variation. Several sensitivity studies are performed to provide an error estimate on the a posteriori source. The relative contribution of the assimilation of the individual satellite data set is also examined. I think this study is serious and innovative and in general well written. I recommend publication after some points detailed below are clarified.

General comments:

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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.

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 LNOX source estimation.

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).

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, LNOx over oceans in figure 7 is maximum in the lower troposphere below 900hPa. This seems unrealistic. Please clarify this point.

5) In Pickering et al. (1998), 3 vertical profiles of LNOx 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?

6) Section 4.4 : I do not understand your explanation for the negative analysis increment in the upper tropospheric LNOx 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 subtrop-

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13, C10544–C10546, 2013

ics. In figure 8, the negative analysis increment due to TES is for the southern tropics and also for the northern midlatitudes.

7) section 6.1.4 : Could you explain the latest step in the calculation of the total error (p 29230 l l2-18) ?

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.

Minor comments:

Page 29206 line 10: 'etc' to be 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.

Page 29212 line 14: the ensemble mean analysis is then

Page 29213, line 13: typo

Page29221 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?

Page 29224, line 4: 153 S -> 15S

Page 29229, line 2: typo

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 29203, 2013.