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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The global lightning-induced nitrogen oxides source" *by* U. Schumann and H. Huntrieser

Anonymous Referee #2

Received and published: 9 April 2007

This review paper provides very comprehensive coverage of the topic of production of nitrogen oxides by lightning. The authors must have spent considerable time in gathering the references, reviewing them and preparing this manuscript. They are to be congratulated on this effort!

The authors have referenced nearly all of the pertinent literature on the subject. I have suggested only a very few additional items that should be referenced. The manuscript is also very well organized with introductory sections concerning the importance of LNOx for upper tropospheric chemistry and the physics and measurement of lightning. These are followed by sections dealing with measurements of LNOx, global parameterization of LNOx distributions, and the climatic effects of LNOx. These sections are followed by a comprehensive review of the methods that have been used to constrain

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the NOx production per flash and the global LNOx source. The last review of this topic was in 1995, and much work has been done since then. An updated review has been overdue. I recommend that this manuscript be published after the relatively minor items below are addressed. Detailed changes are suggested below.

p. 2626, line 3: add DeCaria et al., 2005

p. 2628, lines 8-9: include organic nitrates in this list, but make it clear that PAN and many organic nitrates are not taken up in precipitation and deposited.

p. 2629, lines 6-7: reword to "the reaction of NO2 with O3 to form NO3, the oxidation of NO2 by NO3 to form N2O5, and the subsequent hydrolysis of N2O5 on aerosols contributesĚ.

p. 2631, lines 5-6: wet removal lifetime of HNO3 of 40 days seems too long compared with other estimates

p. 2636, line 13: change VHF to VLF

p. 2636, line 27: change momentary to rapid

p. 2642, lines 10-17: Mention that the Boccippio et al. (2001) results suggest no dependence on latitude. Also mention the conclusion of Boccippio et al. that the IC/CG ratio is more determined by storm strength and morphology than environmental variables. Check the reference for his exact wording of this.

p. 2644, top of page: This discussion implies that NO is only produced in the return stroke. This is unlikely to be the case. Please clarify.

p. 2645, line 17: Add Stith et al. (1999) and Huntrieser et al. (2002) here.

p. 2647, line 6: Add Cooper et al. (2006) to the references cited here.

p. 2658, lines 17-19: I don't understand this sentence.

p. 2659, lines 8-12: It needs to be mentioned here that it is important that the LNOx

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emissions be at the same times and locations as convective transport of ozone precursors and HOx precursors. Otherwise, the chemistry will not be correct.

p. 2661, line 24: Remove “therefore”

p. 2667 - Section 2.7.4: This section needs to be expanded to be comparable in detail to the section on global modeling of LNOx. How are flashes simulated in models with explicit electrification? How are flashes specified and placed in cloud-scale models without electrification?

p. 2668, lines 3-4: some rewording suggested here: Ĕ.stronger positive radiative forcing which may intensify the warming and thus produce more thunderstorms.

p. 2668, line 8: CO provides negligible radiative forcing. It should be removed from this sentence.

p. 2670, lines 24-25: I don’t understand how an increase of LNOx of 15% implies a much larger increase in lightning flashes. Please clarify.

p. 2671, line 9: Change “know” to “known”

p. 2673, line 24: Change “by” to “to about”

p. 2676, lines 9-12: The peak currents mentioned here are higher than what is now measured by the NLDN after upgrades to the system. The values in Orville et al. (2002) are smaller and are post-upgrade. It should also be mentioned that peak current measurement by ground-based networks are very much network dependent. The magnitude of the mean peak current depends on the spacing of the stations in the network.

p. 2677, line 1: 29.5 km

p. 2678, line 20: Add DeCaria et al. (2005) and Ott et al. (2007).

p. 2682. lines 11-14: I don’t think Martin et al. (2006) and Hudman et al. (2007) used

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1000 moles/flash for the midlatitudes. I think they used 500 moles/flash over North America.

p. 2686, lines 26-28: Mention that with coarser resolution ozone is overpredicted due to artificial dilution of NOx.

p. 2687, line 16: change “gotten” to “became”

p. 2687, lines 19-20: Acetone is now thought to be not as important as a HOx precursor as it once was. It is now thought to have a longer lifetime in the upper troposphere as a result of more recent absorption cross section data from Blitz et al. (2004???).

p. 2688, lines 6-7: add Allen et al. (2000)

p. 2690, line 11: Ĕsource larger than 10 Tg Ĕ

p. 2698, line 25: symmetrically

p. 2703, line 2: well instead of good

p. 2708, line 2: optical instead of tropical

p. 2758 - Table 3: for STEP, add Pickering et al. (1993)

p. 2759 - Table 3: add PEM Tropics-B - Pickering et al. (2001) for INTEX-A/ICARTT/ITOP - add Cooper et al. (2006) and Bertram et al. (2007).

p. 2767, Table 10: STERAO - altitudes of 4-7 km do not seem correct. Should be higher altitudes.

p. 2778, Table 19: CRYSTAL-FACE should be 16 and 29 July 2002.

p. 2782, Table 21: DeCaria et al. (2000) should be 14-28 x 10**25 molecules

p. 2783, Table 21: Ott et al. (2007) should be 21.7 x 10**25 molecules

p. 2787, Table 24: Add Allen et al. (2000)

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p. 2799, Fig. 9: there should now be a plot available of the full 11 years of OTD/LIS climatological lightning flash data from NASA Marshall.

p. 2805, last sentence of caption for Fig. 15: remove “in the top panel”

p. 2811, Fig. 21 caption: MOZART has a CTH parameterization listed in the caption. Please add the type of parameterization for ECHAM5/MESSy and TM4. I. Emmons should be L. Emmons.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 2623, 2007.