

Interactive comment on “Estimates of lightning NO_x production from GOME satellite observations” by K. F. Boersma et al.

Anonymous Referee #2

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General Comments: This manuscript contains a comprehensive treatment of the pattern of GOME tropospheric NO₂ due to lightning, its relationship with cloud-top height, evaluation of model lightning parameterizations, and the errors associated with each of these aspects. In general, the methods used are sound. However, I have a number of comments below concerning the details. The paper addresses a major uncertainty in chemical transport models. Therefore, it is worthy of publication after modification.

Specific Comments: Abstract: How representative is 1997 in terms of number of flashes? This was an El Niño year. Was the lightning distribution affected as a result? The OTD data should have something to say about this.

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- p. 3050, lines 24-25: How does focusing on areas downwind of storm systems over areas relatively free of pollution avoid the difficulty mentioned in (3)?
- p. 3053, lines 4-5: Aircraft measurements and cloud-resolved modeling have shown.....
- p. 3053, line 12: substitute the word "larger" for the word "higher"
- p. 3053, lines 16-17: 750 hPa is not a high cloud
- p. 3053, line 27: How does one get negative values of NO₂ column amount? What is the physical significance of such values?
- p. 3055, lines 6-7: There is a significant source of NO_x from biomass burning in the 5 N to 40 S region. I don't think this can be called "minor".
- p.3055, line 9: Need to explain what "ghost column" means.
- p. 3055, lines 18-19: "The annual mean tropospheric NO₂ column...." Need to explain that these are annual means of tropospheric column NO₂ for convective events, not a full annual mean.
- p. 3055, line 26: How was background defined? It seems to me that it should vary due to a variety of reasons (e.g., the amount of biomass burning, convective transport, contribution of NO_x from the stratosphere, etc.) Could the background for a particular grid cell and time be derived from the TM3 model?
- p. 3056, lines 23-25: How was the exponent value of 4.9 chosen? The range of possible values is +/- 2. Why weren't the values of 4.6 (ocean) and 5.1 (continents) maintained?
- p. 3057, lines 20-22: Please explain this statement about relative sizes. What type of error does the large footprint of GOME cause when dealing with convective storms that are much smaller?
- p. 3059, lines 7-8: It needs to be mentioned that this assumption of CG strokes being

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10 times more energetic than IC strokes (and therefore 10 times more productive of NO) is very uncertain. A number of recent analyses (Gallardo and Cooray, 1996, Tellus; DeCaria et al., 2000, JGR; Fehr et al., 2004, JGR) are now pointing toward CG and IC flashes being nearly equivalent in NO production on average.

p. 3059, line 18: I think that (2) should read, "between T = -15C and the top of the boundary layer". Is this correct?

p. 3059, line 22 to p. 3060, line 3: It appears that the H5 scheme is a modification of the Price and Rind (1992) scheme, which should be again referenced here. Also reference Boccippio (2002, JAS) for justification for modifying the marine relationship.

p. 3060, lines 9-10: The convective parameterization in the ECMWF model DOES NOT necessarily place convection at the times and locations of actual convection. No current convective parameterization is capable of doing this.

p. 3061, line 4: Mention that there is also some contribution of NO_x from the stratosphere.

p. 3061, line 27: which TM3 lightning parameterization is being shown in Fig. 6 and discussed here?

p. 3066, line 25 to p. 3067, line 2: Note that it is not surprising that the CP scheme overestimated LNO₂ over the ocean. The CP scheme being used was developed using primarily continental data. Also, reference Petersen and Rutledge (1998, JGR) concerning differences in the relationship between flash rate and convective precipitation between continents and ocean.

p. 3067, line 4: change "found" to "suggested"

p. 3067, line 5: also reference DeCaria et al. (2000), Fehr et al. (2004)

p. 3067, lines 7-8: DeCaria et al. (2000) and Fehr et al. (2004) suggest that both the NO production per CG flash and per IC flash are most likely are between those

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suggested by Price et al. (1997) for CG and IC flashes.

p. 3067, lines 9-16: This is not a viable option since the OTD climatology indicates that the ratio is ~ 10 .

p. 3067, line 26: It is not true for thunderstorm anvils that the NO_x from lightning will reside above the cloud. Aircraft data from field projects such as STERAO-A (Dye et al., 2000, JGR: DeCaria et al., 2000, JGR) and CRYSTAL-FACE (Ridley et al., 2004, JGR) show that the maximum NO_x is within the anvil cloud and that NO_x is typically not enhanced above the anvil.

p. 3068, lines 19-21: The OTD/LIS climatology over tropical South America has much more lightning in Dec-Jan-Feb than in Jun-Jul-Aug.

p. 3069, lines 17 and 22: In line 17 it should be noted that it is not a full mean LNO₂ distribution shown in Fig. 10. It is really only for clear-sky situations.

p. 3070-3072: How representative is 1997 for lightning? You should be able to use OTD data to determine this.

p. 3076, line 10-11: Why is this so?

p. 3080, line 10: This statement assumes that updrafts are directly proportional to lightning NO_x production and that updrafts are weaker over the ocean. This is very uncertain. Please change the word "updrafts" to "storms".

p. 3080, lines 23-25: This change is not appropriate based on the OTD climatology.

p. 3081, lines 22-24: I enthusiastically endorse further investigation of the effects of reduced CG:IC energy ratios. However, use of increased convective intensity (lightning frequency) ratios between continents and oceans is not appropriate. This statement should be removed from the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 3047, 2005.