

***Interactive comment on* “Estimating the NO_x produced by lightning from GOME and NLDN data: a case study in the Gulf of Mexico” by S. Beirle et al.**

S. Beirle et al.

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We want to thank the referee for his constructive review and the several helpful remarks. In the following, we refer to them point by point.

Reviewer comment 1) The final result for NO_x production per flash (77 moles/flash) and for global production (1.5 Tg N/yr) are near the lower end of estimates in the literature. Other lightning NO_x analyses using GOME data by Beirle et al. (2004) and Boersma et al. (2005) have also yielded relatively low values of global production (2.7 and 3.5 Tg N). The authors of this manuscript should address the question of why all of the GOME-derived estimates are lower than the estimates derived by other methods.

Reply: The mentioned studies using GOME data for the estimation of LNO_x, though following different approaches, all result in comparably low values. However, uncertainties remain quite high, and none of the cited studies could exclude the often used number of 5 Tg N per year. One particular reason for the extreme low number of 1.5 (now 1.7) Tg resulting from our current estimation is the global lightning rate taken from Christian et al. (2003) that is significantly lower than previous estimates, i.e. the lightning frequency from Mackeras et al. (1998) that has been used in Beirle et al. (2004). In any case, the extrapolation of global flash rates from a single observation is arguable, and more studies are needed to yield representative means. We add a short discussion on this aspect in the conclusion.

Reviewer comment 2) p. 11297, line 8: change "about some" to "several"

Reply: To avoid two times "several" in the sentence, we changed it to "of the order of a few days"

Reviewer comment 3) p. 11297, line 21: reword to the following: Further complications arise from differences in the lightning frequency used in the calculations and possibly from differences in NO_x production for cloud-to-ground (CG) and intracloud (IC) flashes.

Reply: The respective sentence will be reworded and amended for clarification.

Reviewer comment 4) p. 11297, line 26: Add a couple of sentences referencing Huntrieser et al. (2002, JGR). She made two estimates of global production based on EULINOX data (3 and 4 Tg N/yr).

Reply: Will be amended accordingly.

Reviewer comment 5) p. 11299, line 21: ...studies on particular lightning events using GOME data have also....

Reply: Will be changed accordingly.

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Reviewer comment 6) p. 11300, line 16:independent of longitude.... I assume a dependence on latitude is included. If so, this should be mentioned.

Reply: The description of the reference sector method is extended for clarification.

Reviewer comment 7) p. 11303, lines 15-17: constant e-folding lifetime - how long a lifetime for NO_x is used in the model? I think a vertically varying NO/NO₂ ratio would be better. Please comment in the manuscript on these issues.

Reply: Paragraph 2.4. is meant to describe the general features of the transport modelling with FLEXPART. For the different studies, different runs with different lifetimes are performed. The assumed lifetimes are given in the respective sections (see 4.3.1, 4.3.2 and 4.3.3). For clarification, we add a sentence in paragraph 2.4 about the chosen lifetime being dependent on the considered scenario.

Reviewer comment 8) p. 11306, line 26: Table 2 is referenced here. I cannot find it in the paper. Was there a Table 1? The authors need to provide some details on how the Profiles of Pickering et al. (1998) were employed. The profiles are of the fraction of the total lightning NO_x that is injected into each 1-km deep layer within a storm. How much actual mass of LNO_x was assumed in this case? How was it partitioned between NO and NO₂?

Reply: The citation of table 2 is meant to refer to the table 2 presented in Pickering et al. (1998). This is clarified in the manuscript. The vertically dependent partitioning between NO and NO₂ will be considered in the revised manuscript (see also the reply to comment 2 of referee 1). For the calculation of the total AMF from the box-AMFs and (relative) NO₂ profiles, no assumptions on the total mass of NO_x is needed, since the AMF simply describes the ratio of slant and vertical column densities.

Reviewer comment 9) p. 11306, line 28: I don't think "retrieved" is the correct word here. "Computed" would be better since the profiles come from model calculations.

Reply: The respective sentence will be revised and a short description of the profiles

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from Pickering et al. (1998) will be added.

Reviewer comment 10) p. 11307, line 3: change "measurements" to "model calculations".

Reply: Will be changed accordingly.

Reviewer comment 11) p. 11308, lines 23-26: I would assume that the primary reason for the underestimate is that lightning NO_x is not included in this run of the FLEXPART model.

Reply: The discussion of lines 23-26 was meant to refer to the discrepancies of Fig. 1b and Fig. 5b showing up north from the convective system, even north from 30°. This is clarified in the manuscript.

Reviewer comment 12) p. 11309, line 4: southwards from the coast

Reply: Changed accordingly.

Reviewer comment 13) p. 11309, line 28: ...the fraction of the total LNO_x that is aged LNO_x....

Reply: Changed accordingly.

Reviewer comment 14) p. 11310, lines 15-16: 11% of the LNO_x was aged. You can't assume that the aged LNO_x is 11% of the detected NO_x. It is really 11% of 90% of the detected NO_x, since 10% of the detected NO_x is anthropogenic. So, the aged LNO_x contribution is 10%, not 11%, and the total of anthropogenic and aged LNO_x is 20%, not 21%. Fresh LNO_x contributes 80% instead of 79%. Minor difference, but let's get the logic correct.

Reply: We thank the reviewer for raising this inconsistency. The calculation will be revised and the combination of percentages referring to different quantities (total NO_x / LNO_x) will be done carefully.

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Reviewer comment 15) p. 11310, lines 17-18: Scaling needs to be corrected. It is $100/80 \times 80 = 100$. So no scaling is actually necessary. The scaling factor is now 1 rather than 0.99.

Reply: See 14.

Reviewer comment 16) p. 11312, line 15: Table 4 is referenced here. I cannot find this table in the manuscript. Tables 2 and 4 have been mentioned thus far in the paper. Was there meant to also be Tables 1 and 3?

Reply: See comment 8. The citation of table 4 is clarified to refer to Ridley et al. (1996).

Reviewer comment 17) p. 11313, line 9: I don't think transport should have been neglected for the southern system. This system was older (had been producing LNO_x for a longer period of time prior to the GOME overpass) and there is more of a chance that some portion of the LNO_x had been transported out of the GOME pixels than is the case with the northern system.

Reply: The discussion of the southern part is meant to check for consistency rather than to use it for a quantitative estimate as detailed as from the northern part. While there is probably outflow of LNO_x from the southern part, at the same time there is inflow of LNO_x produced in the northern part. Due to the difficulties of quantifying in particular the former, we neglect these transport effects, that partly balance out each other, for our rough estimation. The respective paragraph is revised for clarification.

Reviewer comment 18) p. 11314, lines 5-6: It is not clear why there would be a shift toward NO with more anthropogenic NO_x. NO and NO₂ come into equilibrium very quickly after emission of NO from anthropogenic sources.

Reply: What was meant is that (the same amount of) anthropogenic NO_x is shifted towards NO when lifted higher up in the anvil.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 11295, 2005.