Atmos. Chem. Phys. Discuss., 5, S2153–S2160, 2005
www.atmos-chem-phys.org/acpd/5/S2153/
European Geosciences Union
© 2005 Author(s). This work is licensed
under a Creative Commons License.

Atmospheric Chemistry and Physics Discussions

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

K. F. Boersma et al.

Received and published: 18 August 2005

We thank referee #2 for his or her critical remarks that have been helpful in improving the manuscript.

SPECIFIC COMMENTS

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

The final result of out paper is the estimate of the global LNOx production and this holds for the year 1997. The scaling year for LNOx production in the TM3 parameterizations was 1998 where ECMWF meteorological parameters were assumed to

produce 5 Tg/N/yr. With constant scaling relations in TM3, we found for 1997 a LNOx production of ˜6 Tg/N/yr. From the OTD website we find that in 1997 and 1998 261 flashes per orbit were registered. In 1996 this number was 226. In other words, the number of flashes does not show much variability from 1996 to 1998.

"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)?"

By focusing on 10:30am -when very little lightning is actually occurring, i.e. downwind of storm systems- both model and observation will be mainly sensitive to LNOx produced over the last 24 hours. As the model simulates a complete 24 hour cycle, and the sampling time of model and observation is at 10:30am, an extrapolation of 10:30am data to a 24-hour average can be avoided.

"p. 3053, lines4-5."

This has been added.

"p. 3053, line 12."

This has been changed.

"p. 3053, lines 16-17."

This has been corrected.

"p. 3053, line 27: How does one get negative values of NO2 column amount? What is the physical significance of such values?"

To clarify, we have added the following to the manuscript: "The retrieval procedure subtracts an estimated stratospheric slant column from an observed total slant column, and hence occasionally small, negative tropospheric columns may occur, consistent with the error bars on the total and stratospheric column estimate. Both positive and negative values should be used in the analysis to avoid biases."

ACPD

5, S2153–S2160, 2005

"p. 3055, lines 6-7: There is a significant source of NOx from biomass burning in the 5N to 40S region. I don't think this can be called "minor"."

We changed "minor" to "small compared to the industrialised Northern Hemisphere".

"p. 3055, line 9: Need to explain what "ghost column" means."

This is explained right below eq.(1); i.e. the predicted column of NO2 from the surface up to the effective cloud height.

"p. 3055, lines 18-19: Need to explain that these are annual means of tropospheric columns for convective events, not a full annual mean."

This has been added.

"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 NOx from the stratosphere, etc.). Could the background for a particular grid cell and time be derived from the TM3 model?"

We've added this to the text for clarification: "The background is defined as the fixed, non-lightning integral of NO2 from the cloud top to the top of the troposphere. The background is determined from all cloudy-sky situations in 1997 with cloud tops lower than 6.5 km." In principle, one would expect a decrease of this integral (the above-cloud NO2 column) with cloud top altitude, as the column of air decreases with increasing cloud top altitude. The uncertainty in the background is discussed in the text and gives rise to a relatively large uncertainty in the exponent.

"p. 3056, lines 23-25: How was the exponent value 4.9 chosen? The range of possible values is +/- 2. Why weren't the values of 4.6 and 5.1 maintained?"

The exponent value of 4.9 was chosen to stay in line with the often-used parametrization of Price and Rind [1992] that is used in the TM3 H5-scheme. The advantage of using 4.9 for both ocean and continent is that the ratio of the pre-factors conveniently

ACPD

5, S2153–S2160, 2005

gives an estimate of the ratio of LNO2 production for clouds of equal height. Other choices -for instance with exp. values 4.6 and 5.1 and their associated pre-factors- gave ratio's very close to the 1.6 number presented here.

"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?"

Our statement is that the interpretation of GOME results is complicated due to the large pixel size. For instance, a new instrument like OMI with a 9 times improved spatial resolution, is expected to sample convective events at their typical size. Then, also events with clouds up to 15 km might be observed, whereas now such events are necessarily smeared out over the large GOME footprint. Note however that our cloud reflectance $>$ 75% requirement implies that the reported cloud height is representative for a largest part of the pixel.

"p. 3059, lines 7-8: It needs to be mentioned that this assumption of CG strokes being 10 times more energetic than IC strokes is very uncertain. A number of recent analyses are now pointing toward CG and IC flashes being nearly equivalent in NO production on average."

This has been added to the text.

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

No.

"p. 3059, line 22 to p. 3060, line 3."

Price and Rind [1992] have now been referenced in the description of the H5-scheme. Boccippio [2002] has now also been referenced in order to justify modifying the marine relationship.

ACPD

5, S2153–S2160, 2005

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

We have weakened our statement by adding "approximately".

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

This has been added.

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

The H5-scheme. This has been added in the text.

"p. 3066, line 25 to p. 3067, line 2: Note that it is not surprising that the CP scheme overestimated LNO2 over the oceans. 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 oceans."

The reference has been added.

"p. 3067, line4: change "found" to "suggested"."

This has been done.

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

This has been done.

"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 between thos suggested by Proce et al. (1997) for CG and IC flashes."

This has been added.

ACPD

5, S2153–S2160, 2005

"p. 3067, lines 9-19: This is not a viable option since the OTD climatology indicates that the ratio is ˜10."

The OTD climatology indicates that the average GLOBAL annual flash rate for oceans is approximately 10x smaller than for continents. I.e. at any given second there are ˜50 flashes over all land vs. ˜5 flashes over the all of the oceans (Christian et al., 2003). This continent:ocean flash rate ratio has not been corrected for the fact that the Earth's surface is 70% ocean and 30% continent. The option discussed in the text on the other hand is changing the convective intensity ratio. This convective intensity ratio is per definition not dependent on surface area, it is simply a scaling number used in the two parameterizations to correct for assumed weaker storms/updrafts over oceans than over continents. Further investigation into this ratio should therefore not be ruled out, although we agree that reducing CG:IC energy ratios is probably more fruitful, given the support from many sides in literature that a 10:1 CG:IC ratio is too high.

"p. 3067, line 26: It is not true for thunderstorm anvils that the NOx from lightning will reside above the cloud. Aircraft data from field projects such as STERAO-A and CRYSTAL_FACE show that the maximum NOx is within the anvil cloud and that NOx is typically not enhanced above the anvil."

We agree with the reviewer that the maximum NOx is within the anvil cloud. However, most cloudy situations encountered by GOME are NOT thunderstorm anvil clouds. Indeed, these cloudy pixels generally have clouds too low to generate LNOx. In other words, the NOx above the cloud decks will predominantly be yesterday's LNOx (if any). To clarify this, we updated sentence as follows: "The NOx originating from mostly yesterday's lightning will largely reside above (today's) cloud cover, where GOME has a large sensitivity compared to clear-sky."

"p. 3068, lines 19-21: The OTD/LIS climatology over tropical South America has much more lightning in DJF than in JJA."

We agree. Our statement however is that there is much less lightning THAN MOD-

ACPD

5, S2153–S2160, 2005

ELLED in DJF and SON compared to MAM and JJA. We do not say anything about lightning over South America in absolute terms.

"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."

This has been added.

"p. 3070-3072, lines 17 and 22: How representative is 1997 for lightning? You should be able to use OTD data to determine this."

This issue has already been adressed in the first 'specific comment'.

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

We have added two sentences so that the text now is: "The relation between LNOx production and the NO2 concentration in the atmosphere is in reality non-linear. This is consequence of the photochemical production of ozone via NOx, leading to OH-formation. This radical may -depending on chemical and meteorological conditions- constitute an important sink for NOx."

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

We have done this.

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

We feel that this issue is adressed in our response to ref.#2's comment under p. 3067, lines 9-19.

"p. 3081, lines 22-24: I enthusiastically endorse further investigation of the effects of reduced CG:IC energy ratios. However, use of increase convective intensity (lightning freqency) ratios between continents and oceans is not appropriate. This statement

ACPD

5, S2153–S2160, 2005

should be removed from the paper."

We thank ref.#2 for his enthusiasm wrt reducing CG:IC ratios and we hope we can live up to it in the near future. On the other hand, based on our current knowledge, we cannot justify removing the statement on increased convective intensity ratios. The OTD climatology global annual flash ratio is just not the same as the convective intensity ratio used in the parameterizations. We feel that a future experiment should aim at simultaneously tuning both the CG:IC and convective intensity ratios and find the best match with observed patterns of LNO2.

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