

Interactive comment on “Intercontinental transport of nitrogen oxide pollution plumes” by M. Wenig et al.

Anonymous Referee #2

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This comment focuses on the treatment of lightning NO_x production that was used in the FLEXPART model as part of the analyses contained in this manuscript. It also raises the issue of the NO₂ retrieval technique relative to others in the literature.

It appears that the lightning NO_x treatment in the FLEXPART model is not correct. The model uses the LIS lightning data to obtain flash rates. However, LIS only sees a given point on the earth for a few minutes during its overpass. Therefore, the data represent only a very small sample of the total lightning in a given storm. The LIS data can only be used in a climatological sense. I suspect that the effect of lightning on middle and upper tropospheric NO_x has been substantially underestimated. The lightning flash rates could instead be estimated using various parameters from the convective parameterization within the ECMWF assimilation system, such as upward cloud mass flux, convective precipitation rate, or cloud-top-height (e.g., Allen and Pickering, JGR,

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2002).

How does the FLEXPART model inject the lightning NO emissions in the vertical? It has been noted that most of the lightning NO is located in the middle and upper troposphere (e.g., Ridley et al, JGR, 1996; Pickering et al., JGR, 1998).

The paper states (page 2158) that most of the lightning over the ocean is intracloud. What is the justification for this statement? Is there a reference? Most parameterization schemes for the IC/CG ratio either assume (rightfully or wrongfully) that this ratio is dependent on latitude or the cold cloud depth. The production per flash used in the FLEXPART model is apparently the value from Price et al. (1997, JGR) for IC flashes. If instead there are a significant number of CG flashes over the ocean, the emission would again be too small.

On page 2154 it is stated that it is difficult to determine whether enhanced NO_x in the upper troposphere is from lightning or boundary layer pollution. Starting in the year 2000 CO measurements from MOPITT began. The manuscript might note that colocated enhancements of CO and NO₂ would indicate pollution, whereas enhanced NO₂ without CO would be indicative of lightning NO production.

Appendix A notes that this manuscript uses GOME tropospheric NO₂ retrievals from the method of Leue et al. (2001, JGR). How does this method differ from the other tropospheric NO₂ retrieval methods that have been published (Richter and Andreas, 2002) and Martin et al. (2002, JGR)? How does the Leue technique perform compared with in-situ data?

The lightning issue is an important one to treat carefully. If the considerations that I mention here are taken into account, the conclusion of the paper may be affected.

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 2151, 2002.

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