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Interactive Comment

Interactive comment on "Global lightning NO_x production estimated by an assimilation of multiple satellite datasets" *by* K. Miyazaki et al.

Anonymous Referee #3

Received and published: 2 January 2014

Overview

This study constrains the global lightning NOx source by using top-down constraints from multiple species (NO2, CO, ozone, HNO3) as observed by satellite with an ensemble Kalman filter (EnKF) approach in the CHASER-DAS system. This is the first time a simultaneous, multiple-species approach has been applied to lightning NOx, which the authors demonstrate has excellent promise in reducing uncertainty in the assimilation of this important source. In particular, I see the ability of EnKF to simultaneously correct global mean OH and the ozone production efficiency very useful for improving estimates of lightning NOx emissions from observations of ozone, CO and HNO3. I support the publication of this article, pending some suggested clarifications and improvements in the discussion of uncertainties, as outlined below.

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General Comments

1. The a posteriori lightning NOx product will reflect corrections to convolved errors in the model representation of both flash activity and NOx yields per flash. The lightning flash rate was not assimilated (satellite coverage is poor; global ground networks have low detection efficiencies). However, the flash rate parameterization was also not adjusted to match the satellite climatology from LIS/OTD, as is done for most global models. This is surprising, because the global lightning flash rate distribution is the best-known aspect of the lightning NOx source. If the authors wish to maintain discussion of the assimilated LNOx emissions in the individual context of the unconstrained flash rate (Section 6.2.1) versus NOx yields per flash (Section 6.2.2) –Âăboth of which have very large uncertainties in models – then the flash rate distribution of the model should be shown and quantitatively evaluated against the spatial and seasonal distribution from LIS/OTD. The authors seem to suggest that the lightning flash rate parameterization performs very well when unconstrained, which would be a very surprising result in the context of the literature (e.g., Tost et al., 2007), and therefore should be documented.

2. The technique used here should not be able to distinguish between co-located NOx emission sources in a grid cell (e.g., surface lightning and anthropogenic sources, free tropospheric aircraft and lightning), and assumably depends on the a priori fraction of emissions for source attribution. If this is the case, some discussion should be included as it pertains to the results presented here. E.g., if the Ott et al. (2010) vertical probability distributions for lightning emissions were used instead of Pickering et al. (1998), which had a much smaller fraction emitted in the boundary layer, then the assimilation would attribute more of its surface NOx corrections to anthropogenic sources than lightning, which would influence the total lightning NOx value. Corrections of biases in surface sources in strongly polluted but lightning-prone regions (e.g., Gulf Coast, Congo) may be erroneously ascribed to lightning. Similarly, it is unclear to me how this technique could be used to differentiate between IC and CG flash yields,

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unless they have very separate spatiotemporal signatures from one another.

Specific Comments

p29206 l25-27 - Does it not also have the potential to introduce larger errors if uncertainties are large in the additional constraint? e.g., the bias in TES UT ozone as shown in Fig. 8?

p29206 l29 - I suspect the "while" is erroneous?

p29207 l13-17 - Equation 1 would be better placed in Section 3.

p29207 l23 - remove subjective term "strong," perhaps replace with "useful"

p29208 l23-25 - There appears to be a missing word after "halfway"?

p29210 l6-10 - Version and access date should be given for the OMI/MLS product, which has changed over time.

p29211 l25 – p29212 l4 - What is meant by "based on"?

p29212 l3-4 - The authors should compare the aircraft emissions used here in the context other estimates from the literature (e.g., Wilkerson et al., 2010, http://doi.dx.org/10.5194/acp-10-6391-2010). The interpretation of the assimilated LNOx results will be sensitive to uncertainty in aircraft emissions, which should be acknowledged.

Section 3.1.2. This section could use clarification, particularly for readers not familiar with data assimilation and/or EnKF. It would be helpful to include a sentence or two that qualitatively describe how the EnKF works. Does the error covariance matrix take into account errors in the observations (e.g., those discussed in Section 6.1.1), or does EnKF blindly treat all the satellite products as truth, even in instances where we know the observations to be poor or highly uncertain? What averaging kernels are used in H(x), assumably those from each satellite product? What is the value of k?

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p29213 l22-23, p29231 l9-10 neglect to acknowledge the existence of ground-based networks with global coverage, e.g., the World Wide Lightning Location Network (WWLLN; Abarca et al., 2010, http://dx.doi.org/10.1029/2009JD013411) or Vaisala's GLD360.

p29214 l6-10 - why was a global scaling factor chosen to give 41.2 flashes s-1, rather than one to match the climatological value from satellites? Also, the more recent climatology using the combined LIS and OTD instruments (46 flashes s-1; Cecil et al., 2012, http://doi.dx.org/10.1016/j.atmosres.2012.06.028) should be referenced, instead of the old OTD-only reference.

p29214 l20-24 - z is not the IC/CG ratio as stated by the authors, but the CG proportion of total flashes. (Otherwise, setting z to zero makes no sense). Also, the coefficients for z given here are those from Price and Rind (1993, http://dx.doi.org/10.1029/93GL00226), not those in Price et al. (1997).

p29214 l26 – p29215 l1, p29232 l18-19 - The difference in yields between IC and CG flashes is still very uncertain. Comparison of what is used here with the literature should be given. Most recent work suggests the CG/IC production ratio should be closer to unity, cf. Table 19 of Schumann and Huntrieser (2007), although not all (e.g., Koshak et al., 2013; http://dx.doi.org/10.1016/j.atmosres.2012.12.015).

p29215 l1-5 - Were the Pickering et al. (1998) profiles scaled to local cloud top height, or were fixed altitudes used? Why were the Pickering et al. (1998) profiles used instead of the Ott et al. (2010) profiles?

p29216 l4 - "lighting" should be "lightning."

p29216 l5-6 - The Cooper et al. (2007) and Hudman et al. (2007) studies examined North America, not the tropical upper troposphere. Better references for comparison would be Sauvage et al. (2007, http://dx.doi.org/10.5194/acp-7-815-2007) or Murray et al. (2012), who examined the influence of lightning in the tropics.

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p29221 l3-7 - The assimilated changes in mean OH could be independently evaluated by comparison to the methyl chloroform and methane lifetimes, available from observational constraints (cf. John et al., 2012, and references therein; http://dx.doi.org/10.5194/acp-12-12021-2012). In addition to OH, I would also expect a major benefit of the multiple-species to be in its ability to constrain ozone production efficiencies (OPE, which may be approximated as PO3/PHNO3, cf. Cooper et al., 2010, http://dx.doi.org/10.1029/2010JD015056), which are non-linearly dependent on NOx, and would be important for inversely determining LNOx emissions from ozone observations.

p29222 Section 5.1 - The authors might consider showing Ascension instead of Irene, given the expected strong influence of lightning on the South Atlantic ozone maximum, the dominant mode of seasonal variability in tropical ozone (e.g., Sauvage et al., JGR, 2007, http://dx.doi.org/10.1029/2006JD008008).

p29223 l4-5 - Convection and lightning are heavily parameterized everywhere in the model. Please cut, or give an objective argument as to why tropical W Pacific is expected to have worse convection or lightning than elsewhere in the model.

p29223 l15-17 - Please clarify what is being compared in these sentences.

p29224 l26-27 - Please justify why large uncertainties in cumulus cloud and biomass burning activity are "expected" in this region

p29228 l18 - “tests is” should be “tests are”

p29228 l25-28 - Please clarify what is meant by the phrases “mean analysis spread” and “spin-up period for the assimilation” (I thought Kalman filters only require the previous state?). Also, “week” should be plural.

p29231 l16-19, p29237 l2-3 - I find this conclusion weak unless more is done to objectively evaluate the flash rate distribution in the model. It could easily be due to a systematic low bias in the a priori NOx production per flash over the ocean. Whether

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or not this is primarily due to underestimation of (1) the flash rate, or (2) NOx yields per flash over marine regions could be determined by comparison of the simulated flash rates with the LIS/OTD climatology.

p29231 l23-24 - 6.3 Tg N yr-1 using a global mean flash rate of 46 flashes s-1 from the LIS/OTD climatology corresponds to 310 mol per flash. Do you expect your 41.2 flashes s-1 for 2006 could be explained by interannual variability in the global mean lightning flash rate?

p29232 l10-14 - An extremely useful figure for the community would be a map of the average NOx yield per flash, calculated by using the assimilated LNOx emissions divided by the flash rate distribution from (1) the model parameterization, and (2) the LIS/OTD climatology. This would be helpful for informing CTMs/CCMs as how to implement differential LNOx yields per flash, which are typically done in arbitrary manner, but necessary for matching global ozone distributions. To me, this is the most useful and unique scientific contribution enabled by this work. The greatest uncertainty global models face at present in reproducing the lightning NOx source is in NOx yields per flash, since most constrain the flash rate magnitude and distribution to the LIS/OTD climatology.

p29237 l7-8 - Please rephrase to make it clear that this is because errors in simulated flash rates are small in this study. Many CTM studies find it necessary to constrain the lightning flash rates for their ozone simulations (e.g., Martin et al., 2007; Sauvage et al., 2007; Jourdain et al., 2010; Allen et al., 2010; Murray et al., 2012).

Fig. 9 - superfluous axis labels and titles could be removed to increase panel box sizes

Fig. 12 caption should clearly state which difference is taken (I assume with minus without the cloud-covered observations)?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 29203, 2013.