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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Improvement and evaluation of simulated global biogenic soil NO emissions in an AC-GCM" *by* J. Steinkamp and M. G. Lawrence

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1. Summary

This article presents a comprehensive update of the very widely-used parametrization of soil NOx emissions by Yienger and Levy (1995). The topic is important for the atmospheric chemistry modeling community, since soils represent a poorly quantified, widespread source of NOx over continents. The most important model improvements include the use of a much larger database of flux measurements distributed worldwide; new databases for land use and leaf area index (LAI); a revised estimate for the fraction of N-fertilizer application which is released as NO to the atmosphere; and the use of volumetric soil moisture to determine the dry or wet state of the soil. The impact of

each model update is evaluated separately. The emissions calculated with the final updated algorithm are generally higher than with the original algorithm. Evaluation against a top-down emission distribution based on satellite NO_2 data indicates a better agreement over tropical rain forests, although large underestimations are seen over arid and semi-arid regions.

However, as explained further below, the methodology used has two important flaws, which concern (1) the averaging method used in the derivation of emission factors and (2) the fertilizer-induced emissions in "rice-producing areas", i.e. in Southeast Asia. These flaws can and should be corrected before resubmission of the article. An important effort should be also devoted to improve the text readability. Besides the suggestions (see under Minor comments below), the article would benefit from being checked by a demanding reader. If the issues above can be satisfactorily addressed, this article could represent an important step towards a better quantification of SNOx emissions in models, and would be certainly appropriate for publication in this journal.

2. Major comments

(a) The adopted reduction of SNOx in "rice-producing areas" is very crude, as it assumes that rice is the only crop in these areas. As pointed out by the authors, it leads to unrealistic geographical patterns of SNOx over China and India. This feature was already present in the original parameterization, but it can and should be improved using available data on rice paddies. The FAO statistics include data for fertilizer use and cultivated area per country and per crop type (see http://www.fao.org/ag/agl/fertistat/), although such detailed data might not be available for every year. For example, over China in 1997, rice paddies received only 25% of the total N-fertilizer consumption; this fraction was about 50% in India. Combining the FAO data with the geographical distribution of rice paddies (see e.g. E. Matthews' dataset available at http://dss.ucar.edu/datasets/ds867.0/ but other, more recent datasets should be available), it is certainly possible to improve substantially the treatment of fertilizer application in this work. In addition, since experiments indicate that SNOx is reduced, but not

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completely suppressed due to flooding, the better agreement of the new model with Jaeglé et al. in "rice-producing areas" should be considered as fortuitous.

(b) I don't understand the need for the complicated iterative procedure used to derive the Aw and Ad factors corresponding to each measurement (p. 16020). Why don't you have time series of temperature and soil moisture? This information is available from your model, otherwise how can you calculate the modeled emission rate? As far as I understand, the monthly averaged emission at one location is proportional to Aw (which is itself proportional to Ad). Why can't you just multiply Aw by the ratio (measured flux)/(simulated flux)? Maybe I missed something here. More explanation would be helpful. Furthermore, given the large number of available measurements, wouldn't it be feasible to constrain not only Aw, but also Ad? Or at least, the fixed ratio Ad/Aw used here could be validated or optimized using the available data.

(c) Regarding the derivation of the emission factors Aw and Ad for the different land cover classes (p. 16020-16021): Although the distribution of measured fluxes is indeed approximately log-normal, it doesn't imply that the appropriate average emission rates are given by Eq. (7), i.e. by the geometric averages of the measurement-derived emission rates. Eq. (7) is appropriate in order to capture the median emission, but the true average lies higher. In the example given on p. 16013 (l. 24-26), $e^{1.3}$ = 3.67 is the geometric mean. With a standard deviation of 1.1, the mean is $e^{1.3+(1.1^2)/2}$ = 6.7, i.e. almost a factor of 2 higher. Taking the extreme case of ecosystem 14 (mixed forest, with only 3 measurements), the average emission is about an order of magnitude higher than the median. I recommend to calculate the emission factors for the different land use classes as plain arithmetic averages of the emission factors for individual measurements, also without discarding the negative values. The consequence will be of course larger emissions from all ecosystems.

(d) It is important to stress that the top-down estimates might be biased due to model errors and/or biases in the NO_2 data. For example, over many isoprene-rich areas (e.g. Amazonia, Eastern US, Pearl River Delta), [OH] appears to be underestimated

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by CTMs. Since OH levels control the sink of NOx and therefore the apparent lifetime of NO_2, top-down estimates based on NO_2 abundances should be considered as indicative. This is confirmed by the sometimes large differences between the results of different inverse modeling studies. You should in particular consider several more recent studies focusing on China: Wang et al., JGR 112, D06301, doi:10.1029/2006jd007538, 2007, Zhao and Wang, GRL 36, L06805, doi:10.1029/2008gl037123, 2009, and also Lin et al., ACP 10, 63-78, 2010. Especially Wang et al. (2007) reported considerably higher soil emissions over China compared to the YL95 inventory, likely due to an underestimation of fertilizer-associated emissions.

3. Minor comments

p. 16008, l. 6 and elsewhere in the manuscript: maybe use "compilation" or "compilation of measurements" instead of "database"

p. 16008, line 13: "Adopting the fraction of SNOx induced by fertilizer application based on our database" : unclear, re-phrase by e.g. "Adopting a fraction of 1.0$\pm$2.1% for the applied fertilizer lost as NO, based on our measurement compilation"

p. 16008, line 18: "Switching from soil water column..." : 2-weekly precipitation rates (not the soil water column) were used by YL95 to distinguish between dry and wet state.

p. 16008, line 22: "our total SNOx source ends up being close to one of the top-down approaches": this doesn't tell much, please be more specific. Please also provide an error estimate on the global SNOx. Same remarks hold for the following sentence on the geographical variations.

p. 16008, line 25 "This suggests that a combination (...) could be combined..." : poor sentence. More importantly, explain how the two approaches can be combined. In fact, the top-down approach already combines atmospheric observations with prior information obtained from bottom-up inventories.

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p. 16009, line 22 "...SNOx seems to mostly be underestimated..." : rephrase (e.g. "..SNOx seems to be generally underestimated...").

p. 16010, line 3: You could begin this section by explaining why the EMAC model is needed, as this might not be immediately clear to the reader.

p. 16010, lines 21-26: Explain 'FIE' and 'SL10'

p. 16011, l. 14: "If there has been no precipitation in a grid cell during the last 14 days". Does "no precipitation" mean really zero precipitation, or a value below some threshold?

p. 16011, l. 19-21: confusing. Since d is the pulse duration, replace e.g. "1<d<3" by "d=3"

p. 16012, l. 1-2: define "vegetation layer".

p. 16012, l. 4: why "For comparison with Jaeglé et al."? The CRF was first introduced by Yienger and Levy.

p. 16013, l. 11-12: how are these climate classes defined?

p. 16013, l. 20: rewrite as "spans from -6.89 to 547 ng m^{-2} s^{-1}"

p. 16013, l. 23 and p. 16014, l. 1: removing the negative values, or setting them to a small positive value will introduces a positive bias in the average fluxes. Are negative values caused by e.g. deposition to vegetation? If they just reflect the random errors in the measurements, then they should not be discarded.

p. 16013, l. 24-26: What are "normal numbers"?

p. 16014, l. 8: change "in agriculture" to "in agricultural lands". Does that include pasture? If not, this could be called simply "cropland". Does it include rice paddies?

p. 16014, l. 12: change "under anthropogenic influence without agriculture" to "in non-agricultural land with anthropogenic influence"

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p. 16014, l. 15: the anthropogenic influence apparently induces a factor 2-3 enhancement of the rates. Some more discussion might be useful. Which land cover classes appear to be more affected by this influence?

p. 16014 l. 19: change title "Methods to improve the emission model" by "Emission model updates"

p. 16014, l. 22: replace "the flux from all the ecosystems... is underestimated" by e.g. "the fluxes from all ecosystems... are underestimated". What explains the very large underestimations for deciduous and coniferous forests? Were the few measurements used by YL95 so low compared to the new flux data used in this study? Or is there another explanation?

p. 16014, last line: change "this effects" by "this affects"

p. 16015, l. 6-8: does that mean that pulsing was based not on 24-hour precipitation rate, but on precipitation accumulated over only one time step (which is presumably very small)? If so, this would be obviously wrong.

p. 16015, l. 14: replace "simple implementation" by "very crude parameterization".

p. 16015, l. 22: replace "merge" by "combine"

p. 16015, l. 25: Does the error bar represent interannual variability?

p. 16015, l. 21-25: 24 land cover classes are used, but with emission factors from the original YL95o model which has only 12 ecosystems. What are the emission factors for the 24 land cover classes? What is the point of this subsection, since the largest emission changes appear to be related to the treatment of fertilizers and not to the distribution of ecosystems?

p. 16016, l. 4-5: what is the reason for the increase over Europe and Central USA? The next paragraph concerns the reduction of emissions associated to fertilizer use in rice paddies.

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p. 16016, l. 12: remove "Saudi"

p. 16016, Influence of model resolution. Please remove the discussion of results for YL95e, since the effect of the model time step is obviously an uninteresting artefact. It could have been interesting to see the effect of resolution for the final YL95/SL10 model.

p. 16017, l. 13: is the scaling factor unique for all land use classes? What are the values of the scaling factor(s)?

p. 16017, l. 15: replace "unlinearities" by "non-linearities"

p. 16017, l. 16-18: this doesn't make any sense to me. Clarify, or remove that last sentence which is probably not indispensable anyway.

p. 16017, l. 20-28: this first paragraph is difficult to follow. It is said that agriculture is ignored as an ecosystem in YL95e, but agriculture is listed in the YL95e ecosystems in Table 1. Please explain better the procedure used in YL95e, as well as the differences with the new procedure adopted here.

p. 16018, l. 2-14: are fertilizers assumed to be applied during the whole year (which would be wrong), or during the growing season only?

p. 16018, l. 12: replace "this already represents" by "which already represents"

p. 16018, l. 15: replace "dataset" by "compilation"

p. 16018, l. 16: remove "it was observed that". How many measurements were used for the $1.0 \pm 2.1\%$ average fraction? What are the reasons for the lower value compared to YL95, and the higher value compared to Stehfest and Bouwman?

p. 16018, l. 22: replace "Our calculated reduction of fertilizer fraction" by "Our estimated fertilizer fraction"

p. 16019, l. 6-7: Does the threshold of 15% apply to soil moisture in the first soil

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layer, or to an average moisture weighted by root depth? Provide some information on the soil model and its assumptions regarding root depth distribution, field capacity and the permanent wilting point. In any case, it would be interesting to see a map of the annually averaged frequency of wet state when using the 15% threshold. It should be reminded that the soil moisture fields are model dependent, and the 15% threshold might lead to very different results in other climate models than the one used here.

p. 16019, l. 23 "individually for each year of the simulation, then determining the mean emission factor from this": unclear, please re-phrase.

p. 16019, l. 25 "we used simulated... for those days": replace by "we used monthly averages instead of averages over those days..."

p. 16020, l. 19: "therefore"

p. 16020, l. 21: the landcover class of the model at the location of the measurement might sometimes differ from the local landcover at the measurement site: which one do you use then?

p. 16021, l. 1: what is "the length of the measurement"? The duration of the experimental period?

p. 16021, l. 3: "and N is the number of measurements"

p. 16021, l. 3: "being"

p. 16021, l. 9: replace "individually" by "separately"

p. 16022, l. 5: the title "Results for iteration by region" is unclear. That section 3.6.1 could be shortened. Table 7 is certainly useful, but the discussion of differences between regions is superficial.

p. 16022, l. 5: replace "clearly" by "well"

p. 16023, l. 2-3 "For the new emission factors... in Table 3": poor sentence.

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p. 16023, l. 5: "the horizontal lines represent...": which horizontal lines?

p. 16023, l. 9: "In the step from LC+FIE+VSM to YL95/SL10, we find an increase of all emission factors in all landcover classes..." could be replaced by something like "Upon the implementation of updated emission factors, the emissions increase in all landcover classes". Note that even larger emission increases could result from the arithmetic averaging of emission factors instead of Eq. (7), with important consequences for the discussion of the results.

p. 16024, l. 2-3: Provide mode details on LAI dataset: year(s), resolution, and how it compares with datasets used in previous SNOx models.

p. 16024, l. 4: "comparison"

p. 16024, l. 5-7: what are these numbers 0.76 and 0.88: hemispheric averages for specific months? The CRF values presumably show a wider variation. What means "a nearly constant interannual reduction"? Do you mean that the global SNOx reduction shows little interannual variability?

p. 16024 l. 10-11 "higher agricultural emissions": this is contradicted by both Table 3 and Fig. 7, i.e. agricultural emissions are lower in YL95/SL10. Could part of the difference with the YL95e simulation be due to differences in the LAI dataset?

p. 16024 l. 12: what means "CRF+0.85"?

p. 16024, l. 11-12: poor sentence, difficult to follow.

p. 16024; desert and shrub have a higher CRF

p. 16024, l. 17: use "adjusting" instead of "tuning"; remove "to provide the best statistics" since it is unnecessary.

p. 16024, l. 22-24: poor sentence, difficult to follow.

p. 16024, l. 25: what is the a priori global SNOx emission in Jaeglé et al.?

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p. 16024-16025 (1st paragraph): In this section, you should first briefly describe the specific application of YL95 by Jaeglé et al., and compare their a priori emissions with YL95o or YL95e. Then, briefly describe the NO_2 data and inverse modeling procedure which was applied by Jaeglé et al. and compare with your YL95/SL10 results.

p. 16025, l. 8, delete "simulated"

p. 16025, l. 9: Please describe first the broad features of the differences: your emission rates are higher over forested areas at mid- and high latitudes, whereas the top-down emissions are higher over relatively arid regions, and relatively a good agreement is found over tropical rain forests.

p. 16025, l. 18: the high top-down emissions in arid regions (notably the Sahel) in Jaeglé et al. are due to the constraints provided by the satellite NO_2 columns, and cannot be explained by differences in landcover classes, unless you mean that crops are widespread in most arid areas. However, it is possible that the emission factors for grassland and shrub ecosystems are underestimated in the model, or the representation of pulsing in these environments might not be well represented. The YL95/SL10 model is based on a fairly large number of measurements, but the geometric averaging implied by Eq. (7) might lead to large underestimations of average emissions. Still, the top-down emissions might be largely overestimated, e.g. due to CTM deficiencies or NO_2 column uncertainties.

p. 16026, l. 2: Replace sentence by "As expected, the geometrical mean values of the measured rates are in good agreement with the simulation of the YL95/SL10 model"

p. 16026, l. 3-4: "The small deviations occur... estimate of the statistical error": unclear, please rephrase

p. 16026, l. 13: replace "duration of the measurements" by "duration of the experimental period"

p. 16026, l. 13-15: unclear. Do you mean that ONLY these classes have short-term

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measurements?

p. 16026, l. 20: replace "the calculation of the new emission factors" by "the calculated emission factors"

p. 16028, l. 6-9 and also l. 15-18: very poor sentences, please rephrase

Table 1: The caption should better describe the table. The first impression is that the ID (1st column) refers to the MODIS landcover.

Table 3: The footnote (a) mentions values between brackets which are not found in the corresponding columns of the Table. The global emission (sum) is missing for the YL95/SL10 model.

Table 4: What are the numbers between brackets? Rephrase "Relative under- and overestimation..." as it is unclear. Rephrase "And the global area defined as wet...".

Table 6, caption, "for the exactly corresponding yearly period": unclear.

Table 7: Replace "Adopted wet and dry emission factors... Fig. 1" by "Derived wet and dry emission factors for the large regions shown in Fig. 1" Replace "2AFR" by "AFR". The number of simulated and measured points is missing for landcover 21.

Fig. 4, caption: "for each corresponding period of the year": which period?

Fig. 6, caption: add "pulsing" after "without". What means "outlined"?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 16007, 2010.