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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Nitrogen Oxide biogenic emissions from soils: impact on NO_x and ozone formation in West Africa during AMMA (African Monsoon MultidisciplinaryAnalysis)" *by* C. Delon et al.

Anonymous Referee #1

Received and published: 7 November 2007

The title indicates an investigation of soil NOx emissions and their influence on ozone formation in West-Africa. This is an interesting subject, and the neural network method used to describe the soil emissions seems an improvement compared to previous methods and emission inventories. However, the article also analyses lightning NOx emissions, while an evaluation of the regional NOx budget should not ignore anthropogenic emission sources. Clearly, the emphasis is on soil emissions, but the description and discussion of the other emission categories is rather thin or absent. For this reason the article does not provide a very deep insight in the importance of soil NOx

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emissions in the regional tropospheric NOx and ozone budgets and a more systematic and thorough analysis is warranted.

The model has been applied to a very short period of 2 days, 5-7 August 2007. How long was the spin-up period? There are several parameters, such as soil moisture, which are influenced by periods of weeks to months previous to 5 August 2006. Since the simulation period is so short, the results are strongly dependent on initial conditions.

The model uses ECMWF data for initial and boundary conditions of meteorological parameters. For chemical parameters only some undefined ozone profiles are used. Owing to the lifetime of ozone of several weeks in the latitude region studied, ozone concentrations within the model domain are dominated by transport. To a lesser degree this also applies to NOx, most significantly in the free troposphere. Furthermore there is no mention of boundary conditions for other compounds. Therefore, either the model run is poorly described or the simulation setup was poorly defined. This is not acceptable since the simulation results will be sensitive to this setup. I suggest the runs are repeated with proper chemical boundary conditions, i.e. from a global model. If the group cannot provide such data internally, public domain model results may be used (see e.g. http://airdata.mpch-mainz.mpg.de/ or contact messy_web@mpch-mainz.mpg.de).

On p.15163 it is mentioned that the new ANN algorithm uses a canopy reduction factor (CRF), which has been ignored for the Yienger and Levy (1995) emissions. Therefore the comparison between the two schemes is not a fair one, and the Yienger and Levy emissions would be lower by including a CRF, regardless of the soil NOx flux parameterization. Although the article includes a relatively comprehensive description of the ANN scheme, it is unclear how the CRF is calculated. Page 15164 mentions that the CRF parameterization is simple. How simple?

p.15165, section 5: The Yienger and Levy (1995) dataset indicates a larger NOx emission flux than the control run. This is a trivial analysis because the control run does not include soil emissions. Further, YL95 produces a different NOx emission flux than

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ANN. Even though the ANN scheme applies a CRF, the emissions seem to be higher than YL95. The analysis of this difference is not very insightful. This section also indicates that the effects on ozone are substantial, though the analysis is weak. It should be investigated how representative the initial ozone fields are, e.g. by comparing with the aircraft data. For example, if the initial ozone concentrations in the model are too low, than the effects of adding NOx to the system will be exaggerated (which seems to be the case). It would be most convincing if the ANN scheme produces realistic NOx and O3 concentrations. Once this has been established it will be very useful to perform sensitivity calculations to show the importance of different NOx sources and parameterizations.

In several instances the article emphasizes that higher soil moisture leads to stronger NOx emissions. It is not clear how this works. Is this an effect of efficient microbial NO production in wet soils or "pulsing", i.e. a consequence of the poorly soluble NO being driven out of the soil after a rain shower? Please also explain how the ANN algorithm deals with these phenomena.

Presumably, the reason for focusing on the period 5-7 Aug 2006 is that aircraft measurement data are available. However, the comparison between measurements and model results is not systematic and the results are not convincing. The potential to compare model output directly with the measurements, i.e. on the same locations and points in time should be exploited.

p.15167: Section 6 presents a validation of the simulation results. I am not convinced the model has been validated by a limited set of aircraft measurements and an eyeballing comparison method. It is stated that the model results are within the range of the aircraft data. Of course, this is a nice result but I recommend a more systematic comparison, which should be possible given the relatively high spatiotemporal resolution of the model. This should be done for both NOx and O3.

The language use should be improved. I propose that the native English speakers in

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the team of authors review the grammar and spelling.

In summary, the ANN scheme seems to improve the NOx emission simulations, especially the response of soils to modeled rain events. The internal consistency of the model to simulate the soil NOx source is much improved. However, the comparison between the different soil NOx emission methods and of the simulation results with the measurements is not very thorough and will need to be substantially improved before considering publication in ACP. The description of methods and the application of models will also need much additional attention. Therefore I recommend rejecting the manuscript in its present form and reconsider it after major modification. In the revision the model description should be improved, and then the results of the selected most comprehensive model run (ALLNOX) should be compared more thoroughly with aircraft measurements. Subsequently sensitivity runs in which emission routines are replaced (YL95) or source categories switched off (soils, lightning, biomass burning, industrial emissions etc.) will help provide an overview of the NOx budget and the influence of source categories on ozone formation.

Minor comments

p. 15156: please remove undefined abbreviations from the abstract (CRTL, YL95, ALLNOx, SOILNOx)

l. 23: remove "a"

l.25: NOx does not directly react with VOCs. Better write "Tropospheric oxidation of VOCs in the presence of NOx and sunlight leads to the formation of ozone".

p.15159, l.11-18: please describe the model setup more comprehensively.

p.15160, l.1-5: How can a study in 1994 corroborate one in 2005, and how can fertilizers be poorly used? Please reformulate.

p.15162/3, last/first lines: It is stated that the Yienger-Levy inventory has too low emissions because West-Africa (and other parts of the world) are poorly defined. Please

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explain.

p.15162, l.11-17: Why are the ozone measurements described in this section even though the data are not used?

p.15163, l.23-24: How is the pH connected to the soil moisture? Please explain.

l.26: Same problem with the sand percentage. Please explain how this affects NOx emissions.

p.15169, l.26: Good that more work can be done. What would you recommend, and in which order?

p.15179, typo caption fig 2: 1010 molecules

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