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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 #2

Received and published: 19 December 2007

The paper (part of a Special Issue on AMMA) arouses strong interest in the reader for two reasons, (a) a rather new approach to estimate biogenic soil NO emissions (ANN) has been applied, and (b) the "further fate" of soil emitted NO has been treated by a coupled mesoscale chemistry-transport model (MesoNH-C). Indeed, this is a new, original, and promising combination.

However, having read the paper, initially high expectations of the reader might be disappointed. This is due to a serious of reasons.

Like referee#1, this referee likes to emphasize, that there is absolutely no satisfying description (even not a summary) of the MesoNH-C model. An comprehensive and (generally) understandable description is necessary to give the interested experts, as well as the potential "average" reader of ACP the chance to judge the scientific progress which might be achieved in understanding the "further atmospheric fate" of soil emitted NO. In this respect, this referee goes 100% along with the corresponding comments of referee#1 of this paper (Atmos. Chem. Phys. Discuss., 7, S6650-S6654, 2007).

The most important concern of this referee relates to the "validation" of ANN/MesoNH-C model results by aircraft measurements. In this direction, the authors are referred to the specific comments no. 25-35, given below. In the present form, the "validation" seems to be neither systematic nor convincing.

Having read the present paper and the paper by Stewart et al. (Biogenic emissions of NOx from recently wetted soils over West Africa observed during the AMMA 2006 campaign; Atmos. Chem. Phys. Discuss., 7, 16253–16282, 2007), the reader cannot help thinking, that both papers are interconnected to some extent. This admittedly somewhat ironic statement of this referee is based at least on the fact, that there is only some half-hearted citation of each other's paper. The reader is left with a strong impression, that there could be much more co-ordination, joint evaluations and other cross-cutting activities between both papers - at least as "validation" issues are concerned (i.e., to yield more than "eye-balling" comparisons as mentioned by referee#1). This referee (also referee for the Stewart et al. paper) likes to go even so far as to recommend a joint paper of Stewart et al. and Delon et al.

Another major shortcoming of the present manuscript is, that major conclusions (as done in section 7) are drawn mainly from a one day model result - which seems to be (at least) daring. The authors mention on page 15163 (line 29) an extended field campaign (Hombori (Mali), July 2004). Was there ever an attempt to reach the same conclusions on the basis of the (most likely) longer-lasting data base of the Hombori-experiment?

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Like referee#1, this referee also recommends rejection of the manuscript in the present form and reconsideration after (at least) major modifications/revisions, particularly in more co-ordinated form between both AMMA Special Issue contributions, Stewart et al. and Delon et al.

specific comments:

1. page 15156, line 20 "This equation allows..." : which equation?

2. page 15156, line 26 "NO emission from soils, among other sources, directly influence NOx concentrations; the emitted NO is quickly oxidised to NO2" rather than "NOx concentrations are, among other sources, directly influenced by NO emitted from soils, which is quickly oxidised to NO2"

3. page 15157, lines 1-2: "Changes in NO sources will consequently modify the rate of ozone production" : O3 production only, if [NO] : [O3] $>$ 2 x 10-4, otherwise there will be (photochemical O3 destruction), see: Crutzen, P.J., Role of the tropics in atmospheric chemistry, in: The Geophysiology of Amazonia, Dickinson, R.E. (ed.), 1987, pp. 107-132, John Wiley & Sons, New York.

4. page 15157, lines 4-6: Ludwig et al., 2001: in the context of line 4-6, one should add a suitable reference from the NOFRETETE project, e.g. Pilegard et al. (2006), Factors controlling regional differences in forest soil emission of nitrogen oxides (NO and N2O), Biogeosciences, 3, 651–661, http://www.biogeosciences.net/3/651/2006/bg-3-651-2006.pdf

5. page 15157, lines 16-23: in this context it would be (at least) fair to add here Ganzeveld et al. (2002); already in the manuscript's reference list (see page 15172, line 15-17)

6. page 15158, line 26: "with 30 levels from 0 to 2000 m": considering the discussion on page 15169, lines 1-18, it would be more than necessary to mention what the explicit resolution of the Atmsopheric Boundary Layer is in the MesoNH-C model.

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7. page 15159, lines 15-16: "Those values come from standard concentrations that can be found in unpolluted air in West Africa": are there any references to provide a proof for this statement?

8. page 15159, line 25: "Fig 1b" rather than "1b"

9. page 15160, lines 24-25: "In order for different situations to be represented,…": it seems to be some awkward English

10. page 15160, lines 26-27: "For this purpose, the databases used in this algorithm contain data from temperate and tropical climates": does this really work? In other words: are "temperate data" working properly und "tropical conditions"?

11. page 15161, eq. (1) and (2): what is the (physical, conceptual) meaning of all these w,i's and S,i's ? page 15161, lines 5-8: the potential "average" reader of ACP should have a fair chance to understand the discussion in section 4 of the manuscript, particularly on page 15163; for that a (short) summary of the ranking of importance of the seven parameters (which are used in ANN) seems to be definitely necessary. Furthermore, in agreement with referee#1 of this manuscript, the potential "average" reader of ACP should have another fair chance to understand the fundamental importance and impact of soil moisture on biogenic NO emission (e.g. describing the "optimum curve" response of NO emission to soil moisture; a very good fundamental study in this direction is published by Skopp, J., Jawson, M.D., Doran, J.W., Steady-state aerobic microbial activity as a function of soil water content, Soil Science Society of America Journal, 54, 1990, 1619–1625.

12. page 15163, lines 3-25: Is it really fair to "compare" monthly & 1°/1° simulations (YL95) with single day & 20x20km simulations (ANN)? Wouldn't it be more appropriate to "up-scale" ANN-simulations to 1 month & 1°/1°, and compare then? By the way, this referee goes 100% along with referee#1, as far as the basic problem of "comparing" YL95 and ANN is concerned, see ACPD, 7, S6650-S6654, 2007; page S6651, para 4.

13. page 15163, lines 20-23: Is the response of NO emission really so fast? Also: was the drying of soil so fast? The rain event was obviously late on 5 August / morning of 6 August (see page 15162, line 2), therefore, drying and NO emission response has happened within 6-9 hours?

14. page 15164, lines 5-9: "If we consider that the emission deduced from the ANN calculation ranges from 0.8Œ1010 to 4.2Œ1010 molec cm−2 s−1 (which compares well with Ganzeveld et al. (2002) for the same region) a total …": To judge whether or not the ANN results "compare well" to the estimates of Ganzeveld et al. (2002), the results (numbers) of Ganzeveld et al. (2002) would be necessary.

15. page 15164, lines 12-13: "A part of the NO emission from soil is deposited onto plants in the form of NO2, this fraction depending on the vegetation density": The referee feels, that the potential "average" reader of ACP should have a fair chance to understand what is behind the "Canopy Reduction Factor". This statement (lines 12-13) is definitely not enough, at least because the problems of in-canopy chemical transformation of NO to NO2 (by O3) and the potential back-reaction (photolysis of NO2) is generally not known… A pretty good description can be found in Jacob, D.J., Bakwin, P.S., 1992. Cycling of NOx in tropical forest canopies. In: W. B. Whitman (Editor), Microbial Production and Consumption of Greenhouse Gases. American Society of Microbiology, Washington, D.C., pp. 237-253

16. page 15164, line 13: "A simple equation has been implemented in MesoNH-C, considering …": Like referee#1, this referee likes to ask "how simple?" (s. ACPD, 7, S6650-S6654, 2007; page S6651, para 4, last 2 lines. It appears a bit awkward, that the authors leave the reader with the complexity of CRF formulations/approaches, citing Ganzeveld (2002), Gut et al. (2002), and Kirkman et al (2002), whereas their own approach is (a) named "simple", and (b) not given.

17. page 15165, line 5: "dispersion" rather than "diffusion"

18. page 15165, line 6-8: "Note that the transect between 5 and 21_ N covers a

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large spectrum of vegetation, through mosaic forest, cropland, grassland and desert": It is suggested to provide a CRF-map of the domain in order to support that sentence (and to give the potential reader an impression about the significance of vegetation distribution to the simulated NO emissions).

19. page 15165, lines 8-12: This can't be seen from Fig. 3a.

20. page 15165, lines 12-15: considering the content of this sentence: what is really shown in Fig 3b: SOILNOX- CTRL or SOILNOX-YL95?

21. page 15165, line 18: "As NOx is one of the principal precursors of tropospheric ozone,…": see comments of this referee to page 15157, lines 1-2

22. In the context of page 15165, lines 18 ff : It is not easy to follow the authors how (and why) they have chosen those three plots in Fig. 3 and Fig. 4. May be, for the sake of clarity/consistance, it may be more appropriate to show: Fig.Xa: YL-CTRL, Fig.Xb: SOILNOX-CTRL, Fig.Xc: ALLNOX-CTRL, and finally Fig.Xd: ALLNOX-YL95?

23. page 15166, line 6: "This is verified in our simulations, where the…": One should be very cautious using "verified" in this context; "This is SHOWN in our simulations, where the…" seems to be more appropriate.

24. page 15166, lines 10-11: "…when the effect of convection on chemistry is usually the strongest": Is there (any) reference for this statement?

25. page 15167, line 6: there is a subheading "6.1" (Comparison between chemical profiles from model and measurements); is this referee right with the assumption, that the corresponding subheading "6.2" is missing somewhere between page 15167 and page 15169?

26. page 15167, line 13: "to sample freshly emitted NOx air masses": it seems to be some awkward English

27. page 15167, line 17: altitude in Fig.7: in m (above sea level) or in m (above

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ground)?

28. page 15167, line 18-19: "…in the south west to 200 pptv in the north east of Niamey": this can't be seen from Fig. 7, since Fig. 7 displays an latitudinal cross-section.

29. page 15167, line 27 to page 15168, line 4: However, the BAe data are (at least from 15.1 to 15.8°N) higher than the ANN data at least by a factor of two! This discrepancy would become serious, if one would include the NOx concentrations measured during the BAe flight directly in Fig. 8 (in the corresponding colour code of Fig. 8): then, at 450 m & around 15°N, one would see a distinctive trace of red & purple colours within the green (deep green) results of ALLNOX-simulation!

30. page 15168, line 5-10: "Roughly, the range of measured concentrations is reproduced by the model": That's funny. On page 15163 (line 13-16), there is a strong emphasis given to the 20x20km resolution of the ANN/MesoNH-C model. If this spatial resolution would be still be stressed for the "validation" of the ANN/MesoNH-C results by the BAe results, then the "validation" fails - there is (at least) a factor of 2 difference (if not a factor of 4). However, ONLY if the authors here "reduce" the resolution to the order of 1°/1° (as for YL95), than (with corresponding averaging) ANN/MesoNH-C results may "reproduce" the BAe results!

31. page 15168, lines 17-23: As long as the "validation" of ANN/MesoNH-C results by the BAe results is (in the best case) within a factor of 2, it seems not very appropriate to discuss whether or not a "slight increase" of NOx (even in the order of 30-40%) as observed by the BAe measurements has also be found or not found by the ANN/MesoNH-C model.

32. page 15168, line 24: "…and at the top of the boundary layer,…": What was the top altitude of the boundary layer?

33. page 15169, lines 1-11: "The underestimation above 500m may be explained by

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the dynamics": by the (supposed) dynamics of the (real) boundary layer/troposphere or by the model (MesoNH-C) dynamics? Furthermore: is the difference of the boundary layer height (model vs. observations) a problem of non-appropriate model dynamics? If so, what is then the significance of "validation" as described in section 6?

34. page 15169, line 23: "…between −2 and +7 ppb": in Fig. 11a, the y-axis ranges from between −2 and +7 ppt (not ppb!)

35. page 15169, line 25-26: "The ANN model is an improvement on YL95,…": Where is the justification for this statement coming from?

35. page 15170, lines 14-15: "….and can also be slightly identified in the boundary layer": This referee likes to ask, whether a quantitative (qualitative) proof has been provided for this statement.

36. page 15170, lines 25-27: " A pretty good agreement has been found between modelled and measured concentrations on the 6 August, where the model reproduces correctly the passing of a convective system and the observed resulting NOx enhancement": This referee doubts seriously the justification for "pretty good agreement" and "reproduces correctly" (since there is a difference of at least a factor of 2 between ANN/MesoNH-C results and BAe results).

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 15155, 2007.