Interactive comment on “Biogenic emissions of NO\textsubscript{x} from recently wetted soils over West Africa observed during the AMMA 2006 campaign” by D. J. Stewart et al.

Anonymous Referee #2

Received and published: 26 January 2008

There is no doubt that the manuscript by Stewart et al. is of high scientific significance. It is basically worthwhile to be published in ACP. However, scientific quality as well as presentation quality of the paper is still not matching the high level of scientific significance. Like referee#1, I suggest significant revisions of this paper and/or merging with another AMMA Special Issue paper (see below).

comments:

page 16255, line 22-24

It is strongly recommended, that in a revised version of the paper, a certain piece of text
should be found, where the issue of the atmospheric transformation of NO to NO2, the (fulfilled/not fulfilled) assumption of photostationary equilibrium between NO-NO2-O3, etc. is elucidated to potential (non-specialized) readers of ACP.

The problem of non-specific conversion of the molybdenum converter (TECO) is addressed; however, discussion and conclusions from the data < 1 ppb certainly still need more quantitative elaboration (particularly some enhanced statistical analysis of uncertainties). The results of Steinbacher et al. (2007) would expect 27-50 % systematic overestimation by the TECO instrument (rather that only 16% as found by the authors). The findings of Dunlea et al. (ACP, 7, 2691-2704, 2007) would rather support 50 % systematic overestimation. In this respect the authors’ own findings on page 16265, line 16-19, namely "in the latter case the PAN levels were of the order of 60 ppt whilst the TECO instrument was overestimating NOx by around 200 ppt" will certainly grab the critical attention of any (experienced) reader.

The validity of eq. (1) presumes a variety of (justified?) assumptions (as stated by the authors), e.g. negligible horizontal and vertical advection of NOx, as well as negligible vertical divergence of NO-, NO2-, and O3-fluxes in the entire atmospheric boundary layer. The serious problem of (NO-, NO2-, O3-, etc.) advection has been already addressed by referee#1 (Delon et al.), as well (to some extent) by referee#1 of this paper. However, the neglecting vertical divergence entirely (just on the assumption of complete vertical mixing) seems a bit risky (without any further proof). Is there no support (for "complete" mixing) from vertical profiles of (virtual) potential temperature, or any other scalar quantity? How, Delon et al. (2007) have handled this problem?

this equation contains definitely the wrong sign in the exponent of the (most right hand
side) exponential function.

The dry deposition of NO₂ to west African soils (vegetation?) may be low, but \( v_d(\text{NO}_x) = 9 \times 10^{-2} \text{ cm/s} \) could be considered dramatically low. Giving the rich treasure of related literature, the authors are kindly asked to consider more than only one reference for it (a few examples are given below; "references (1)").

The authors obviously do not belong to the biogeochemistry/soil flux community. For a revised version of their paper, the authors may consider the fact, that the biogenic NO emission (\( F(\text{NO}) \)) from soils is a strongly non-linear function of soil moisture (water filled pore space, WFPS), i.e. the emission follows an optimum curve (the reviewer suggests for semi-arid, sandy soils: a narrow maximum of \( F(\text{NO}) \) around WFPS \( \leq 0.2 \)). This fact may help in particularly interpreting the results in sub-chapters 3.2 and 3.3. It will particularly address the question of biogenic NO emission vs. wetting and/or (fast?) drying of soils. By the way, on page 16267, line 5, the authors claim to have used data of satellite measurements of soil moisture. So, if data on soil moisture are available, why not to make explicitly use of the known dependence of \( F(\text{NO}) \) vs. WFPS? The authors will find a lot of information and helpful support in this direction in the papers cited at the end of referee#2’s comments ("references (2)").

Like referee#1 of this paper, referee#2 also wonders that "the authors assume that soils that have not been wetted recently do not emit NOₓ". The authors are kindly referred to corresponding literature which will provide them with some helpful data and material (given at the end of referee#2’s comments, ("references (3)"). Particularly, the authors may find in Otter et al. (1999) interesting information on the effects of "pulsing" and
re-wetting on the biogenic emission from semi-arid African soils.

Page 16266, line 12

There is a more recent review on "NO from soils... for a variety of land surface types in Africa and other tropical regions" than the 1977 review of Davidson and Kingerlee (namely, Meixner and Yang, 2006, see "references (3)”).

Page 16267 and 16268, section 3.6

Referee #2 agrees 100% with referee #1 of this paper, as far as criticism on the authors' flux estimates is concerned.

Page 16269, line 6-8

To judge whether or not "Figure 7 shows a good correlation...", some more information, e.g. statistical quantities (n = ?, P(0.05), P(0.01), etc.) might be helpful.

The referee of this manuscript (Stewart et al.) is also referee of another paper (De lon et al., Nitrogen oxide biogenic emissions from soils: impact on NOx and ozone formation in West Africa during AMMAACPD, 7, 15155–15188, 2007), which has been also submitted to the ACP Special Issue on the AMMA 2006 campaign. The authors of the present paper have mentioned that in their manuscript once, page 16256, line 10-18. There are only two more citations of the paper by De lon et al., namely page 16262, line 2-5 and page 16269, line 10-12. In turn, De lon et al. made only two (minor level) references to Stewart et al., namely on page 15167, line 19-22 and page 15168, line 15-16. Both papers make some mutual use of data and/or results. It is undoubtful that both papers share an identical aspect of one of the major present time scientific endeavours (the AMMA project). On the other hand, the reader of both papers (in their present form) is hardly able to deny a substantial lack of co-ordination between both papers. However, that would not only be desirable from a scientific point of view, it is being supposed to have occurred with respect to the well-known and high level of (scientific) co-ordination.
within the AMMA project. In their present form, both papers received substantial criticism (see: http://www.cosis.net/members/journals/df/article.php?paper=acpd-7-16253, and http://www.cosis.net/members/journals/df/article.php?paper=acpd-7-15155, particularly with respect to "not very thorough comparison between the different soil NOx emission methods/simulation results with the measurements" and to considerable "flaws in the interpretation of flux measurements and their extrapolation". The referee feels, that more and also more comprehensive co-ordination of both papers would easily overcome these criticisms. Moreover, with respect to the high importance of the common scientific issue, it is strongly recommended to join both papers; any (anticipated) arguments about an (oversized) length of such a joined paper should not discourage the authors: two (companion) papers sharing the same title, but separated into "Part I" and "Part II" should even increase the attractiveness and importance of both papers.

minor corrections:

page 16255, line 3 "Prather and Ehhalt" rather than "Prater and Enhalt"

(1) references for dry deposition of NO2:


Wesely, M. L. (1989), Parameterization of surface resistances to gaseous dry deposition in regional-scale numerical models, Atmospheric Environment, 23 (6), 1293-1304.

(2) references for the dependence of biogenic NO flux from soil moisture:


Ludwig, J., Meixner, F.X., Vogel, B., Förstner, J. (2001), Processes, influencing factors, and modelling of nitric oxide surface exchange&amp;#8212;an overview, Biogeochemistry,
52(3), 225-257.


(3) references concerning biogenic emission of NO from arid and semi-arid ecosystems:


