Biogeosciences Discuss., 12, C1143–C1145, 2015
www.biogeosciences-discuss.net/12/C1143/2015/

Biogeosciences
Discussions
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Interactive comment on "Modelling the effect of soil moisture and organic matter degradation on biogenic NO emissions from soils in Sahel rangeland (Mali)" *by* C. Delon et al.

Anonymous Referee #2

Received and published: 10 April 2015

After revisions, the discussion paper "Modelling the eect of soil moisture and organic matter degradation on biogenic NO emissions from soils in Sahel rangeland (Mali)" is finally recommended for publication in BG. Despite the poor data which are challanging to get in these remote conditions, the major concerns are all adressed sufficiently. In general the autors should be careful to speculate about production and consumption processes based on their modelling results. For further conclussions about processes/pathways more biogeochemical analyses and molecular data are necessary. The paper adresses an highly interesting topic of N cycling in Sahel rangeland soil.

Minor comments are:

Since the release of NO is limited to the uppermost layer of soil, where water is lacking in the Sahel, the statement "Abstract, l.5: [...] the contribution of the Sahel region in emitting NO is no more considered as negligible." should be reformulated. According to recent knowledge these emissions are stated to be abiotic release of NO (e.g. McCalley and Sparks 2009). Due to a lack of additional measurements no final conclusion can be drawn about the origin of NO emissions.

p.1156, l.5: "The link between NO production in the soil and NO release to the atmosphere is investigated in this study [...]" This statement should be reformulated. Due to poor data this study focuses only on model development, not validation.

p. 1157, l. 5: The importance of (gaseous) N-deposition and N2 fixation for natural soils is missing.

p. 1157, l. 21: "A difference has to be defined between NO production in the soil and NO emission (release) to the atmosphere. NO emission to the atmosphere might deviate significantly from the production of NO in soil." Reformulate: The release of NO as well as the NO flux is the result of production and consumtion processes in soil. In many previous studies it was observed that the NO release rate equals the NO production minus the NO consumption.Therefore, for sure NO production and NO emission (release) are different as indicated already in their units.

p.1162,l. 4 ff.:

The chamber method needs to be explained in much more detail. It is confusing to refer to the chamber as dynamic if it is closed and the formula of a static chamber was used to calculate fluxes. Furthermore, for the usage of such a chamber it is of importance if (1) the chamber was transparent or opaque, (2) the inflow was O3 free, (3) the pressure in the headspace of the chamber is equilibrated to ambient pressure levels (see e.g. Pape et al. 2009 BG). The pump of the 42C TL NOx analyzer creates an underpressure. Connecting the instrument directly to the chamber should create a strong pressure difference between the chamber headspace and ambient. It is highly

recommended to measure the pressure in such a setup.

1163, l. 11: Under the usage of O2 and constant temperature in the lab, other autors found a detection limit of 150 ppt. Therefore, it is recommended to use 150 ppt as detection limit. Furthermore, the detection limit could be converted into a minimal measurable flux. This flux should be included in Fig. 6b, to give readers an idea about how close this fluxes are to the detection limit of the instrument. Furthermore, it is suggested to investigate the temperature dependency of the analyser for different NO mixing ratios due to high and canging ambient temperatures in the field. This will maximize the quality of the few data points which are available for validation.

1163, l. 16: Correct "zone"

Correct "Schindblacher et al., 2004".

Confussive use of the term concentration. Replace by mixing ratios when using unit ppb, ppt.

Interactive comment on Biogeosciences Discuss., 12, 1155, 2015.