We would like to take the opportunity to thank the reviewers for their valuable and constructive comments. Every question is answered in italic in this document, and the text has been modified accordingly in the manuscript.
Some typos have been corrected throughout the manuscript, and some new references have been added, without modifying the content of the text.

Acknowledgements have been added:
"The authors thank the reviewers and the editor S. Noe for their help in improving the manuscript.
Special thanks to Corinne Jambert for her wise and useful advices on flux measurement techniques.
D. Moorhead is also acknowledged for providing us with the GENDEC model."

Responses to Referee #1

Delon et al. provide a comprehensive and rather well-written overview of practical stepwise application of three different models (STEP, GENDEC and NOFlux) to simulate potential biogenic NO emissions under scarce available data condition for understudied arid region (Sahel, Mali). Indeed, such coupled model is desirable to roughly rapidly estimate NO emissions in remote or abandoned regions with a little input data. However I am concerned about the validation of this model, since the measured data are really very limited and, besides these data do not coincide well with simulated ones. Nevertheless it is still noteworthy attempt which can give a hint to scientific community to stimulate further field measurements and to look closer to couple model approaches for remote regions investigations. Therefore, I can recommend publication of the manuscript in BG. However, there are some major points that should be addressed in order to increase the accessibility of the text and to discriminate it from reviews that have been published recently.

Major points

1. Authors have too little measured data for model validation.

Yes indeed too little data is available to validate the NO fluxes. The model reproduces quite well orders of magnitude of NO fluxes in Sahel, and gives the opportunity to provide simulated data for larger scale models which usually do not simulate the whole quantity of NO emitted by Sahelian soils. Therefore these modeling results, in accordance with the few measurements available in the region, can be useful to large scale modelers.

2. Authors should try to think how present more attractively the 'coincidence' of these scarce data of wet season with your simulated ones. As it is now I would not present them in Fig. 6, because they 'tell' nothing for support your model. Otherwise you can exclude it from Fig.6 and mention only average values in Table 3.

Fig 6b has been removed. Measured data for wet season 2004 and 2005 were already in Table 3. They are now referenced by the key word « this work ».

3. It is still not clear for me why you prefer to use units per year (kg N ha-1 yr-1) throughout the manuscript for mean values. It is not possible to express daily and

seasonal average (or even sum) in per year units. Is it just common mistake? Please explain it.

This unit is often used in the literature to describe N compound fluxes and was therefore chosen to easily compare our results to other works. It is true that other units are also often used, such as ngN.m^{-2}.s^{-1}. (which are more appropriate when dealing with daily or subdaily fluxes)
We have therefore changed kgN.ha^{-1}.yr^{-1} in ngN.m^{-2}.s^{-1} throughout the paper to use a more consistent unit in accordance with the time steps used in this work, except when yearly averages are calculated. In that case, we add the unit kgN.ha^{-1}.yr^{-1} in parenthesis to the unit ngN.m^{-2}.s^{-1}.

Minor points

- Introduction section. Please use updated knowledge from some recent reviews (see below) for that section (e.g. in P1157: L8-9, L25-26): Schreiber et al., 2012 doi:10.3389/fmicb.2012.00372; Pilegaard, 2013 doi:10.1098/rstb.2013.0126; Medinets et al., 2015 doi:10.1016/j.soilbio.2014.09.025

Thanks for these suggestions. The references have been included in the text.

- P1158 L2-3 Very general statement. Please concrete which gases do you mean (quotation is needed).

The sentence has been modified and becomes:
« Most of the trace gas production and consumption processes in soil (trace gases such as NO, N_2O, CH_4, CO) are probably due to microorganisms (Pilegaard 2013, Conrad 1996, Fowler 2009). »

- P1158 L15 What about re-deposition in form of NO_2 (check Gessler et al., 2000; Butterbach-Bahl et al., 2004)

The concept of Canopy Reduction Factor and net emission above the canopy was introduced a bit later in the introduction. We have moved the sentence concerning CRF, and added the following sentence :

« After NO is oxidized into NO_2, NO_2 can be deposited on the vegetation, decreasing the net emission of NO above canopy to the atmosphere. Above canopy emissions are calculated by introducing the Canopy Reduction Factor (CRF) concept, based on the Leaf Area Index (LAI), and considering the canopy as an absorber of NO_2 (Yienger & Levy 1995, Butterbach-Bahl et al, 2004).

- P1159 L13-16 I think it is too ambitious statement that modelling can help to describe and understand processes . I think it is other way around: laboratory and field measurements can describe and help to understand process and can help improve model as well.

I totally agree with this statement. Our basic idea was to say that the model helps having a more comprehensive environmental description around NO fluxes, because it provides more

environmental variables than what can be easily provided in field experiments. The sentence has been changed in

« Modelling is therefore a precious help to describe the environmental conditions that favour or not NO emissions. However, in the same time, laboratory and field measurements are necessary to better understand production and consumption processes in the soil leading to the release of NO, and to improve modeling approaches".

- P1163 L16 Ozone

OK

- P1163 L18-20 Please indicate at which height NO ambient concentration was measured (in chamber or 2 m height or)

NO ambient concentrations are measured at 20 cm above the ground surface, i.e. the height of the chamber. This was added in the text.

- P1163 L22 Please indicate below which magnitude

The magnitude has been specified. The following sentence has been added:
"Pape et al. (2009) estimate that even for cases with a large absolute chemistry effect (meaning NO fluxes from soils up to 4 ngN m-2 s-1, with NO mixing ratios above 5 ppb and ozone mixing ratios between 15 and 20 ppb), the underestimation due to chemical effects is less than 50%. As a comparison, Laville et al. (2001) finds a maximum underestimation of 25%. "

- P1163 L27 The same as previous

This reference was removed because the experiments by Schindblacher et al. (2004) were done in the dark, excluding photochemical reactions. This example was therefore not consistent with the subject developed here.

- P1165 L12, L14 and FURTHER THROUGHOUT THE MANUSCRIPT It is not correct to express daily fluxes in kg N ha-1 yr-1 (should be per s-1, min-1, h-1, d-1).

$kgN.ha^{-1}.yr^{-1}$ has been changed into $ngN.m^{-2}.s^{-1}$ throughout the manuscript. For yearly means, fluxes are still indicated in parenthesis in $kgN.ha^{-1}.yr^{-1}$. See answers to major comments 3.

- P1171 L7-8 Estimated or measured data? Your data or include citation

This is calculated from our data. This was specified in the sentence.

- P1178 L7-8 If your data put R value or quotation

This is explained later in the "sensitivity tests" paragraph. To be clearer, we have completed the sentence. The sentence is now:
"Actually, the influence of temperature also exists during the wet season at a diurnal time step (highlighted by Ludwig et al., 2001), but is dominated by soil moisture effect. This temperature effect is better described later on in the sensitivity tests paragraph.

- P1178 L28 Please cite these several studies

The sentence has been developed to give more precisions:

"Several studies have shown different ranges of NO fluxes, but always with a strong link to soil moisture, especially in tropical regions where distinct dry and wet seasons exist, and where large pulses of NO emissions occur at the onset of the rainy season (Ludwig et al., 2001, Otter et al. 1999, Meixner et al., 1997, Yang & Meixner 1997, Meixner & Yang, 2006, Van Dijk et al., 2002, as examples in tropical and semi arid regions)"

- P1179 L1 It is totally different example. Can you cite example from semi-arid or arid regions

This example was given on purpose, to give an idea of the magnitude of fluxes in other tropical regions where the soil N content is totally different. Examples from semi arid regions are now given just above as suggested.

- P1179 L6-7 Where did you get data for ratios. I missed any dry season data throughout the manuscript (only wet season and annual data were mentioned). Please include it in Table 3 or mention somewhere.

Dry season data were included in Table 3.

- P1181 L25 reSpiration

OK

- P1183 L21-22 Even wet season dataset is not enough for validation, but not only dry season data.

The sentence has been modified:
"Measurements during the dry season are scarcer in the literature than during the wet season, which complicates even more the validation of the modeling results."

- Table 3. Check UNITS for mean values! Include dry season data if any?! I would suggest to arrange all the mentioned data chronologically for each site (easy to deal for readers)

Units have been checked and corrected, dry season values have been included (they correspond to model results). Data from measurements have been arranged in chronological order, and separated from model results.

- Figure 2. Name of Y axis: 'Soil moisture' is better than 'Soil humidity'

OK, this was changed

- Figure 6. Already mentioned in Major points

Figure 6 was modified.

Responses to Referee #2

After revisions, the discussion paper "Modelling the effect 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.

The sentence "the contribution of the Sahel region in emitting NO is no more considered as negligible." is not a result of the present study, but a sentence that sums up what has been found from previous studies (Hudman et al., 2012, Delon et al., 2012, Galy-Lacaux & Delon, 2014 as examples).
The following sentence has been added:
"Indeed, NO is one of the most important precursor for tropospheric ozone, and previous studies have shown that arid areas potentially display significant NO emissions (due to both biotic and abiotic processes). Previous campaigns in the Sahel suggest that the contribution of this region in emitting NO is no more considered as negligible. However, very few data are available in this region, therefore this study focuses on model development".

- 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.

This sentence was completed:
"NO production in the soil and NO release to the atmosphere is investigated in this modeling study, by taking into account vegetation litter production and degradation, microbial processes in the soil, emission fluxes, and environmental variables influencing these processes, using a coupled vegetation-litter decomposition-emission model."

- p. 1157, l. 5: The importance of (gaseous) N-deposition and N_2 fixation for natural soils is missing.

The sentence has been modified and completed:
"In natural soils, these compounds come from Biological Nitrogen Fixation (BNF, Vitousek et al., 2013), from atmospheric dry and wet deposition (Laouali et al., 2012, Galy-Lacaux et al., 2009) and from the mineralization of organic matter through the bacterial and fungal decomposition of dead matter"

- 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.

The sentence was reformulated:

"The release of NO (NO emission) to the atmosphere is the result of production and consumption processes in the soil. In many previous studies it was observed that the NO release equals the NO production minus the NO consumption."

- 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.

Some details and clarifications have been added in the text, concerning the several points raised by the reviewer. The sentences in "" have been added in the text.

"NO fluxes were determined at Agoufou during summers 2004 and 2005, from closed dynamic chambers (flowed-through-non-steady-state) measurements defined in Pihlatie et al., 2013. A comprehensive description of the chamber device and calculation flux theory is available in Pape et al., 2009."

Chambers are made of stainless steel and are therefore opaque.

"The inflow is not ozone free. Therefore, due to chemical reactions inside the chamber, the fluxes are underestimated. This underestimation is calculated (see below) and is small due to low ozone mixing ratios."

No pressure difference has been noticed by switching from chamber head space to ambient concentration.
"A small vent of 4mm in diameter provided the pressure equilibrium between the inside and outside of the chamber. As the chamber is ventilated (a circulation of air is always assured by the small vent and the pumping), the system is assumed to be dynamic."

The following sentence has been added:
"Following Davidson et al. (1991) and Serça et al. (1994), the flux is calculated from the slope of the increase of concentration within the chamber, assuming that this increase is linear during several minutes (no chemical or deposition loss during that period), and that the air flow is constant. One should note that, as long as the air flow rate is constant, it does not need to be taken into account for the flux calculation (see below).

Considering that the mass of NO within the chamber at time t + dt is equal to the mass of NO present at time t, plus the mass of NO entering the chamber in the dt interval (soil flux), minus the mass of NO leaving the chamber in that same dt interval, if the air flow is constant, only the soil flux has to be taken into account in this mass balance."

- 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.

According to reviewer #1, figure 6b has been removed, and comparison of measured fluxes with simulated fluxes is included in Table 3.
The minimal flux calculation is made for a 150 ppt sensitivity threshold and included in the text. This minimal flux is 0.25 ngN m^{-2}. s^{-1}.

The temperature dependency of the analyzer has not been taken into account, since the analyzer was new and well functioning when used in the field.

- 1163, l. 16: Correct "zone"

OK, this correction was done.

- Correct "Schindblacher et al., 2004".

After a comment from reviewer #1, this citation page 1163 has been removed because it was not correct to use it to illustrate photochemical reactions (measurements were made in the dark).

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

This was done throughout the text when necessary.