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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.

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On behalf of all co authors, I would like to thank anonymous referees for their positive and constructive comments on this study. We have tried to address all comments and to bring some new information to the paper, in order to improve its structure and its readability. Each point raised by the referees is reminded and is given an answer. Answers for Referee #2 answers are given below answers for referee #1. A revised manuscript is proposed in supplement, as well as table 1, hardly readable in the following text.

Referee #1

1) NO flux methodology: - Is there any proof of evidence, that the emission of NO is of biotic origin? Due to a Mc Calley and Sparks (2009) abiotic processes seem to dominate the emission of NO. The use of the terms NO emission, NO release, and NO production is misleading.

Indeed, the use of NO emission instead of NO production or NO release is not appropriate and will be corrected in the text: Page 1 end of last paragraph: “A difference has to be defined between NO production in the soil and NO emission (release) to the atmosphere. Emission of NO to the atmosphere might deviate significantly from the production of NO in soil. Several biotic and abiotic processes in soils and plants are mechanisms for production and consumption of NO (Galbally 1989, Conrad 1996). Microbial nitrification and denitrification constitute the principal processes (Ludwig et al., 2001). According to Mac Calley & Sparks (2009) and references therein, fluxes are regulated by factors that include the concentration of inorganic N (NO3- and NH4+), soil moisture, temperature, accessibility of labile C, and physical soil properties.”

Furthermore, the role of NO consumption and NO compensation point (see works from Conrad et al.) is not explained.

Some explanations have been added in the text concerning production, consumption and compensation rate. Page 2 first paragraph: “Most of the trace gas production and consumption processes in soil are probably due to microorganisms. Oxidation of NO to nitrate has been found to be the dominant NO consumption mechanism in some soils (Conrad 1996 and references therein). Release rates of NO can be much lower than the NO production rates, since NO consumption is of similar magnitude to NO production. NO shows both high and variable production and consumption rates in soil and consequently highly dynamic compensation points (Conrad 1996). The concept of the compensation concentration is based on the observation that production and consumption of a trace gas occur simultaneously in a soil and that the consumption rate is a function of the trace gas concentration, whereas the production rate is not (Conrad, 1994). According to Ludwig et al. (Biogeochemistry, 2001), the net exchange of NO

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between ecosystems and the atmosphere is globally dominated by biogenic emissions of NO from soils. Only at exceptionally high ambient NO concentration, direct deposition to plants might constitute a significant removal mechanism for atmospheric NO (Ludwig et al., 2001)."

p. 1, line 65 recent review about pulsing (Kim et al., 2012 BG) is missing.

Kim et al. (2012) has been added page 2 second paragraph.

The importance of canopy reduction factor (CRF) is missing.

The definition of CRF has been included in the text page 2 third paragraph. However, as our study takes place in the Sahel, the CRF is of lower importance than in other ecosystems: indeed, the LAI reaches 1.8 at the most for the year 2006 for example (Mougin et al., 2014), which reduces the release of NO to the atmosphere at a rate of 17% max during a short period (10 days). The following very simple equation has been used for CRF, based on Delon et al. (2008) (CRF = 1 - 0.0917*LAI). This simple equation gives the same results as the one used in YL95. As a consequence, resulting fluxes are slightly lower than in the preceding version of the paper during the wet season (between 8 and 16% less depending on the year).

Improvement of Yienger and Levy algorithm by Steinkamp and Lawrence (2013) is missing.

OK, added in the text page 2 third paragraph, as a comparison to YL95. As far as the authors know, Steinkamp and Lawrence is a 2011 contribution.

p.3, line 180: 'NO flux data sampling' correct to: 'Calculation of NO flux' and include equations!!! –

OK, done page 3 second column. Equations can also be found in Serça et al., 1994, concerning calculation of NO flux from concentration increase in the chamber, and in Delon et al. (2007) concerning the ANN algorithm, as already mentioned in the text.

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p.3, line 182 and p.3, line 184 contradiction: Did you use a static or dynamic chamber?

We use closed dynamic chambers (flowed-through-non-steady-state). These chambers are different from static chambers (non-flow-through-non-steady-state chamber), or open dynamic chambers (flow-through-steady-state). This was specified beginning of section 2.2.

Please include equations as well as additional parameters (dimension of chamber, conversion factors, limit of detection, etc.). Additionally, it would be great to demonstrate in a proof of evidence, that the setup was not impacted by temperature and pressure. A lot of static chamber setups the temperature and pressure within the chamber differ significantly from the ambient conditions which leads to artefacts.

Added in the text page 3 second column: "Stainless steel chambers of 800 cm2 area (40*20) and 18 cm height were used. A stainless steel frame is inserted into the ground before the measurement which starts when adjusting the chamber on the frame, sealing being assured by a slot filled with water. The air inlet is on one side of the chamber, the air outlet on the other side is connected to the analyser with two meters of Teflon tubing, so that the chamber is swept with an air flow only due to the pump of the instrument. The residence time of the air inside the chamber is approximately 12 minutes. No significant change in air temperature in the chamber has to be noticed during this lapse time. Pressure is assumed to be constant throughout the flux measurement and equal to ambient pressure. The net flux is calculated from the slope of the increase of concentration within the chamber. F=(dC[NO]/dt)(VMN/SRT) dC/dt is the initial rate of increase in NO concentration calculated by linear regression (ppb $s-1$), MN is the nitrogen molecular weight (grams/mol), S=800 cm2, surface of the chamber V=18l, volume of the chamber R=0.082 cm3 atm mole-1 K-1, T(K) is the air temperature in the chamber.. F in ngN m-2 s-1. (Serça et al., 1994).

NO concentration in the chamber was measured using a ThermoEnvironment® 42 CTL analyser. This analyser detects NO by chemiluminescence with O3. Detection

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limit and sensitivity is around 0.05 ppbv. Flow rate in the analyser and the chamber is about 0.8 l min−1. Multipoint calibration was checked before and after each field experiment with a dynamical calibration system."

Overall it would be helpful to present mixing ratios as the first order results measured by the analyzer and a later conversion to the NO flux. It would be easier to follow if the authors would spend as much time and lines for section 2.2 as they did for the model description.

An example of concentration measurements and conversion to flux can be given for one specific case: Case of 30th June, 2004: dC/dt = 8.3 ppb min-1 = 0.14 ppb s-1 = 0.14 * 0.18*14*1000/(0.082*(273.15+33.2)) = 13.8 ng m-2 s-1 Where 0.18 is the chamber height in m (Volume/Surface), 14 is the nitrogen molecular weight (g/mol), 1000 is a conversion coefficient; 0.082 is R (cm3 atm mole-1 K-1), 33.2 is the temperature inside the chamber in °C."

In our opinion, a plot of the temporal variation of concentrations would not help, because it will only show increases and decreases of the concentration over time, corresponding to the increase of the concentration inside the chamber when it is closed, and the decrease when the chamber is open. No routine concentration measurements were made, due to the lack of power and the use of a generator on the field (far from the analyzer to avoid pollution). Furthermore, as far as we know, usually NO concentrations are not reported in the literature when NO fluxes are studied.

2) vegetation data: - p.2, line 164: why you use just the data from 2004 to 2008 which equals just about 50% of the overall data?

Because the simulation is performed for these years only. Forcing meteorological data were not sufficiently complete for other years, and vegetation data were not as detailed as in 2004, 2005, 2006, 2007 and 2008.

Added in the text for vegetation data, page 3 end of second paragraph: "Furthermore, vegetation data are more numerous during these years, when the AMMA (African Monsoon Multidisciplinary Analysis) experiment took place in West Africa."

And for meteorological data page 3 third paragraph: "These data were quality checked and gap filled for the years 2004 to 2008 only". Meteorological data and a part of vegetation data are available in the AMMA data base: http://bd.amma-catch.org/amma-catch2/main.jsf. Other data are progressively integrated in this data base.

3) modelling section: In general without any public available code or equations used in the model, it is not possible for other scientists to reproduce your results and apply the model for other studies. The equations are given in 3 documents: STEP equations are detailed in Mougin et al. (1995), and updated in Jarlan et al. (2008), GENDEC equations are given in Moorhead & Reynolds (1991), and NO flux equations are given in Delon et al. (2007), updated in Delon et al. (2008). I understand that if the code is not available publicly, it is rather difficult to extract equations from the publications and use them to try to reproduce the results. However, if people are interested in using this code, they may have the possibility to ask for it in the frame of collaboration between research institutes.

- p.3, line 227: why did you use this version and not the previous one? What is the improvement?

P3 line 227 the text "Now, the main difference between the previous version and the study presented here, is that the N availability in the soil is calculated from buried litter (vegetation and feces) decomposition and is no more prescribed." Has been changed page 4 third paragraph: "In the new version, the N availability in the soil is calculated from buried litter (vegetation and feces) decomposition and varies in time, thanks to the coupling with the other models which provide vegetation and organic matter information in a dynamic way. The mineral N used as an input to the NO flux calculation is therefore more realistic than in the previous version where it was prescribed without any link with vegetation".

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p.3, line 268: again, why did you exclude data?

The meteorological data are available from 2002 to 2010, but were quality checked and gap filled for the years 2004 to 2008 only. Furthermore, vegetation data are more numerous during these years, when the AMMA (African Monsoon Multidisciplinary Analysis) experiment took place in West Africa. This was already added above.

p.4, line 264: The simulation of soil temperature from air temperature is highly critical for different soil types. What kind of soil properties did you use? I am wondering how well your simulation will perform in comparison to a q-10 value of approx. 2?

Added in the description of STEP, page 4 end of second column:

"The soil temperature is calculated following Parton et al. (1984), reporting a simplified soil temperature model in a short grass steppe. This model requires daily max and min air temperature, global radiation (provided by forcing data), plant biomass (provided by the model), initial soil temperature, and soil thermal diffusivity. Thermal diffusivity (cm2 s-1) is the ratio between thermal conductivity (W m-2 K-1) and volumetric heat capacity (1500000 J m-3 K-1). Thermal conductivity for each layer i of the soil is Cond(i)= -9.77 + 12.19*(soil_humidity(i)**0.0528)."

Simulated temperatures are compared to measured temperatures, which gives good results as shown in figure 3. Until now, this model is supposed to be used in semi arid regions, where soil properties are comparable to the ones of Agoufou (sandy soils are main soil type in the Sahel). If the model were to be used for other types of soils, the soil temperature would be of course tested against field measurements before use.

Concerning the comparison to a Q10 value of 2, if the temperature increases by 10°C, the effect on NO flux is rather limited. As shown in Figure 9, when T is forced from 33°C to 48°C, NO emission decrease. Indeed, increasing soil temperatures above a certain threshold, especially in tropical regions, show no strong effect on NO emission. As already mentioned in the text (in the "soil temperature" subsection, "Results and discussion" section), the effect of increasing temperatures in tropical ecosystems is not clear, and may increase or decrease emissions. Furthermore, this effect is not linear during the wet season, and is completely masked by soil moisture effect. However, day to day fluctuations of NO emissions are linked to temperature variation. In GENDEC, Q10=3 in the range 0-30°C for the rate of decomposition.

p. 4, line 318: [: : :] which aim is to examine the interactions between litter, decomposer microorganisms, [: : :]. It is highly surprising that you apply a model for decomposer microorganisms to study the release of NO. According to my knowledge out of the heterotrophic microorganisms which are involved in decomposition of C are only denitrifiers. Due to the lack of organic carbon and soil moisture these microorganisms should be of low abundance in this soils. Nitrifiers are usually autotrophic and use CO2 instead of organic carbon.

Indeed, the GENDEC model needs a more suitable description concerning the functioning of this semi arid system where nitrification is the dominant process for NO production in the soil and NO release to the atmosphere.

GENDEC paragraph becomes (section 3.3, page 5): "GENDEC (for GENeral DE-Composition) is a general, synthetic model, which aim is to examine the interactions between litter, decomposer microorganisms, and C and N pools, and to explore the mechanisms of decomposition in arid ecosystems. The decomposition of buried litter by micro organisms is the first step of the GENDEC model, giving access to the mineral C and N pools of the ecosystem. GENDEC has been specifically developed to reproduce these processes in semi arid ecosystems, where inputs of organic matter and soil moisture are low. The ultimate step, C and N mineralization, is fed by (1) decomposition of organic matter, and (2) growth, respiration and death of microbes (microbial dynamics). The general modelling approach is based on fundamental decomposition processes. Six pools of C and N are used in this model, representing dead organic matter (labile materials with high N content and rapid decomposition rate, celullose and related materials with an intermediate decomposition rate and very little associated N,

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very slowly decomposing recalcitrant compounds with moderate levels of physically associated N, and dead microbiota with high N content and rapid decomposition), living microbial biomass (final pool of organic matter), and soil N for the nitrogen submodel (Moorhead and Reynolds, 1991). C/N ratio is different for each of these compartments, and is set to 10, 1000, 34, 8, 25 and 9 respectively for labile compounds, holocellulose, resistant compounds, dead microbial biomass, living microbial biomass and nitrogen pools, based on Moorhead and Reynolds (1991) and experimental results from the Agoufou site detailed below. Flows between these pools are driven by empirical relationships according to characteristics of the microbial community. Climatic parameters such as soil moisture and soil temperature are important drivers for C and N dynamics. The model describes the processes underlying the interactions between C substrate, principal decomposers and nutrients that ultimately result in mineralization. Decomposition and microbial metabolic rates increase with increasing moisture availability, (at least until saturation leads to anaerobic conditions) and with increasing temperature (at least at temperature below 30-40C). The dynamics of the soil N pool gives the net mineralization. The model emphasizes the association between C and N dynamics and microbial processes. Wetting drying events increase the turnover of microbial processes, stimulate C mineralization, and involve a short term carbon dynamics since soil organic matter and nitrogen content are low. Microbial growth and respiration are function of total carbon available, i.e. total C losses from the litter. Mineral N used to calculate NO release to the atmosphere is directly linked to mineral C used to calculate the respiration of microbes (i.e. CO_2 release). GENDEC is driven by organic matter input coming from four different boxes in STEP: buried litter (herbs and tree leaves), trees, fecal matter, and dry herb roots. It is also driven by soil temperature and soil water potential calculated in STEP. Input parameters include the assimilation efficiency and the microbial mortality rate (see table 1). At last, mineral nitrogen, total quantities of C and N, respiration are obtained for each box (buried herbaceous litter, buried leaf trees, dry roots and fecal matter). The addition of these four contributions gives access to the total C and N in the soil. Organic carbon is assumed to be the sole source of energy and substrate for heterotrophic microbial growth. Organic matter mineralization driven by heterotrophic activity of soil microorganisms releases mineral nitrogen. This is the starting point for the calculation of nitrogen transformations in soils (Blagodatsky et al., 2011). The mineral nitrogen is then used as an input in the NOFlux model described below."

p. 4, line 372: Please include equations/ description of emission algorithm

OK. Equations have been included section 3.4 page 6.

p. 5, line: 394: "Furthermore, the Water Filled Pore Space (WFPS) remains below 20% (soil moisture below 10%), [: : :]. Please clearify gravimetric or volumetric soil moisture.

Volumetric soil moisture

How fits the assumption about predominant nitrification to aerobic denitrification and N2O production under low soil moisture?

Aerobic denitrification or co-respiration is the simultaneous use of both oxygen (O2) and nitrate (NO3-) as oxidizing agents, performed by various genera of microorganisms. This process differs from anaerobic denitrification not only in its insensitivity to the presence of oxygen, but also in that it has a higher potential to create nitrous oxide. This question is surely very interesting, but I have to admit that my knowledge on the subject is rather limited. After checking the literature, I am wondering if aerobic denitrification occurs in natural soils, or if it is produced by bacteria specially included to get rid of nitrates in wasted soils. In our study we did not identify the bacteria in the soils, and I would not be able to know if this process is really occurring. Biogeochemical models usually do not include this process as a possible provider of NO and/or N2O. After reading several contributions on this subject, I could not isolate any modelling study. Furthermore, if you are really talking about N2O, this stays beyond the scope of this study as we simulate NO.

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p.5, line 409: What is the reason that the output of mineral N equals 0? I recommend rather to indicate the modelled data points which are based on a mineral N of 0 then applying 0.01 as a first guess.

When soil moisture is too low, microbial respiration is blocked in the model, microbial dynamics is frozen, and mineralization is stopped. Sentence has been corrected end of first column page 6.

p. 5, line 439: gravimetric or volumetric?

Volumetric

For a future validation, the field measurements for NO should not just cover two short periods and instead performed every month over the whole period.

We totally agree on this point. The site of Agoufou is 12h drive from Bamako (close to the small city of Hombori), accessible only by 4 wheel drive, without power supply, and with hot and sandy weather conditions. Leaving electronic device on the field for a long time is just not possible. Furthermore, this part of the world is now unreachable and forbidden to foreigners. You may ask why we have chosen this site for this simulation? Actually, the available data set on such a remote site is unique, in terms of fluxes, meteorological and vegetation parameters (see Mougin et al., 2009). In our opinion, the sahelian region is very interesting in terms of N cycle, due to the drastic changes in soil moisture and the N turnover far from anthropogenic influence.

4) Figures: - Fig. 2: It is not appropriate to compare the absolute value of 5 cm depth with an average of 0-2 cm. Furthermore, the goodness of fit criteria of the comparison are missing. Fig. 3: See previous comment

Indeed comparing 5cm depth with 0-2cm is not exact. It is in general quite difficult to have in-situ soil moisture measurements in the very first soil centimetres especially over sandy soils (for example out of more than 40 networks in the international soil moisture network database, available at https://ismn.geo.tuwien.ac.at/, only 5 provide

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data at shallower depth than 5cm). The main focus here is on the soil moisture temporal response which, as discussed in the paper, is well reproduced by STEP. Goodness of fit criteria are already given in the text (R2 = 0.69). We agree that, in addition to the SM threshold fixed by the field capacity in the tipping bucket approach which is already discussed in the manuscript, the higher SM peaks observed in the measurements as compared to STEP may be also due to the deeper soil depth at which the measurements are taken. A sentence has been added to explain this page 7 first paragraph.

5) Minor corrections: - Please spend some time to go over the general submission section in BG before re-submission!!! E.g. the citation in BG should be (Author et al., Year) and not Author et al. (Year).

The guideline for authors in BG mentions that: "In general, in-text citations can be displayed as "[. . .] Smith (2009) [. . .]", or "[. . .] (Smith, 2009) [. . .]". The use of latex does not give the same result if a same reference is given as \citet{ref} or \citep{ref}.

p. 1, line 40: These compounds come from the mineralization [: : :] correct to: In natural soils these compounds come from the mineralization [: : :] to indicate that in agricultural soils the major source of these compounds is due to fertilization.

OK, corrected.

p. 5, line 447: " [: : :] fluxes of emission.": I don't understand this English. - p. 2, line 111: emission of NO fluxes (NO emission)? I don't understand this English.

OK, Corrected throughout the text. Module has been replaced by model throughout the text.

Referee #2

1) It is difficult to separate the details from what is genuinely new in this paper. Is the only new thing a coupled model? Or are there insights into the behavior of the system that emerge from the coupled model that were not possible without the coupling? The authors should make a clearer statement in their introduction and conclusion about

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what the state of the art was prior to this work and what the state of the art is as a result of the paper.

The main innovation lies in the specificity of the region (semi arid) for which the coupling model is developed. Indeed we have tried to highlight in the introduction that pulse emissions in semi arid regions release non negligible quantities of NO to the atmosphere, leading to ozone formation. Added in the introduction: "Modelling results are compared to data collected in the northern Mali site of Agoufou for years 2004 to 2008. This modelling tool has been developed for semi arid regions where specific processes such as pulses need to be taken into account. Indeed, pulses are usually underestimated by global scale modelling, and the specificity of a model developed for semi arid regions helps to provide magnitudes of NO fluxes. Furthermore, the Sahel region is a large region grazed by domestic cattle, and the role of animals in biomass management, as it is included in our modelling approach, is seldom highlighted in regional or global models."

Added in the conclusion: "The coupling between the three models is successfull, and well adapted to the specific functionning of semi arid ecosystems, where mechanistic models have usually not been tested. The biomass management in the Sahel is also driven by the presence of livestock, which provides feacal biomass and buries surface litter by trampling. The existence of this coupled model provides a new insight in the representation of biogenic NO release to the atmosphere in semi arid regions, where processes of emissions are usually adapted from temperate regions, and not specifically designed for semi arid ecosystems"

2) The paper should be copy edited to improve the English. It is difficult to read in places because of the awkward use of language.

OK. Corrections and simplifications have been made throughout the paper.

3) Section 2. The paper should not be published without some form of access to the experimental data for other researchers.

Mentioned end of first column page 3. Meteorological data and a part of vegetation data are available in the AMMA data base: http://bd.amma-catch.org/amma-catch2/main.jsf. Other data are progressively integrated in this data base. The authors can be contacted for more information.

4) Section 2. Additional details on the NO flux measurements should be presented. Were the chambers open during rain, the variability between nearby sites should be described.

Some details on the experimental method have been added in the text, concerning NO fluxes measurements section 2.2 page 3. The measurements were done manually. Chambers are open every 12 minutes, during approximately 3 minutes, allowing the analyzer to measure ambient concentrations. The chambers were open during rain, since measurements were done after rainfall events. The variability between nearby sites has been added, as follows:

"Several different places at the site of Agoufou have been sampled. In June-July 2004, 180 fluxes were sampled. The chambers were placed on the soil, 90 with short vegetation inside, 90 over bare soil. In August 2005, 70 fluxes were sampled, mostly over vegetation, the whole site being covered by vegetation in the core of the wet season. Fluxes were sampled every day between June 30th and July 13th 2004 and between August 11th and 13th 2005, in the morning and in the afternoon, and daily means were calculated and plotted in Fig. 6b. Standard deviations are indicated on the plot, showing the spatial heterogeneity of fluxes at the same site.

5) Section 3. The description of the model should include enough numerical detail that another researcher could check code written independently. Perhaps a table of example input and output.

As the three individual models are published, we did not detail equations for each. We propose to add a new table with inputs needed by each model. It would be tiresome to mention all outputs for each model. Outputs specifically needed for the simulation are

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already mentioned in the text. The table is given in supplement.

6) It would be useful to have a discussion of the appropriate horizontal spatial scale for use of this model.

Added in the “limitations and uncertainties” section 5 page 9: “While the STEP model was initially designed for 1D-simulations in well documented study sites, it has also been recently used at the regional scale in the Sahelian belt (12°N-20°N; 20°W-35°E) by Pierre et al. (2011, 2012), to estimate the amount of dust emissions in that region.. The NO flux model has also been applied in the region of Niamey, Niger (Delon et al., 2008), to reproduce NO pulses at the beginning of the wet season, and their impact on ozone formation during the AMMA field campaign in 2006. Furthermore, it has been used at the regional scale in the Sahel (Delon et al., 2010) and in West Africa (Delon et al., 2012) to calculate NO release to the atmosphere. Concerning the GENDEC model, it has been successfully applied for situations very different from those upon which it was based (Moorhead & Reynolds, 1993; Moorhead et al., 1996). In other words, we can seriously consider using this coupled STEP-GENDEC-NOflux model in the Sahelian belt by making approximations, concerning for example biomass, livestock, N and C pools. Considering the need of information in this region of the world, it would be conceivable to simulate such processes of emissions at a larger scale. The challenge is worth to be done, knowing that NO emissions participate at a larger scale to the production of tropospheric ozone.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C5760/2014/bgd-11-C5760-2014-supplement.pdf

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STEP	Inputs	Unit	Value
Initial parameters	Conversion efficiency	g(d.m.) MJ^{-1}	4
	Initial green biomass	g(d.m.) m^{-2}	0.8
	% C3	%	29.3
	Initial Specific Leaf Area	cm^2 g^{-1}	180
Meteorology	Precipitations	mm	Daily variation
	Global radiation	MJ m^{-2}	Daily variation
	Min and max temperatures	°C	Daily variation
	Pressure	hPa	Daily variation
	Wind speed	m s^{-1}	Daily variation
Soil	Thickness of 4 layers	cm	2 ;28 ;70 ;200
	Initial water stock (4)	mm	0.1 ;1.5 ;7.3 ;38
	Clay content (4)		4.5 ;5.5 ;5.2 ;5.5
	Sand content (4)		91.2 ;91.3 ;91 ;92.3
	pH (4)		6.7 ;6.7 ;6.7 ;6.7
Annual Vegetation	Initial dry biomass and litter	g(d.m.) m^{-2}	10;30
	Root fraction (3)		0.75;0.2;0.05
	Dicotyledons %	%	29.5
	Max tree foliage biomass (year before and current year)	kg ha^{-1}	600;400
Animals	Animal categories (cattle, goat, sheep, donkey, camel, horse)	%	Monthly variation. Ex for January: 0.826;0.091;0.055;0.024;0.001;0
	Animal stock (12 months)	Head number	2893;5288;15626;22537; 13874;7832;1191;408; 3168;2835;2510;3348
	Grazing area	ha	5000
GENDEC	Inputs	Unit	Value
	Soil temperature	°C	From STEP
	Matrix Potential	MPa	From STEP
	Microbial assimilation efficiency		0.6
	Carbon pool	gC	From STEP (depends on biomass quantity)
	Microbial death rate		0.2
	N/C (6: labile compounds, holocellulose, resistant compounds, dead and living microbial biomass, nitrogen pools)		10;1000;34;8;25;9
NOFlux	Inputs	Unit	Value
	Surface WFPS	%	From STEP soil moisture
	Surface soil temperature	°C	From STEP
	Deep soil temperature (0-30cm)	°C	From STEP

Fig. 1.

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