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Biogeosciences Discussions

Interactive comment on “Nitrogen compounds emission and deposition in West African ecosystems: comparison between wet and dry savanna” *by* C. Delon et al.

Anonymous Referee #2

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General comments

A long-term (6 years) time series of biosphere/atmosphere reactive nitrogen (Nr) fluxes is presented for West African wet and dry savanna sites. The quantities measured are NH4+ and NO3- in rain, and atmospheric gaseous inorganic Nr concentrations (NH3, NO2, HNO3). Dry deposition is calculated by inferential modelling (aerosol and organic N deposition not accounted for), while emissions of NH3 and NO are also modeled. There were no in-situ surface/atmosphere flux measurements to validate either NH3 or NO modeled fluxes. The N budgets presented are very uncertain and the data are therefore somewhat over-interpreted, bearing in mind that so much is based purely on

model simulations (eg biogenic NO emission), or very crude assumptions (eg a flat emission factor of 30% for NH3 from organic fertilisation).

Nevertheless, the long-term monitoring effort and its tentative interpretation towards a better understanding of the N cycle in the W African region are well worthwhile. The value of the measured datasets (concentrations in air and rainfall) is twofold; i) the long-term time series allows the study of seasonal and possibly inter-annual cycles that are clearly linked to meteorology, vegetative cycles and biomass burning; ii) the climatic (latitudinal) gradient shows contrasting behaviour of dry and wet savanna. However, key messages are difficult to extract, the structure of the paper is a little confusing, with results being shown in the materials and methods section, while this section actually devotes very little space to the description of sites, measurement protocols and models used. Even if the ‘present paper is a continuation of Delon et al. (2010)’ it would be useful to provide a little more background information in an M&M section, while shifting the actual results to a different section.

Concerning dry deposition, the inferential model that was used is based on the premise that the deposition velocity / canopy resistance are independent of concentration, which is neither true for NH3 nor NOx, which are regulated by compensation points. I noticed in Adon et al (2010) that NH3 concentrations were high, in the range 3-10 ppb on average, which is comparable to levels observed in very intensive agricultural areas of Europe. It may well be that, rather than leading to high dry deposition fluxes to the ecosytem (NH3 from exogenous sources), the high concentrations in fact result from high emissions by soil, vegetation, as well as dung and urine (endogenous sources). Thus the error bars attributed to NH3 dry deposition (Fig. 4) are undoubtedly much too small. (The same argument applies to some extent to NO2, which is oxidised in or above the canopy from the soil-emitted NO, and is captured by the passive sampler). Basically, there is no way to tell whether the ecosystem is a net source or sink until some sort of validation using actual flux measurements is done. Since this was clearly outside the scope of the study, this paper should make it clear that the uncertainties associated with dry deposition are much larger than currently shown. I hold the same to be true for biogenic NO emissions as predicted by the model, unless there have been flux measurements in the past at some of these sites, that are not mentioned in the current manuscript.

Specific comments

p7225, section 2.1: add Table summarising the main characteristics of the 5 sites: annual T, P; maximum grazing density; min-max LAI; main vegetation species; etc

p7230, l15-16: there are of course also very large seasonal changes in vegetation in European (temperate) conditions, which I don't see as being any less drastic than at the savanna sites. Annual crops (wheat, potatoes, maize, etc) grow from a leaf area index of 0 to 4-6 m2/m2 within a few months, before harvest, from canopy height z=0 to z=1-3 m, etc. Deciduous trees leaf out in spring and photosynthesize during the summer, before leaves drop in the autumn. Thus stomatal conductance and roughness length, which control dry deposition, thus also undergo large seasonal changes in Europe, not more, not less than in Africa.

p7230, l5-l27: it would be helpful to provide the mean or median Vd for all sites (only the range is provided at present). I would expect large differences in annual mean Vd between wet and dry savanna, though this only reflects the parametrisation used in the model for dry and wet surfaces, rather than actual flux measurements.

p7231, l1-8: as said above in general comments, I would not be so quick to rule out a compensation point, in the foliage and probably more importantly in the leaf litter on the soil surface, since warm and wet conditions favour a rapid decay, mineralisation and turnover of plant material, releasing NH4+ which can either be nitrified, but also be lost to the atmosphere directly by volatilisation. It seems to me that this pathway of NH3 emission is not accounted for in the emission inventory described in Section 2.2.4 (with only dung and urine contributing with a 30% emission factor) ?

p7231, l9-13: this belongs to materials and methods, but much of the text that has come before was results. There should be a clearer split between methods and results.

p7231, l18-19: these uncertainties are certainly much higher

p7231, l20-21: are these wet-only collectors?

p7232, l1-2: dissolved organic nitrogen can contribute a significant fraction (20-30%) of total wet N deposition in Europe; how about Africa?

p7232, l7-8, what is TRMM3B42 for the layman?

p7232, section 3.1: much of this section actually describes how the models work. It is important to draw the line between describing how a model responds to input data, and inferring mechanisms of emission and deposition from actual (flux) obervations, which are not available here. Thus it must be clear that the whole discussion on nitrification/denitrification, soil turnover, biogenic emissions, etc, is a reflection of mechanisms encoded in the model, and is not measurement-based, to avoid the danger of over-interpreting the actual observations.

Technical corrections

Figures: The quality of figures 2, 3 and 5 should be improved by increasing font sizes; the values, axes, legends are barely legible.

Figure 2: in caption, replace 'compounds' by 'fluxes' after 'oxidised N' and 'reduced N'. It would be useful to indicate for each site whether dry or wet savanna. It would also be helpful to show the measured concentrations alongside the simulated fluxes, at least for NH3, which dominates dry deposition, in order to assess how much of the variability in fluxes is due to meteorology (through the model), as opposed to driven by seasonal and interannual variations in concentrations.

Figure 3, caption: indicate 'Total GASEOUS dry deposition flux…'. 'Total' is slightly misleading as aerosols and organics were not included.

Figure 4, legend of (b), change to ‘NO3- wd’ and ‘NH4+ wd’

Figure 5: a pie chart should represent additive quantities, whereas emissions and deposition of Nr have opposite signs. Fig 5 and Fig.6 should be combined, with for each site the total N_dep and N_em fluxes shown as stacked bars of different colors for the different contributions.

Interactive comment on Biogeosciences Discuss., 8, 7221, 2011.