

Interactive comment on “Isotopic identification of global nitrogen hotspots across natural terrestrial ecosystems” by E. Bai et al.

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Reviewer 1's comments: The authors go to some trouble to emphasize that no net discrimination would be expected to take place as a result of transformations within the plant-soil system. I agree. But it might be helpful to readers also to mention that there *is* systematic discrimination in plant N uptake, and to comment on whether or not this would affect soil ^{15}N .

*Authors' response: We agree with this comment and have revised the paper accordingly. We now stated that plant uptake can discriminate against ^{15}N , particularly via mycorrhizal mutualism. Because plants return whatever they take to the soil by litter-fall, discrimination against ^{15}N in plant uptake does not result in a change of soil ^{15}N under steady state conditions. See L107-122.

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Reviewer 1's comments: 1. The analysis implies that the total throughput of N through the land biosphere is on the order of 130 Tg N/yr, and this is in line with many estimates in the literature of the rate of input of N. If I have understood the analysis properly, the main determinant of this total number is a model of symbiotic N_2 fixation that is not particularly well constrained by data (because the data are sparse). Estimates of N deposition and asymbiotic fixation are included as well, but the latter in particular is assumed to be small. I argue that we actually do not know the rate of N_2 fixation, especially asymbiotic N_2 fixation by free-living heterotrophic bacteria, and by free-living and endophytic cyanobacteria. Furthermore, it is not clear to me that the estimated rate is sufficient to support the observed rate of NPP. Presumably any such calculation will depend strongly on the rate of recycling of N in ecosystems. Are there other observations that could constrain the recycling rate? Or at least, what does this analysis imply about N recycling rates, and is it plausible?

*Authors' response: The N fixation input was based on CASA-CNP model for the non-crop area. We agree that fixation rate is still poorly constrained and this has increased the uncertainty of our results. Since the fixation rate was comparable to the estimation by (Cleveland et al., 1999) which was based on empirical data, and the total input was within the range of previous estimates as the reviewer mentioned, we believe it is reasonable to use CASA-CNP model as an input. This kind of multiple-model approach is common for global-scale biogeochemical cycles due to the complex processes and the large spatial and temporal resolution of such inquiry (Charria et al., 2008; Schaldac and Pries, 2008; Thornton et al., 2009).

The estimated total N input of 129 Tg C year⁻¹ does not include crop and pasture regions. If N fixation from crop and pasture is included, the global total of N fixation will be much higher. The N fixation estimates are taken from (Wang and Houlton, 2009) who have used the global estimates of N inputs in estimating the land sink potential under present and future climate conditions (see Wang and Houlton GRL, 2009. VOL. 36, L24403, doi:10.1029/2009GL041009).

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Reviewer 1's comments: 2. The partitioning of N losses in denitrification to N₂O versus N₂ is extremely uncertain as it depends sensitively on the modelling of water-filled pore space. As much gaseous emission is thought to take place episodically in association with rainfall events, it is quite possible that the effective soil wetness has been underestimated. And a small underestimation towards the "wet end" of the WFPS scale could lead to a large underestimation of the proportion of N lost as N₂.

*Authors' response: We agree that the effective soil wetness may be underestimated. For the same amount of rainfall, different precipitation pattern (e.g. long light rain vs. short heavy rain) may cause different partitioning of N loss between N₂O and N₂. Because of the monthly time step in the modeling, we cannot assess the sensitivity of N gaseous loss to different temporal variation of rainfall. However the close agreement between our estimated NO_x fluxes with the satellite-derived NO₂ concentrations suggested that our estimates are quite reasonable.

Reviewer 1's comments: 3. Elsewhere, (some of) the authors have written about a concept of "underexpression" of the soil isotopic enrichment effect due to gaseous losses. According to this concept, "DEN should be of smaller magnitude in wetter environments, approaching zero in the wettest tropical forests. Variation in "DEN was invoked as an explanation for the widely observed trend towards more negative plant and soil ¹⁵N values in wetter environments. Several papers showing such a trend are cited in the present manuscript. I am not advocating this concept. But if the authors really have abandoned it as it appears, then (a) they should say so, and why; and (b) they should indicate how they now explain the observed trends in ¹⁵N along precipitation gradients. I think, from Fig. 7 especially, that they simply attribute the largest (fractional) gaseous losses to dry environments, but this is surely inconsistent with their earlier publications.

*Authors' response: We agree that underexpression of the soil isotopic enrichment was observed in extreme conditions. In our previous publications, only when the annual precipitation was very high (e.g. MAP > 4 m), underexpression was considered. At global scale, this kind of condition is rare. We have added a sentence to explain why

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underexpression was not considered in the model (L190-192).

Reviewer 2's comments: In this MS the authors apply the N-isotope model developed in Houlton and Bai (2009) at a global scale to estimate global NO, N₂O, and N₂ emissions from unmanaged soils (and the regional distribution of these emissions). This provides a useful comparison with other methods for estimating emissions from denitrification. Although the paper is generally well-written, a little more care needs to be taken in the initial explanation of the model. A section giving a general overview of the model before the detailed explanations of the individual components would be helpful. Also there are several places where symbols are not explained, or where the symbols used change. Finally a table of emissions by region should be included in the main paper. Specific comments:

Reviewer 2's comments: Structurally it would be nice to have an overview of how the model works at the beginning of section 2, before the detailed descriptions of each component.

*Authors' response: The general overview of the model was in the Introduction section. We have moved it to section 2 to make it more logical (L92-100).

Reviewer 2's comments: For the N isotope model it is important to emphasise that the equations represent long-term equilibrium values. For example, equation (2) does not imply that a change in the delta-N of the inputs will immediately alter the soil delta-N ratio, rather that this would be the long-term equilibrium delta-N level of the soil.

*Authors' response: We have included an explanation of this point in the revised manuscript (L205-208).

Reviewer 2's comments: The diagram of the model (Figure 2) does not show ammonia losses. If you are using equation 6 as the "Soil N isotope model" then it is actually splitting the N inputs into "denitrification N losses" and "Hydrologic and ammonia volatilization N losses". If another equation is used to discriminate between leaching

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and ammonia volatilization losses then this should be explained in the text and model diagram.

*Authors' response: Thanks for pointing this out. Ammonia losses should be included in the model. We have revised Figure 2 to reflect this N loss flux. Because ammonia volatilization becomes a significant pathway for N loss only when soil pH is high (>8), and the total area of unmanaged system with such high pH is quite small globally, therefore we have not explicitly estimate the ammonia loss. This is now stated in the text (see L177-178).

Reviewer 2's comments: In the description of the enrichment factor it is not explained what the ratio 14k/15k represents.

*Authors' response: We have added an explanation of this point in the revised manuscript (L134).

Reviewer 2's comments: Section 2.6: In this section the symbol ϵ_{den} is used whereas in the previous sections it was ϵ_G

*Authors' response: Thanks for pointing this out. We have changed ϵ_{den} to ϵ_G in the revised manuscript.

Reviewer 2's comments: It would be useful if you stated the total N input amount used in your model. Particularly given that in section 3.1 you quote the global N loss due to denitrification as a fraction of the N input.

*Authors' response: We have added the total N input amount (129 Tg N yr⁻¹) in the revised manuscript (L219).

Reviewer 2's comments: Supplementary Table 2 is useful for comparing your model result with previous studies, but is not so useful for summarising your findings. For example, the global total is not simply the sum of all the listed Biome types or regions. It would be nice to have tables that listed the total estimated N losses (including leaching and volatilization) by biome and geographic region (for all biomes and regions so that

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the sum equals the global total).

*Authors' response: We have added a table summarizing regional modeled results and the corresponding texts in the revised manuscript (Table 2).

Reviewer 2's comments: Page 12128: For the global N₂O flux N₂O emissions from fossil fuel burning and industrial processes should also be considered.

*Authors' response: We agree with this. We have stated clearly that we only estimate soil sourced N₂O in the revised manuscript (L424, L430).

Reviewer 2's comments: Page 12129, line 4: this should be 77% of the global natural (rather than total global) emissions?

*Authors' response: This has been corrected in the revised manuscript (L434). Technical corrections

Reviewer 2's comments: Pg 12116, line 25: should be "partition" rather than "partitioning".

*Authors' response: This has been corrected in the revised manuscript (L81).

Reviewer 2's comments: * Page 12122: The last sentence of the first paragraph needs to be clarified.

Authors' response: This has been corrected in the revised manuscript (L249-250).

Reviewer 2's comments: Pg 12124, line 16: the upper end of the confidence interval should be $\mu_T + \sigma_T$ (not $\mu_T - \sigma_T$)

*Authors' response: This has been corrected in the revised manuscript (L312-313).

Reviewer 2's comments: Figure 2: There are two N output boxes labelled N₂ and no N₂O box.

*Authors' response: This has been corrected in the revised manuscript (Fig. 2).

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Reviewer 2's comments: Pg 12131, line 5: the double negative "less unlikely" is confusing and should be revised.

*Authors' response: This has been corrected in the revised manuscript (L485).

*Authors' response: Please see the supplement for the revised manuscript.

References: Charria, G., Dadou, I., Llido, J., Dréville, M., and Garçon, V.: Importance of dissolved organic nitrogen in the north Atlantic Ocean in sustaining primary production: a 3-D modelling approach, *Biogeosciences*, 5, 1437-1455, 2008.

Cleveland, C. C., Townsend, A. R., Schimel, D. S., Fisher, H., Howarth, R. W., Hedin, L. O., Perakis, S. S., Latty, E. F., Von Fischer, J. C., Elseroad, A., and Wasson, M. F.: Global patterns of terrestrial biological nitrogen (N₂) fixation in natural ecosystems, *Global Biogeochemical Cycles*, 13, 623–645, 1999.

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*Please see the supplement for the revised manuscript.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/8/C6376/2012/bgd-8-C6376-2012-supplement.pdf>

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Interactive comment on *Biogeosciences Discuss.*, 8, 12113, 2011.

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