Biogeosciences Discuss., 8, C5010–C5012, 2011
www.biogeosciences-discuss.net/8/C5010/2011/

Biogeosciences
Discussions

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

I. C. Prentice (Referee)

colin.prentice@mq.edu.au

Received and published: 23 December 2011

I enjoyed reading this MS. I am pleased to see a global analysis of a key aspect of the N cycle based on isotopic constraints, and it's good to see the global distribution of δ15N put to good use. The assumptions that are made about which pathways discriminate against 15N, and which don't, is well supported by data and implies that the analysis of δ15N data ought to be in principle straightforward, and much more widely used than it is today. The conclusion that the tropics are hotspots of N cycling is not new, but it is good to see it arrived at from a top-down approach. The idea that leaching is the dominant pathway of N loss in mid- to high latitudes, as opposed to gaseous losses in the tropics, is consistent with other evidence and modelling. In fact one could turn the argument on its head and say that this modelling provides a plausible and quantitative

explanation for the latitudinal gradient of δ15N.

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 δ15N.

The evaluation of inferred NO fluxes against satellite-derived NO2 concentrations is another example where I am delighted to see a constructive use of this hugely under-utilized resource.

The authors do a good job of noting and analysing the impact of "uncertainties" (of which there are always plenty in global top-down studies). However, I am more concerned about possible larger errors that could be present in the analysis, and should be flaggedâĂŤnot so much "uncertainties", as "enigmas" or "mysteries"! Alternatively, I will be happy if the authors can explain away my concerns. . .

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 N2 fixation that is not particularly well contrained 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 N2 fixation, especially asymbiotic N2 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 conatrain the recycling rate? Or at least, what does this analysis imply about N recycling rates, and is it plausible?

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.

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, εDEN should be of smaller magnitude in wetter environments, approaching zero in the wettest tropical forests. Variation in εDEN was invoked as an explanation for the widely observed trend towards more negative plant and soil δ15N 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 δ15N 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.

Note: Fig. 2 has two boxes labelled N2. One of them should be N2O.

Colin Prentice 23 December 2011

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