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Biogeosciences Discussions

Interactive comment on "Nitrogen input ^{15}N-signatures are reflected in plant ^{15}N natural abundances of sub-tropical forests in China" *by* Geshere Abdisa Gurmesa et al.

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General comments

This paper discusses two processes that affect the delta 15N of forests. Firstly the mixing with N deposition with a different 15N signature than the forest itself and secondly the fractionation of the 15N through different N transformation processes followed by the loss of the lighter fraction resulting in enrichment of the remaining N with 15N. The latter process is thought to happen more strongly if N availability is larger and so it is thought that a higher delta 15N is an indication of a higher N availability.

The authors present two sets of delta 15N results of two forests in southern China:

the results of the ambient situation ('control') and the results of a long term N addition experiment in the same forests. In the control experiment they find rather low delta 15N values compared to literature values. As the delta 15N value of the substantial N deposition is also rather low, they conclude that the mixing with N deposition with a low delta 15N is the dominating process in determining the 15N of the vegetation and fractionation combined with loss is not important. Secondly they discuss the effects of a long term addition of N with a higher delta 15N than that of the ambient deposition. Here they conclude that the increase in delta 15N in the vegetation is not the result of increased fractionation and loss, due to the higher N availability but that this is the result of the mixing of the added N with a high delta. Their general conclusion is that when delta 15N of forests is used to say something about N availability more attention should be given to the possible influence the delta 15N signature of the N deposition can have.

Although I do not think that the main conclusions of the authors are incorrect, I think their argument does need substantial improvement.

1. In the first place they do not make clear what delta 15N value they do expect for their forest (under ambient conditions) as a result of fractionation and loss, as described in their hypothesis i (line 101). This hypothesis i is unclear (see below) and they do not explicitly compare this hypothesis with their results. In the Discussion section they compare their results not with individual forests from the literature but with large datasets synthesized from many different forests. Why would their conclusions about their own forests not be true for the forests they cite from the literature? If not, what could be the relevant differences between their forests and those from the literature? Maybe the values calculated for southern China by Amundson et al (2003), based on MAP and MAT can help to structure this part of the discussion? 2. At first sight it seems reasonable to consider mixing to be important in the control experiment, but this could be supported with some calculations of the effect of mixing. It seems the authors have carried out such calculations at least for some cases according to their statement in line 440, but

this would be useful for this case as well. 3. The reasoning in lines 339-356 is very difficult to follow. I will make more specific comments below.

4. Concerning the N addition experiment it can be said that both the mixing process and the increased fractionation plus loss process (expected as a result of larger N availability) would lead to an increase in the delta 15N of the vegetation, so it is unclear why the authors choose that the increased delta 15N values found in the vegetation were the result of mixing and not of increased fractionation plus loss. What result of the experiment and the measurements would have led them to the other conclusion? In fact probably both mixing and fractionation plus loss contribute to some extent to the increase of delta 15N in the vegetation. Again some calculations of the mixing of the deposition might give more insight into the potential contribution of this process.

5. I think there is something wrong with the statistical results presented in Tables 3 and 4. The tests for siginifant differences sometimes yield significant p values while the difference tested is smaller than the sum of the two standard errors. This cannot be correct. I suggest the authors provide the data and the script they have used to calculate the statistics so it becomes clear what they have done. See for example in Table 3 twigs difference between BF and PF is 0.29, while the sum of the SEs is 0.96 and $p<0.01$ and in Table 4 tree leaf in BF difference between control and N addition is 0.6, while the sum of the SEs is 1.1 and $p<0.01$. I assume two-sided tests were carried out although this was not mentioned.

6. I would suggest that the authors should be more careful in using the terms 15N enriched and 15N depleted and define what exactly is meant by them and relative to what (below or above zero, or relative to the delta of some other pool or flux). They use these terms many times throughout the text. See e.g. my comment below on line 402. In line 32 even the term “more enriched” is used.

Specific comments

L25 “examined the measurement”: this suggests the paper is about measurement

techniques. I suggest to rephrase this.

L31 “leafs” the text contains many spelling errors; I suggest the authors check the text throughout for these.

L31: “old-growth forest” this forest is everywhere else described as broadleaved forest, so I would suggest to use that term here as well

L48 “recently” I think it is relevant to be more specific, so the reader knows how long this N addition has been going on. In the methods the 1990s are mentioned for DHSBR (L113).

L67 “above the atmospheric standard” I wonder whether for this criterion 0.0 0/00 is the relevant value, as atmospheric N2 is not a direct source of N for a terrestrial ecosystem.

L81-82 “hotspots” If this is meant to be high in N deposition, I would suggest to use the latter term.

L102 The comparison of a high N forest with temperate forests seems inconsequent. What about temperate forests with high N status?

L103 second hypothesis: I wonder which results could lead to the rejection of this hypothesis, given the experimental conditions. The first part of this hypothesis seems not very challenging and the second part is not very specific.

L114 “steep slopes” Amundson et al (2003) have suggested that under these circumstances delta 15N might be lower (see their paragraph [26])

L182 “including a dry period” If the authors mean that there were not any water samples in Dec and Jan because of a lack of precipitation then please state this.

L186-187 A collector with an area of 8000 cm2 seems extremely large. Is this a correct value?

L215 “plant species as a random factor” Apparently this is not the case for the pine

forest, which contains only one dominant species (L159).

L233 Table 2 At first sight it looks like leaching losses have lower deltas than the deposition, indicating the occurrence of fractionation and loss of N with low deltas, thus increasing the 15N content of the remaining N. However, as deposition is dominated by NH4, while leaching is dominated by NO3, this is not the case. Calculating a weighted average delta 15N for all chemical species in all fluxes may show this. This can support the argument that fractionation plus loss is not evident from this budget, although it is of course incomplete. Are there not any values for the added N plots?

L233 Table 2 In the text it is stated that runoff was measured only in one plot per treatment (line 192), so how can there be an average of three measurments for runoff here?

L251 Fig.1 Are these samples that were taken monthly between Sept and Feb (4 samples) with 3 replicates, in total 12? I suggest to explain this in the caption. Again how was this for runoff (see my previous remark on Table 2)? What could be the cause for the variation found? Is there no substantial time delay between the moment of deposition and the moment the deposited N reaches the subsoil or the runoff?

L261-263 “N concentration of N pool weighted average plant pools calculated per plot”. The reader is referred to Table 3, but in there are only N concentrations of individual pools.

L295 Fig.2 I suggest to increase the size of the symbols in the legend so the different patterns used are more easily recognized. This is also a problem in the supplement figure.

L307 “decrease as expected” It is true that the delta of the N input into the forest is still lower than the the delta of the soil, but the addition has substantially increased the delta of the total N input, so one might as well expect an increase in the soil delta as a result of this.

L325 “other regions” please specify which regions are meant.

L337 “surprisingly” I suggest the authors clarify what they expect here.

L342-345 This remark on the enrichment factor seems misplaced here, as nowhere else in the paper something is said about the enrichment factor. It is also unclear to me why this would support the previously mentioned hypothesis.

L345 “rejects this hypothesis” Which result precisely makes the authors decide to reject? Do the authors reject the full hypothesis or only mean that the increase in delta 15N simply does not happen? Nothing is said about hypothesis i from the introduction. I would suggest to refer to this hypothesis as well, although it needs to be rephrased, as I mentioned earlier.

L347 “other depleting factors” I think “other” should be removed as the previously mentioned process is an enriching factor.

L348-349 “in other Chinese forests with high N deposition” Why only or especially in Chinese forests? And would this not depend on the delta 15N value of the N deposition? Maybe the authors have the literature in mind they mention in lines 323Â■. If that is the case they should refer explicitly to these results. The authors make a different and more general statement in lines 454-455.

L381 “It was interpreted” I suppose this was done by the references mentioned just before this sentence. To make this clearer to the reader I suggest to change the sentence from passive into active voice.

L393 “in line with our second hypothesis” This can only be true for the first part of this hypothesis

L396-397 “it shows again” I disagree. From these results one could argue as well that it is the result of increased N availability resulting in increased fractionation plus loss of depleted N.

L402 “also after addition of 15N depleted N” In the experiment by the authors the N added was 15N enriched (at least compared to the ambient N deposition).

L440 “calculations based on an isotope mixing model” I would suggest to add some information on how this was calculated and which simplifying assumptions were made in the calculation.

L445 “in humid tropical forests of southern China” why would this be true for all these forests, not just for the forest investigated? Possibly because of the delta 15N value of the deposition there (see line 323âĂŚ)? Then the authors should refer to this. Would the region differ in this respect from other regions in the world?

L447 “further confirmed” see my remark on L396

L452 “more important” this is only the case if the 15N signature of the Ndeposition differs sufficiently from the delta 15N of the ecosystem, and the N deposition is sufficiently large. If that is not the case the mixing probably would not dominate the fractionation plus loss of depleted N.

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