

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2021-11

Anonymous Referee #1

Referee comment on "Cycling and retention of nitrogen in European beech (*Fagus sylvatica* L.) ecosystems under elevated fructification frequency" by Rainer Brumme et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-11-RC1>, 2021

General comments

The study presented here tries to understand the effect of mast years on the nitrogen cycle of forests, especially for European beech. This is an interesting topic and the authors provide good insights into it. There are, however, a number of points that could or should be improved.

Introduction

Can it really be stated so generally that atmospheric deposition reduces litter decomposition? Especially for high C/N litter, it could be the contrary.

At the end of the introduction, instead of starting already with material and methods, it would be better to indicate either research questions or hypotheses or goals.

Material and methods

The N balance for the soil (eq. 1) is not complete: litterfall is missing. Either the equation is for the soil and litterfall should be included, or it could be for the ecosystem and then both litterfall and tree uptake (as internal fluxes) should be excluded. Further, a comment about the (limited) accuracy of N leaching should be made, especially because preferential water flow in the soil is a frequent phenomenon and can markedly bias this kind of estimate. Finally, the production of N₂ by denitrification is not mentioned. Can it really be neglected?

In the second paragraph of this section, the word "replicates" is used several times for multiple samples within one site. These are thus not experimental replicates, only so-called pseudo-replicates. Using the word "replicates" is in my opinion misleading.

Results

The structure of the results could be improved. Specifically, section 3.3 tells about seepage, then retention, then seepage again. Reordering this would improve the readability.

In the results about litterfall, it would be very useful to read something about the LAI of these sites. The results of the ¹⁵N labelling are presented shortly but without a reference to tab. 6, where tracer recovery is given for all sites. The word "recovery" is used both for the recovery rate (ratio relative to the amount of tracer applied) and for recovered labelled N (absolute amount). In most tracer studies, "recovery" is used only in the first sense and it would avoid unnecessary confusion to do the same here. For the second sense "recovered tracer N" would be a good wording.

Discussion

The discussion starts with a paragraph that is actually like an introduction with only short references to the own results. Further, on the contrary, own results are rehearsed quite in detail. In my opinion, the discussion should be more a real integration between previous publications and own results.

The relation between N deposition and frequency of fructification is a really interesting result. It would certainly be more convincing if it could be shown also spatially and not only historically. Several other factors could indeed also contribute to the historical changes, like global warming or changes in silvicultural practices affecting stand structures. As participants to ICP Forests, the author could check if there is also a spatial relationship between N deposition and mast frequency.

An effect of fruits on the retention of leaf litter N is given as an interpretation of correlations seen across the sites of the study. This is a relatively weak evidence for such an effect. Unfortunately, the labelling experiment did not include labelled fruits or even separately labelled seeds and cupules. This would have brought a much stronger integration of the labelling experiment into the whole study. One thing that the authors could still easily improve would be to bring the total C/N of the fruits into the discussion, not only the C/N of seeds and cupules separately. Even if the decomposition may indeed be "spatially and temporarily decoupled", the overall C/N of the fruits would help to demonstrate the potential of the fruits to immobilise N during their decomposition. Out of tab. 4, it seems that this total C/N is around 45.

Style

The language of the submitted contribution is well understandable. To a reviewer using English as a third language, it appears, however, that there are a few errors (see some in the details below). Generally, the style could be improved especially in the discussion. In my opinion, some sentences are rather understatements while other, on the contrary, are too strong (see also details below).

Details

L. 96: why ΔS and not ΔN ?

Eq. 1: as noted above, why no litterfall here? Generally, in a mathematical equation, single-letter symbols should be used, not abbreviations made of several characters. For example, ND could be misunderstood as the product N times D. To avoid this, simply use subscripts to differentiate the N fluxes.

L. 142: "was used" is not clear. Specify "for N_E ".

L. 167: "that", not "which".

L. 193: The results chapter starts with exact numbers as if they would be for a specific site, but which one? Only after checking tab. 3, one can understand that the numbers are averages over sites, and then one misses an indication of the standard deviation.

L. 206: do you mean that pollen is present in the samples, i.e. has been retained by the mesh of the litter traps?

L. 222: "enrichment" of "excess" is a pleonasm. I would recommend not to use the word excess at all because it could be defined as excess over the reference (atmospheric N) or excess over the natural abundance of the pool or flux. In my opinion, the best terminology is still that of Buchmann et al., *Biogeochemistry* 33 (1996): 1-23 and Providoli et al., *Biogeochemistry* 76 (2005): 453-475. This applies also to the word "recovery", as mentioned above.

L. 254: this should be "increase in frequency" or "decrease in periodicity", not a "decrease in frequency", isn't it?

L. 259: the words "seems" and "likely" together make the sentence too weak. Especially as no doubt is mentioned about the historical data.

L. 261 ff.: do not give only an average as if it would be a constant over all sites. Something about the variability is needed.

L. 264: what are "historic values"? Is it what could be estimated from the present MY and NMY values combined with historical mast year frequencies?

L. 281: It is rather the contrary: the fate of the litter determines (over the years) the chemistry of the soil organic matter of the horizons.

L. 290: the word "primarily" suggests a demonstrated cause-to-effect relationship. This is not the case here. This paragraph explains why this could indeed be the case, but the sentence with "primarily" is much too definitive compared to the absence of direct proofs.

L. 305-310: proposing this as a rule out of a comparison between only 2 sites would at least call for a plausible mechanism.

L. 313: the role of P is derived here from a correlation among 7 sites: in my opinion, this is not really "shown", only "suggested". Calculating many correlations tends to give more "significant" ones, don't forget this in the interpretation of the results.

L. 316-317: it is not clear what applies for all 79 sites and what applies for those with acidic soils. (Or have all 79 sites acidic soils?)

L. 322: is not "bioavailability" the subject? Then the verb should be singular, i.e. "restricts".

L. 323-324: I don't understand this sentence.

L. 324-325: this could also be interpreted the other way, that low C/N favours the biological activity because microbes have then enough N to process more C.

L. 333: "suggests" (singular).

L. 335: what is here the "elasticity"?

L. 357: should be "aggregated".

Tab. 3: the legend "Mast yrs / yrs / yrs per mast" is all but obvious to understand (especially if some other abbreviations like MY are used in the same table).

Tab. 6: the two columns with recoveries and values in % suggest that these are directly comparable data. However, this is not the case because the denominator is not the same. These % values are thus misleading.

Fig. 2 and 3: to make the data structure behind these graphs more obvious, it could be added "n = 7" just before "study sites".

Fig. 5: I suggest to make only one box for clay (with several arrows out of it). The terminology "high but insignificant" in the legend is in my opinion quite awkward. The more common word "tendency" may be better.