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Reply on RC1

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Author 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-AC1, 2021

General comments

RC1: 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.

Reply: Thank You for the positive recognition and the suggestions for improvement.

Introduction

RC1: Can it really be stated so generally that atmospheric deposition reduces litter decomposition? Especially for high C/N litter, it could be the contrary.

Reply: We agree, we meant decomposition and not litter decomposition and deleted litter.

RC1: 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.

Reply: We agree. We changed the order:

"The objective of our study was to determine the influence of a high frequency of fructification on N fluxes in European beech ecosystems (Fagus sylvatica L.). To achieve this objective, we used seven Level II sites with European beech stands were a number of input-output N fluxes and internal N fluxes were monitored for 15 years to assess the effect of masting on N fluxes. Additional to the standard assessments of the regular monitoring programme we carried out a ^{15}N labelled leaf litter exchange experiment at the sites to study the retention of leaf litter N in the soil under the different frequencies of fructification and site conditions."

Material and methods

RC1: 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 N_2 by denitrification is not mentioned. Can it really be neglected?

Reply: We agree that the wording and explanation for the equation is not sufficiently precise. We will briefly explain our assumptions and changes below:

Firstly, the equation is for managed forests at steady state, thus N export with harvesting is included but not the internal N cycling for litter production and removal. We added "for managed forests with a closed with a closed N cycling between plants and soil with litter products…"

Secondly: Each compound of the nitrogen balance itself have a high uncertainty on its own. Therefore, we do not consider it proportionate to highlight only the N leaching. Therefore, we have added a sentence about the uncertainty and accuracy of all compounds as follows: "We are aware that there is a high degree of uncertainty associated with the estimates of N inputs and outputs for the N balance (e.g. Ahrends et al., 2020, De Vries et al., 2003b). However the N balance is a useful tool for determining small N pool changes in forest soils and much more sensitive as compared to repeated soil inventories (De Vries et al., 2006; Fleck et al., 2019)."

And finally: We agree, a consideration of N_2 production by denitrification would be very desirable. However, a field method for N_2 flux measurement is not available. Furthermore, we assume that only under extreme anaerobic conditions microbial production of N_2 may occur in considerable amounts.

RC1: 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.

Reply: We agree and replaced the word "replicates" in all places in the second paragraph. (e.g. samplers, collectors etc.).

Results

RC1: 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.

Reply: We agree and changed the structure of section 3.3.

RC1: In the results about litterfall, it would be very useful to read something about the LAI of these sites.

Reply: We agree, the LAI is a very interesting parameter to describe the locations more accurately. Despite the high importance of LAI for the fluxes of energy, water and elements, long time series of LAI for calibration and validation of flux models are rarely available because the LAI was only recently amended to the ICP Forests monitoring program. Although there are individual LAI measurements for the sites from the past, different methods were used in some cases. Following the work of Thimonier et al. (2010); Eur. J. For. Res., 129, 543-562, https://doi.org/10.1007/s10342-009-0353-8 the direct comparison of the different LAI methods could sometimes lead to very different results. Therefore, we have decided not to directly show the values for the different sites measured with different methods in the past. Actually for deciduous forest, the direct estimation of LAI with the so called litter trap method is the recommended method in the ICP Forests manual and used for the study plots. Unfortunately, the ^{15}N experiments were conducted prior to the earliest LAI observations.

RC1: The results of the ^{15}N labelling are presented shortly but without a reference to tab. 6, where tracer recovery is given for all sites.

Reply: Many thanks! We inserted a reference.

RC1: 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.

Reply: You are right! We swapped to "recovered tracer N"

Discussion

RC1: 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.

Reply: We agree with you that the first paragraph reads partly like an introduction. However, the first sentence presents our results, which show that in the study period of our seven forest site there was a mast year every two years. This result is then discussed with the results of other studies regarding the historical frequency of mast years. The increase in mast year frequencies and therefore an increase in litterfall of seed and couples is a very important point in our work. We think it is appropriate to discuss it at length.

RC1: 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.

Reply: To quantify the effect strength and sensitivity of nitrogen deposition on the frequency of fructification with ICP Forest plots would be a very interesting topic. But that would have to be the subject of a separate study, as many variables would have to be included in the statistical approach. However factors that influences the frequency of fructification were already described in the introduction (line 70-74) and references are given. The interesting result was in our view that the higher mast frequency in beech forest lead to a higher accumulation of nitrogen in forest soils. This has not been investigated yet, and certainly needs further investigation in the future.

RC1: 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.

Reply: We are aware of the week evidence for such an effect, but it seems feasible. This work probably could initiate additional studies to clarify this, up to know, hidden mechanism for the retention of N in forest soil. The idea to discuss the potential of fruits to immobilize N during their decomposition is very helpful. We included the sentence "Even if the fruit compounds decompose simultaneously, a similar C/N ratio of fruits (44) compared to leaf litter would cause a higher N demand during the decomposition of fruits in mast years."

Style

RC1: 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).

Reply: Many thanks for the valuable tips. Our corrections are described in the „details" below.

Details

RC1: L. 96: why ΔS and not ΔN?

Reply: ΔS was changed to ΔN_S to make it comparable so the other fluxes.

RC1: 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.

Reply: In our view Litterfall N belongs to the internal N cycling which was assumed to be equal to the uptake for the production of litterfall (see above). Therefore, we have not taken into account the litterfall at this point. Thanks for the comment on the symbols in the equation. We follow your suggestion and used subscript.

RC1: L. 142: "was used" is not clear. Specify "for N_E".

Reply: We agree and inserted "for N_E" at the end of the sentence.

RC1: L. 167: "that", not "which".

Reply: Thank You, we replaced "which" with "that".

RC1: 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.

Reply: We agree and make clear that we are talking about the mean of the seven sites. As indication of the variation between the different sites we inserted the coefficient of variation after the given mean values in the text. We add a short explanation in the chapter 2.3 statistical analyses: In addition, the coefficient of variation (cv %) was estimated as ratio of standard deviation and arithmetic mean.

RC1: L. 206: do you mean that pollen is present in the samples, i.e. has been retained by the mesh of the litter traps?

Reply: Sorry, this was a little bit misleading. What we mean was an increase in the production of small pieces together with masting. We have modified the sentence to. "…an increase in the production of small pieces together with masting (Table 4)."

RC1: 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.

Reply: We agree that from the point of view of critical loads calculations, the term "excess" is rather to be regarded as unfortunate. Anyway, the term $^{15}N_{excess}$ was defined in the method section and is often used in the literature in such a way (Han et al. 2017, Tree Physiol. 2017 Oct 1;37(10):1436-1443. doi: 10.1093/treephys/tpx095; Zang et al. 2015 PLoS ONE 10(3):e0121132 DOI: 10.1371/journal.pone.0121132; ….). Therefore, we would like to stay with this terminology.

RC1: L. 254: this should be "increase in frequency" or "decrease in periodicity", not a "decrease in frequency", isn't it?

Reply: Yes, thank you: we changed "decrease" to "increase".

RC1: L. 259: the words "seems" and "likely" together make the sentence too week. Especially as no doubt is mentioned about the historical data.

Reply. We agree and changed "seems to be" to "is"

RC1: 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.

Reply: We agree and used "1,4 to 2,9"

RC1: 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?

Reply: Ok, We changed "historic values" in "historic mast frequency"

RC1: 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.

Reply: We are not sure whether the sentence is misleading. What we mean is that the current chemical state of the soil determined the decomposition of litter and led to the formation of a mor- or mull type humus soil. A change in the chemical condition of the soil leads in the long term to a change in the quality of the litter, e.g. the decrease in Mb cations with increasing soil acidification, which changes the decomposition and influences the soil. We have rewritten the paragraph:

"Acid soils with moder type humus (BBR, NHB, SOB) and less acid soils with mull type humus (FRE, EBR, GW, HOM) retained almost the same amount of leaf litter N (16 – 17 kg ha^{-1}). While the majority of leaf litter N was retained in the organic layer of moder type humus soils, under more favourable conditions for decomposition most of the leaf litter N was transferred into the mineral soil of mull type humus soils (Fig. 2)."

RC1: 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.

Reply: We agree and deleted “primarily”.

RC1: L. 305-310: proposing this as a rule out of a comparison between only 2 sites would at least call for a plausible mechanism.

Reply: We agree that we have to explain it more in detail:

“The seven sites are part of 53 Level II plots in Germany of which half of them (n = 27) retained N in the soil calculated by input-output balances (Brumme and Khanna, 2008). N retention increased with the level of total N and sulphur deposition and the thickness of the organic layer at sites with moder type humus (n = 21) and point to the role of organic layers for the retention of N input in acid soils under high atmospheric load. Simulated acid rain and ammonium additions were found in several studies to affect soil microbial activity and thus the C and N cycling in ecosystems in the short-term (Persson and Wiren, 1993; Berg and Matzner, 1997; Janssens et al., 2010). In the long-term, atmospheric deposition lead to a formation of a decomposer refuge by soil acidification (Ulrich, 1992) and high-N leaf litter, which decompose at lower rate than low-N litter in the later stage of decomposition, increased the accumulation of C and N in the organic layer (Berg et al., 1995). Field measurements confirmed the high potential for soil N retention in an acid soil with moder type humus at the SOB site but not in the less acid soil with mull type humus at the GW site (Meiwes et al., 2009; Brumme and Khanna, 2009). Deposited N in excess for plant increment or gaseous outputs was almost completely retained in the organic layer of the SOB site together with C and increased the amount of humus, indicated by soil inventories between 1966 and 2001 (Meiwes et al. 2009). The annual increase in the organic layer equalled 21 kg N ha^{-1} and 347 kg C ha^{-1} and was confirmed by input and output measurements between 1981 and 2002 (Brumme and Khana, 2009). In contrast, at the less acid GW site with mull type humus most of the N was either used for tree N increment or leached with seepage water while a retention in the soil was negligible. Hence, the SOB site was classified to be in the state of humus accumulation (accumulation type) and the GW site in a (quasi)-steady state of formation and decomposition of humus (Brumme and Khanna, 2008). Recent observations at two forest sites proved the direct effect of S deposition as the main driver for N retention in forest soils. After emission control at the end of the 1990s and reduced atmospheric deposition the C and N pools in the organic layer declined at SOB site from 1.86 Mg N ha^{-1} in 1993 to 0.99 Mg N ha^{-1} in 2010 (Förster et al., 2017) as well as at a spruce stand in the Czech Republic (Oulehle et al., 2011) indicating a predominant effect of atmospheric deposition on microbial activity and N retention.”

RC1: 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.

Reply: We agree and used “suggested”

RC1: 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?)

Reply: We agree, that was misleading. We changed the sentence to „Talkner et al. (2015) observed that foliar P content in 79 ICP Forests Level II European beech plots in Europe decreased during 1991 to 2010 indicating a reduced P availability with increasing N enrichment relative to P and increasing soil acidification.“

RC1: L. 322: is not "bioavailability" the subject? Then the verb should be singular, i.e.

"restricts".

Reply: Thank You. We changed to “restricts”

RC1: L. 323-324: I don't understand this sentence.

Reply: We changed the sentence to „Tree N increment increased with decreasing C/N ratio in the mineral soil (Fig. 5) and retained twice as much N as compared to soils via positive N pool change (Table 5).“ Note: Table 5 is formerly (preprint) table 6

RC1: 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.

Reply: In our view the biological activity is the cause for low C/N ratios in biological active soils since microorganisms convert C of plant litter to CO_2 and reused litter N, thus reducing the high C/N ratios of plant litter to low values in soil by using energy.

RC1: L. 333: "suggests" (singular).

Reply: Thanks! We changed to “suggests”

RC1: L. 335: what is here the "elasticity"?

Reply: Elasticity means that a system returns to its original state after a load. Therefore we extended the sentence: “…close to an (quasi-) steady state with high elasticity against environmental threats like deposited acidity which is buffered by silicates or carbonates “

RC1: L. 357: should be "aggregated".

Reply: Thank you: We changed to “aggregated”

RC1: 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).

Reply: We agree and changed the layout and legend of Tab. 3

RC1: 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.

Reply: We agree that the last column is misleading. We deleted the last column as we are discussing the amount of recovery in the organic layer, which is available in Fig. 2 (Note: the Fig. 2 referred here has been newly created from the former table 5 - suggestion of the second review, that the number of tables should be reduced).

RC1: Fig. 2 and 3: to make the data structure behind these graphs more obvious, it could be added "n = 7" just before "study sites".

Reply: We added n = 7 in the figure captions of Fig. 3. Fig. 2 has been completely removed. See comments on the second review.

RC1: 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.

Reply: Thank You for the suggestion: We make several arrows from one box for clay and

changed the terminology to “tendency”.