**REVIEW 1**

**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 European beech stands of Level II sites, where a number of input-output N fluxes and internal N fluxes were monitored for 15 years to assess the effect of mast production on N fluxes. In addition to the standard regular monitoring programme we carried out a 15N labelled leaf litter exchange experiment at these 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 N cycling between plants and soil by input and output …"

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 changes in soil N pools and is much more sensitive than the repeated soil inventories (De Vries et al., 2006; Brumme et al., 2008; 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. Accordingly, we have integrated the following paragraph into the text:

“Seeds usually decompose faster than e.g. needle litter (Zackrisson et al., 1999), and most likely much faster than seed cupules causing an N demand during its decomposition which

may be fulfilled to some extent by the release of N from the decomposition of leaf litter. Even if the fruit compounds decompose simultaneously, a similar C/N ratio of fruits (44) compared to leaf litter (43) 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. The final version was checked by a native speaker.

**Details**

RC1: L. 96: why ΔS and not ΔN?

Reply: ΔS was changed to $\Delta N_S$ 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 (cv) 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 studied here are a 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) indicating the role of organic layers for the retention of N input in acid soils under high atmospheric load. In several studies simulated acid rain and ammonium additions affected 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 may lead to the formation of a decomposer refuge by soil acidification (Ulrich, 1992) and high-N leaf litter, which decompose at lower rate than litter with low N in the later stages 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). Almost all of the deposited N in excess for plant increment or gaseous outputs was retained in the organic layer of the SOB site as indicated by the soil inventories for the period of 1966 to 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 Khanna, 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 with little retention in the soil. 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 “Our study suggest that”

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 an average twice as much N as compared to changes in the N pool of soils (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 changes 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”.

**REVIEW 2**

RC2: I find the submission by Brumme et al. to be an interesting study, well-written and relatively thorough with valuable contribution to the literature on N cycling in beech forests under increased frequency of masts. I recommend the paper to be accepted for publication Biogeoscience after the identified conceptual and technical issues are carefully addressed.

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

General comments

RC2: Although the study described the importance of litterfall on N cycling (L47-49), the specific effect of fructification frequency was only briefly mentioned (L72-75), and I recommend explaining it in more detail. In the introduction, more emphasis should be given on the reason behind the increased fructification frequency and its implication for N cycling in beech forest ecosystems with better connection to the research question to be answered, and this should be started in the abstract. In the results, showing the temporal pattern of mast frequency (result in thus those from literature) and exploring its relationships with some factors such as N deposition pattern would be interesting.

Reply: We agree that including an exploring analysis of the relationship between fructification and other factors such as N deposition, temperature, precipitation etc. would add another interesting perspective to our study. We therefore explained the effects of fructification in more detail and the implications for the N cycling in the introduction and the abstract. However we restricted the exploring analysis of such relationship for three main reasons:

First, we felt that the quantification of the effect strength and sensitivity of nitrogen deposition on the frequency of fructification have to be the subject of a separate study, as many variable would have to be included in the statistical approach. Effect of N on forest ecosystems in terms of soil processes, vegetation changes and growth are often the result of a cumulative N input (Schmitz et al. 2019; https://doi.org/10.1016/j.envpol.2018.09.101). Accordingly, the analysis of the correlation between N deposition and fructification is not straightforward. This is further complicated by the fact that nitrogen deposition is subject to enormous temporal dynamics (increases and decreases) in the period between 1900 and 2020 (Schmitz et al. 2019; https://doi.org/10.1016/j.envpol.2018.09.101). On the other hand, "only" litterfall measurements from the last 20-30 years are available from the study sites of intensive environmental monitoring.

Second, the main factors from other studies that influences the frequency of fructification were already described in the introduction and the associated references are given.

Third, we felt that the addition implementation (in addition to the nitrogen balance, the litterfall measurements and the $^{15}N$ experiment) of additional data and statistical methods in this manuscript might make the text hard to understand.

Thus, while we think an analysis of the temporal pattern of mast frequency and exploring its relationships with N deposition is clearly worthwhile, we would prefer not to make it part of this manuscript.

RC2: The 15N-labelled leaf litter exchange experiment should be explained in depth, and the calculation of the 15N-lablled litter N recovery in soil needs to be shown. It was not explained why the retention of 15N labelled litter N in plant (aboveground and belowground biomass) was not quantified. Were roots present in the soil samples? If so, the 15N recovery in the root should be presented.

Reply: Roots weren't analyzed, accordingly, results on $^{15}N$ recovery in roots cannot be presented. The calculation of the $^{15}N$ uptake by plants seems somewhat problematic from our point of view, since the area of the labelled 15N plots were too small. We have explained the methodological limitations in detail in the subsection 2.1.1 as follows:

"Nitrogen consist of two stable atoms, $^{14}N$ (99,634 %) and $^{15}N$ (0,366 %). During N transformation processes, the ratio $^{14}N/^{15}N$ may change due to isotopic discrimination. For an accurate estimation of the recovery of added $^{15}N$ in labelling experiments, $^{15}N_{na}$ is needed. The $^{15}N_{na}$ of the samples of the sites ranged from 0.365 % in the L layer to 0.368 % in 30 – 40 cm soil. The recovery of the added $^{15}N$ in a labelling experiment $^{15}N_{ex}$ (%) is calculated by subtracting $^{15}N_{na}$ from the measured enrichment of $^{15}N_S$ in the samples ($^{15}N_{ex} = {}^{15}N_S - {}^{15}N_{na}$ (%))."

RC2: It would have been more interesting to see how retention of 15N-labelled litter N differs from that of deposited N, which can be explained by presenting and/o discussing the results in this study with other 15N-labelling studies.

Reply: We took up your suggestion and we are now discussing the N retention in more detail by comparing the $^{15}N$ retention of labelled ammonium and nitrate with the retention of nitrogen in labelled leaf litter:

"Studies with $^{15}N$ labelled N deposition provide a useful insight into the initial retention processes of N deposition in the forest ecosystems. Irrigation studies with $^{15}N$ labelled ammonium on undisturbed soil cores (30 cm length) from the SOB site in the field indicated a N retention of 20 % in the organic layer and 26 % in the mineral soil over a period of 12 months (Brumme et al., 2009a). Irrigation studies of undisturbed cores from different soil depths of the SOB soil showed a N retention of 54 % in the organic layer and of 34 % in each of the mineral soil layers (0 – 10 cm, 10 – 20 cm depth) after a two-month irrigation with $^{15}NH_4$ at 8°C in the laboratory. Low nitrification rates (heterotrophic) at the SOB site (Bauhus et al., 1996; Brumme et al., 2009b) result in the adsorption of $^{15}N$ labelled ammonium and contributed 3 % to the N retention of 54 % in the organic layer, 24 % of 34 % in 0 – 10 cm, and 30 % of 34 % in 10 – 20 cm soil depth. A high adsorption capacity for ammonium extended the residence time and the potential for transformation processes due to microbial immobilization and plant uptake in acidic soils and may have increased the transformation of deposited ammonium in organic compounds. Nitrate was only marginally retained in the soil layers of the SOB site (< 3 %) as shown by a comparable study with $^{15}N$ labelled nitrate (Brumme et al., 2009a). The less acid GW site had a similar retention of 45 % of $^{15}N$ labelled ammonium in 30 cm long soil cores as compared to the acid SOB site (46 % recovery). However, the adsorption of ammonium at the GW site was negligible due to high autotrophic nitrification. Most of the $^{15}N$ retained in the GW cores was transformed to organic compounds in the mineral soil. The high N retention in the mineral soil of the less acid GW site was evident from the high microbial biomass which was two times higher at the GW compared to the SOB site, especially in the mineral soil, where it was five times higher (Brumme et al., 2009c). Ammonium retention was closely related to the microbial biomass at the SOB and GW soils indicating its dominant control on ammonium retention in forest soils in the short-term. Within the NITREX project, where the fate of doubled $^{15}N$ labelled $^{15}NH_4{}^{15}NO_3$ in throughfall was studied over a 12-month period, N retention was twice as high in the organic layer as in the mineral soil of a podzolic soil in Denmark (26 % versus 12 % recovery, Tietema et al., 1998). The uptake of $^{15}N$ by ground vegetation and trees amounted to 45 % of the applied $^{15}N$ showing that a large part of the deposited N was introduced into the internal N cycle. Most of the N usually returns back to the soil with litterfall in the following year and may be retained in the soil by mast products, as was observed in the $^{15}N$ labelled leaf litter exchange experiment. Thus, the initial microbial retention of N is one pathway into labile organic N compounds while the repeated plant uptake and litterfall of N is probably the common transformation processes producing more stable N compounds in soils."

Specific comments

RC2: L19-20: This part misses a logical connection between changes in fructification and its implication for N cycling that needs investigation.

Reply: Thanks for the comment. We extended the sentence: "But the processes involved are not fully understood, notably the effect of fructification on N fluxes".

RC2: L23-25: retention of litter N should come later when discussing N retention.

Reply: Thank you for the suggestion. We shifted the sentence accordingly.

RC2: L35: Consider adding a concluding statement about the important implication of this study.

Reply: We added the following sentence:

"These results have major implications for our understanding of the C and N cycling and N retention in forest ecosystems. Especially the role of mast products for N retention seems to need more research in the future."

RC2: L38-39: the effect of N deposition is presented as things of the past in this paragraph. Better to discuss it as a general issue that has been observed in past, still happening, and is expected to happen in the future using the right tense.

Reply: We agree. We changed the tense and added the sentence:

"Despite reduced sulphur emission since the beginning of the 1980s (Engardt et al., 2017) N deposition still exceeds the N demand for forest growth in unmanaged and most managed forests (Meesenburg et al., 2016; Fleck et al., 2019)."

RC2: L45-46: fragmented sentence; not well connected to the preceding one.

Reply: We agree and moved the sentence upwards.

RC2: L50-51: provide (a) reference (s).

Reply: We changed the sentence as follows:

"There is an increasing evidence that the frequency of fructification in beech forests has increased when compared to that in the past decades: It was …"

References are then given in the next sentences.

RC2: L71-72: Is it the effect on the amount of fruit produced?

Reply: This is only a hypothesis: We changed the sentence in the introduction to: "may affect the fruit production"

RC2: L85-86: Although, the sites used in this study are Level II plots, results from Level I plots from previous studies were also discussed in the result (e.g., line 250). Thus, I suggest giving brief introduction of the Level I and II plots in Europe here.

Reply: Thanks for the suggestion. We provided a brief introduction in this section.

"The study sites are Level II plots of the ICP Forests Intensive Monitoring Programme established under the UNECE Convention on Long-Range Transboundary Air Pollution (De Vries et al., 2003a). The Level II Intensive Forest Monitoring is carried out at about 800 selected forest ecosystems representative of the major European forest types with the aim to study the relationships between stress factors such as air pollution and forest ecosystem responses. The Level I monitoring is based on a systematic 16 x 16 km grid and covers around 6000 plots, where annual crown condition assessments are carried out. So far, two forest soil condition surveys were conducted from 1985 to 1996 and 2004 to 2008, respectively (Cools and de Vos 2011)."

RC2: L99: Are the slope (precipitation) at the experimental sites small enough to exclude N losses from the plot through surface runoff in the N balance?

Reply: Ok, this point should be discussed. We entered in Table 1 the slope gradient for the seven study sites. Additionally we inserted the following sentence in the Text:

"The slope of the sites ranges from almost flat to a maximum inclination of 7.4°". ….

"With regard to the maximum slope inclination and the estimated infiltration capacities, we assumed that N losses from the plots through surface runoff is of subordinate importance. Due to permanently formed macropores and low bulk density providing complete infiltration of precipitation surface runoff is generally rare in forest ecosystems (Jankiewicz et al. 2005; Neary et al. 2009)."

Jankiewicz, P., Neumann, J., Duijnisveld, W., Wessolek, G., Wycisk, P., and Hennings, V.: Abflusshöhe - Sickerwasserrate - Grundwasserneubildung - Drei Themen im Hydrologischen Atlas von Deutschland, Hydrologie und Wasserbewirtschaftung, 49, 2-13, 2005.

Neary, D. G., Ice, G. G., Jackson, C. R.: Linkages between forest soils and water quality and quantity. For. Ecol. Manage., 258, 2269–228, https://doi.org/10.1016/j.foreco.2009.05.027, 2009.

RC2: L133-142: Why is it called 'net' gaseous exchange and how was it estimated (L133)? Have you measured gaseous N uptake too? The N flux data should be moved to the result section.

Reply: NO can be taken up by soils. We are using chambers and the enrichment of gases in the chamber represents the net gas exchange. The data presented are published and only used for the calculation of soil N change by equation 1, thus it would be better in our opinion not to move it to the result section. We added the note:

"…were measured with the closed chamber technique…"

RC2: L155-159: I am less supportive of this part. Where were those 300 seeds obtained from? Since the litterfall were collected from 1998-2008 for the three Bavarian sites, how the number of years without mast and mast year were presented as 22 and 11, respectively?

Reply: We agree. The wording of this section was misleading. We have rearranged the section and extended the sentence to make it more clearly as follows:

"For the Hessian and Lower Saxonian study sites we calculated the number of seeds from the measured dry weight (g DW $m^2$ $year^{-1}$) of annual seed litterfall flux by assuming a one single seed weight of 0.22 g (cv = 13%). This weight was determined by using a subsample of 300 seed from these sites. The calculated value showed a good agreement with other studies (Kaliniewicz et al., 2015; Bezdeckova and Matejka 2015). For Bavarian sites the seed mass was calculated by using a mean ratio between seeds and the sum of seeds + seed cupules of 0.14 for years without mast (n = 22), and a ratio of 0.37 (n = 11) for mast years. These ratios were derived from Rhineland-Palatinate (NHN, KHB, and NHQ sites) where the mass of seeds and cupules were separately measured between 1995 and 2005."

RC2: L171-172: It is not clear why 'the soil columns were cut into slices'. Or are you saying the soil cores were divided into layers?

Reply: We changed "cut into slices" into "divided into layers"

RC2: L175: This sentence about the 15N labelling experiment should come earlier as part of the above paragraph. I suggest revising this whole section 2.2 with more orderly description of 1) experimental design/establishment and then discuss, 2) sampling, and 3) Laboratory analyses.

Reply: We agree and rearranged the section 2.2.1

RC2: L182-183: Does that mean your data did not fulfill the normal distribution? If that is the case, have you tried some sort of data transformation?

Reply: The main reason for our approach to use $r_{Spear}$ is the small sample size. We have only seven study sites and thus a small sample size. For this sample size, normality tests have little power to reject the null hypothesis. Therefore, small samples most often pass normality test (Ghasemi & Zahediasl 2012). A robust alternative is to calculate correlation coefficients according to Spearmann ($r_{Spear}$) (Rhodes et al., 2009; Sachs 1999), as we did in our study.

Ghasemi A, Zahediasl S. Normality Tests for Statistical Analysis: A Guide for Non-Statisticians. Int J Endocrinol Metab. 2012;10(2):486-9. doi: 10.5812/ijem.3505

Rhodes, J. R., McAlpine, C. A., Zuur, A. F., Smith, G. M., and Ieno, E. N.: GLMM Applied on the Spatial Distribution of Koalas in a Fragmented Landscape, in: Mixed effects models and extensions in ecology with R, edited by: Zuur, A. F., Ieno, E. N., Walker, N. J., Sveliev, A. A., and Smith, G. M., Statistics for Biology and Health, Springer, 469-492, 2009.

Sachs, L., 1999. Angewandte Statistik. 9. Aufl. Springer Verlag, Berlin. 881pp.

RC2: L192: Should not it be 'foliar nutrient'? The term 'N cycling' obviously represents a far more complex interaction processes and pools that are not presented under this section.

Reply: Thank you for your suggestion. We used "foliar nutrients" and "litterfall" instead of N cycling.

RC2: L221-230: The subtitle should reflect the data presented, not the method. What does the '15N excess' (L222) represent? How did you calculate it? The 15N excess, to my understanding, is the changes in 15N content of soil pools following the addition of 15 labelled litter N. I also wonder how the 15N recovery (Table 5) was calculated? The 15N excess (‰) and 15N recovery (%) should clearly and separately presented. This should be explained in section 2.2.1.

Reply: We agree. We changed the subtitle 3.2 to "Recovery of $^{15}N$ labelled leaf litter nitrogen in the soil". The description of the $^{15}N$ terms was incomplete, we extended the description in section 2.2.1 as follows:

"Nitrogen consist of two stable atoms, $^{14}N$ (99,634 %) and $^{15}N$ (0,366 %). During N transformation processes, the ratio $^{14}N/^{15}N$ may change due to isotopic discrimination. For an accurate estimation of the recovery of added $^{15}N$ in labelling experiments, $^{15}N_{na}$ is needed. The $^{15}N_{na}$ of the samples of the sites ranged from 0.365 % in the L layer to 0.368 % in 30 – 40 cm soil. The recovery of the added $^{15}N$ in a labelling experiment $^{15}N_{ex}$ (%) is calculated by subtracting $^{15}N_{na}$ from the measured enrichment of $^{15}N_S$ in the samples ($^{15}N_{ex} = {}^{15}N_S - {}^{15}N_{na}$ (%))."

RC2: L252: Which time reference is being referred to as 'in the past' since all the discussion so far indicated that mast frequencies has been increased.

Reply: We changed “in the past” by “before the 1960s”.

RC2: Line259-260: Can you provide the temporal changes in mast production for the European beech forests? This will be useful to explore possible correlation between the temporal changes in mast frequencies and some possible global change factors (e.g., N deposition, as mentioned in the next paragraph).

Reply:

Thus, while we think an analysis of the temporal pattern of mast frequency and exploring its relationships with N deposition is clearly worthwhile, we would prefer not to make it part of this manuscript. A detailed description of our motivations we have given in the section "General comments ".

RC2: L191-294: In this part, texts about dry mass production, need to be either integrated into the rest of the discussion about litter N retention or be moved to section 4.1.

Reply: We agree and included the following text in section 4.1:

“The annual amount of leaf litterfall ranged from 2.76 to 3.88 Mg $ha^{-1}$ (Table 3) and was in the same order of magnitude (2.95 to 3.33 Mg $ha^{-1}$) as in 36 old-growth forest stands of Fagus sylvatica across a broad gradient of soil fertility covering nine mesozoic and kaenozoic parent material types (three limestones, two sandstones, two clay stones, one sand and one loess substrate) (Meier et al., 2005). Leaf litterfall in mast years did not differ from that in non-mast years (Tab. 3), as was also observed by Müller-Haubold et al. (2015). However, a significant correlation between total and leaf litter fall was observed in this study (Tab. 6, Fig. 5). Similar correlations were reported by Meentemeyer et al. (1982)." Note: Table 6 was former Table 7.

RC2: L305: What is the reason for humus degradation at the site? The negative ΔS could be due to the high N leaching at the site (Table 6). The site also has low retention capacity for new N input (litter N). Explain the reason for the low N retention at this site and its implication.

Reply: The reason behind the degradation of humus is not clear but it indicates a shift of mull type humus soils to mor type humus soils driven by soil acidification. Probably the formation of humus is disturbed by aluminum. However, the negative budget is too small and probably not significant which is why we only cited Ulrich (1992), for more information.

RC2: L314-315: This sentence about N deposition effect on N retention comes between two sentences that explains effects of P availability.

Reply: We agree and revised the section.

RC2: L340-341: ‘When comparing historic data with results from litterfall observations across Europe since the 1990s an increase in fructification frequency seems likely’. Is it not certain?

Reply: Absolutely right our wording is not really accurate. We replaced “seems likely” with “is obvious”.

RC2: L341-343: Focus on N, which is the main topic of this study, not carbon and other undefined ‘nutrient’.

Reply: Ok we deleted ‘nutrient’. However, in our opinion the C and N cycles are strongly coupled by several loops. Accordingly, C should also be considered

RC2: L349-350: this confounding effect has not been explained in the discussion. In the context of the study sites, what is the main cause of the soil acidification? Are the study forests considered as N-saturated?

Reply: You are right, we added the discussion about the effects of soil acidification on N retention (s. General comments)

Tables and Figures

RC2: Is it possible to reduce the number of tables (optional suggestion)? Can some of the data in the tables be presented in Figure?

Reply: Good proposal. We have changed Table 5 into a more readable figure.

RC2: Data should be presented with some measure of uncertainties; simple calculated SE would be nice.

Reply: We agree and inserted as indication of uncertainty and variation the coefficient of Variation (cv) 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.”

RC2: Table 2: Present C, N, and P content for soil organic layer as you did for mineral soil? I would also be more logical to present the nutrient content before their stoichiometry.

Reply: We agree and have changed the order.

RC2: Table 6: values in the last column are confusing as they show fraction of fraction. Moreover, the values (which I assume to show 15N recovery in Organic layer divided by total recovery) do not much that when calculated using the 15N recovery data in Table 5 for most sites (e.g., BBR).

Reply: Thanks for the suggestion. We deleted the last column of Table 6 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 following your suggestion to reduce the number of tables).

RC2: Fig 5. The terms 'internal' and 'external' N cycle, as described here, is confusing, if not wrong. Commonly, internal N cycle in an ecosystem refers to N cycle between microbes, vegetation, and soil. The components in the schematic diagram and the direction of the arrows connecting them does not convey clear message. For example, all the DM and N in

different litter type are not connected to the total litterfall. How are soil N pool, N deposition and N uptake are connected to other components in the diagram?

Reply: You are absolutely right, the description is misleading. The diagram shows only the detected effects of litterfall and soil properties on the N retention. We changed the first sentence in: „Schematic view of the detected effects of N uptake, total litterfall, TLF, leaf litterfall, LLF, and soil properties of the organic layer, OL and the mineral soil, MS, on the N retention by tree increment, INCR, the soil N pool change, ΔNS, the leaf 15N recovery, 15N RECOV, and on the seepage N output. “

RC2: Few technical corrections /writing

RC2: L26: While there is no fundamental rules on this issue, generally, numbers from zero through to ten are written as words, and larger numbers are written as numerals. Being consistent is more important.

Reply: Thank you: We follow the recommendation with one exception. When the numbers have units we used numerals.

RC2: L25: Comma should be added after ‘In these forests’. The proper use of comma needs to be carefully checked in the whole text (e.g., L78, L101, L147, L148, L168… and a lot more)

Reply: A native speaker had check the comma in the final version.

RC2: L32: Change ‘about’ to ‘only about’

Reply: We changed it.

RC2: L75: use ‘N’ instead of ‘nitrogen’ as you indicated it in the first sentence in the introduction. Check other places (e.g., 279)

Reply: We agree and used N instead of nitrogen. Other places were also examined.

RC2: L80: change ‘soil specific’ to soil-specific’

Reply: We changed it.

RC2: L98: delete the first ‘deposition’ and ‘from the atmosphere’

Reply: We deleted it.

RC2: L108: It should be written as ‘the BBR’. Check other places.

Reply: We have changed it and checked and adjusted the other places in the text.

RC2: L27: write ‘site specific’ as ‘site-specific’

Reply: We changed it.

RC2: L29: It should be ‘inventories’.

Reply: Yes! Thank you

RC2: L154: ‘sites’. There was only one site at Rhineland-Palatinatehere (L87-88).

Reply: You are absolutely right, we added in the section “2.1 Study sites” the following sentence: “…and three sites in Rhineland-Palatinate, one at Neuhäusel (NHB, 704) and two other sites (Kirchheimbolanden, KHB, and Neuhäusel Quarz, NHQ). KHB and NHQ were evaluated with respect to the litterfall fractions in order to disentangle the different properties of seeds and seed cupules (Table 4).”

RC2: L161: Write ‘light exposed’ as light-exposed’

Reply: We changed it.

RC2: L167-168: while the purpose of the PVC is obviously to create the plots, this is not clearly stated.

Reply: We agree and added “to create the plots”

RC2: L172: Change ‘grinded’ to ‘ground’

Reply: We changed it.

RC2: L176: Change ‘Numbers’ to ‘The number’

Reply: We changed it.

RC2: L181: It should be ‘Statistical analysis’

Reply: We agree and have changed it.

RC2: L198: I think this section (2.4) is misplaced here. Should not it be at the end (after the main text)?

Reply: We agree and placed the section 2.4 (L188) at the end of the main text.

RC2: L199: Change ‘each’ to ‘every’

Reply: We changed it.

RC2: L202: change ‘amount’ to ‘the amount’

Reply: We changed it.

RC2: L203: Change ‘Mean changes’ to ‘The mean changes’

Reply: We changed it.

RC2: L232: Change ‘measured’ to ‘study’

Reply: We changed it.

RC2: L235: Change ‘were’ to ‘was’ and ‘Mean’ to ‘The mean’

Reply: We changed it.

RC2: L272: Change ‘nutrient rich’ to ‘nutrient-rich’

Reply: We changed it.

RC2: L295: Scientific names should be in italicized. Same issue in Table 1

Reply: we changed the scientific names in the text and in Table 1

RC2: L197: Change ‘2-years’ to ‘2-year’

Reply: We changed it.

RC2: L309: Change ‘high’ to ‘the high’

Reply: We changed it.

RC2: L325: Change ‘base rich’ to ‘base-rich’

Reply: We changed it.

RC2: L334: ‘that’ is better’ instead of ‘which’

Reply: We agree and changed it.

RC2: L347: delete the first ‘N’

Reply: We deleted “N”

RC2: L350: delete ‘still’

Reply: We deleted “still”

Reference

RC2: The referencing style needs to be carefully checked. Few examples where correction is needed are:

Reply: Thank you for pointing this out. There was really a lot of work to do!

RC2: DOI should be provided in consistent style (e.g., including URL.)

Reply: We provided all DOI’s in consistent style including the URL.

RC2: L433: Delete the date and month

Reply: We deleted the date and month

RC2: L493: the journal abbreviation is not correct

Reply: We changed it to “Glob. Biogeochem. Cycles“

Figures and Tables

RC2: Texts fonts (e.g., types) in the figure are different from that in the main text

Reply: We have now chosen the same font for the illustrations as for the text.

RC2: Figure pane labels are better be placed at the top left corner of each pane.

Reply: We have placed the figure pane labels for each figure at the top left corner

RC2: Fig 2 and 3: The year on the y-axis is not necessary.

Reply: According to our opinion the given recovery on the y-axis of Fig. 2 and 3 relates to a period of 5.5 years. Therefore, we think that it is not unimportant to specify the exact unit.

RC2: Fig 4: Capitalization of words in the y-axis label

Reply: We have changed the capitalization.