Response to the interactive comment by the anonymous referee # 1

General comments. The study used δ15N of soil profiles to assess ecosystem-level differences in N cycling in three forest ecotypes within the Congo Basin (tropical lowland forest, tropical montane forest, and subtropical Miombo woodland). Based on the distinct δ 15N soil profile observed in each forest, the authors conclude that the montane forest indicate a closed N cycle the lowland forest and Miombo woodland tended to have more open N cycles. The study also examined the effect of surface slope angles on δ15N in the same forests to quantify local differences induced by topography, but they found a contrasting effect. Furthermore, the study did a pan-tropical analysis of soil δ15N to reveal that rainfall, vegetation cover, and topography are the main factors to explain δ15N variability between five different tropical forest sites. I find the submission to be well-written and relatively thorough with valuable contribution to the literature on N cycling in tropical forests, for which limited information is available. The subject of the study is suitable for SOIL. However, there are some conceptual and technical problems and manuscript should be revised before it is accepted.

We thank the reviewer for the constructive and thorough review. We addressed the points raised by the reviewer on a point-by-point basis below. We are happy to address the mentioned concerns to improve our manuscript and believe that this will greatly benefit to the new MS quality.

General comments
Although the study briefly mentioned that soil δ15N values can signal openness of ecosystem N cycle (line 68-69), it lacks explanation on how soil δ15N values are interpreted as integrator of N cycling. Indeed, the interpretation of soil (and plant) δ15N values as indicators of N availability is not straight forward with many contradicting interpretations of observed pattern of soil/plant δ15N, and this need to be highlighted in the study with relevant studies from local to global scales. Many factors (not only N availability) affect soil δ15N values at a given site and across sites. Particularly, I am concerned with the lack of data on plant δ15N. There is no linearity between soil δ15N values and N viability, and this needs to be acknowledged in the manuscript in depth, which is also supported by the data in this study.

We acknowledge the concern of the reviewer that interpreting only soil $\delta^{15}N$ values and try to conclude based on these values on the nutrient status of the soils is ambiguous. Unfortunately, measurements on different processes and plant $\delta^{15}N$ are not available for all the sites. Thus, our interpretations will remain somewhat speculative. Nevertheless, we agree with the reviewer that this issue needs to be addressed more in depth in our manuscript and we will add more context and better identify limitations and uncertainties.

Another major issues/question is why only five sites are included in the SEM? As a result, the SEM was also overly simplified (few sites and few potential variables). Some relevant studies from the bulk studies in other tropical forests in Africa, SE Asia, and S America should be included in the analysis as well as discussion of the results in this study.

We agree with the reviewer that only a few data points were included in the SEM. Our goal was, in addition to studies in the literature, to include factors controlling for erosion (slope, LAI and MAP) in the model and to see if these factors can explain local soil $\delta^{15}N$ variability. Global datasets (for example from Craine et al. 2015) do not contain this information, as slope and LAI of the sampling points are missing. Furthermore, the given GPS coordinates are not precise enough to extract slope values from DEMs for the literature values and within site variability would be neglected. Thus, we included only the studies in the SEM model, where all information was available, and we ended up with 112 samples from 5 different tropical forest ecosystems. We will scan the literature again to see if no more data is available and add the concern of the over simplified SEM into the discussion of the results.

Specific comments
Line 16: Change ‘stable isotope signature’ to ‘natural abundance of stable 15N isotope’

This will be changed accordingly.

Line 23: ‘no influence of topography on soil N cycling’. This is not supported by the study. No effect of topography on soil δ15N does not mean that topography has no effect on soil N cycle, which is broader than soil δ15N. The author needs to be more cautious when using N cycling instead of soil δ15N.

We thank the reviewer for pointing this out and we agree that the sentence needs rephrasing. We will make sure that the differences between soil δ15N and soil N cycle are clearer in the manuscript.

Line 24: ‘δ15N’ needs to be referred to consistently (soil δ15N, soil δ15N signature, stable isotope signature…are all used to refer to soil δ15N in the manuscript).

We will revise the manuscript accordingly to be more consistent in the naming of the δ15N values.

Line 35: ‘However’ does not seem necessary

We agree with the reviewer and will remove “However”.

Line 44: replace ‘forest’ by ‘forests’
Line 52: Delete ‘activity’
Line 55: Correct ‘intact’ as ‘an intact’

We thank the reviewer for the grammatical corrections and will amend the manuscript accordingly.

Line 58-62: revise these sentences. Consider this ‘Some studies from geometrically active sites of the tropics (Costa Rica and Taiwan) found lower N availability and more closed N cycle in steeper sloping positions suggesting that erosion has a significant control on N cycling (Hilton et al., 2013; Weintraub et al., 2015). However, and the magnitude of this effect in more stable landscapes is unknown calling for a consistent study across geomorphic gradients in the tropics.

This sentence will be revised in the new version of the manuscript.

Line 64 : Edits ‘The stable isotope composition of N (δ15N)’ as ‘The natural abundance of stable 15N isotope (δ15N) of plant and soil pools’
Line 66: should be ‘insights into’

We thank the reviewer for pointing this out and will address this in the new MS version.

Line 75-82: A testable hypothesis about the pattern soil δ15N and N availability and openness of N cycle is needed. I would also question the hypothesis that soil δ15N would be lower on steeper slopes because the erosion on steeper slopes removes fresh organic matter input from plants, which would continuously keep δ15N of surface soil low compared to the deeper surface.

We will rephrase the hypothesis to: “We hypothesized N availability and openness of N cycle would be highest in lowland tropical forest, which is indicated by lower δ15N signatures.” We hypothesized that the isotopic signature of topsoil N is more depleted in steeper slopes compared to the isotopic

signature of topsoil N in less steeper slopes and not compared to the deeper surface of the same profile. We will rephrase this hypothesis to avoid confusion.

Line 93: Are both forests used in this study?

The sampled lowland forest catchment (260 ha) consists of these two forest subtypes. As we had a randomly spatial sample coverage, it is most likely that soils from both sub-types have been sampled. However, we did not identify all tree species at each sampling location to determine if it is a monodominant or mixed forest. We will amend the text that it is clearer to the readers.

Line 130: 'Laboratory' is more formal

We agree with the reviewer and will change the sub header to "Laboratory analysis".

Line 135: provide δ15N of the atmospheric N2

This information will be added to the revised version of the manuscript.

Line 146-147: Why only these two sites were chosen?

As we focused on the effect of topography and soil erosion on the soil $\delta^{15}N$ signature, only literature data with reported soil slope values of the samples were considered. To the best of our knowledge no other studies had a sampling strategy with within site variation of slope angles.

Line 155 (last sentence): Consider putting it at the end of the paragraph

This will be changed accordingly.

Line 157-159: The SEM analysis was very simple with only five sites with only few potential factors that affect soil δ15N being included in the model. What are the variables included in the model?

As described above we only included soil $\delta^{15}N$ data, where slope angles of the samples were available, thus only 5 sites were included in the SEM. We included MAP, MAT, LAI, slope and soil C content as predictive variables for soil $\delta^{15}N$ in the model. We will include this information into the new version of the manuscript.

Line 161-165: The values of these variables needs to be directly presented; it is not enthusiastic to many readers to extract the information from the Table (estimates).

The values for N stocks and C:N ratios are already mentioned in the text for each site. We will also add the values for the $\delta^{15}N$ to the text, that is easier for the reader to extract this information.

Line 187: I would not use 'N cycling'. This study did not investigate the many aspects of N cycling. More importantly, the many factors known to affect soil δ15N and which are very important to interpret soil δ15N are not measured.

We agree that the title might be misleading and suggest to change it to: "Using soil $\delta^{15}N$ signatures to assess differences in ecosystem N-turnover"

Line 188-89: Eshetu et al., 2004 Forest Ecology and Management 187, 139–147 (Ethiopia) and Gerschlueret et al., 2019 Biogeosciences 16, 409–424 (Tanzania) are some of the relevant references missing.

We thank the reviewer for providing additional references for our manuscript. So far, we listed only references from old growth natural tropical forests. The suggested papers are from young-growth forests in Ethiopia and semi-natural montane forests in Tanzania. However, it still might be interesting to expand our literature values and we are considering including these references into our manuscript.

Line 207-208: This is not necessarily true as lower soil/plant δ15N is not always associated with limited N availability (closed N cycle). Gurmesa et al., 2017 Biogeosciences, 14, 2359–2370 (many other studies in SE Asia) have reported ecosystems pools can be strongly 15-depleted under N saturated condition.

We thank the reviewer for pointing this out and agree that it is not clear that the presented soil $\delta^{15}N$ profiles indicate a more closed N cycle compared to the other two forest systems. We pointed this out in the subsequent sentence of our manuscript that the generally lower $\delta^{15}N$ values are probably influenced by the isotopic signatures of the inputs. However, we think that we can rephrase this sentence better to acknowledge the concerns more, using the proposed literature.

Line 209: how about the effect of δ15N of deposition N? Craine et al., 2015b?

We agree with the reviewer that N deposition influences the isotopic signature of soil N and will include this with the provided reference in the revised manuscript.

Line 214: depleted N-input from where? Only biological N deposition? Do you have data for N2-fixing plant species as well as their mycorrhizal association in the three forests? These are very crucial to interpret soil δ15N values.

While for the Miombo forest the depleted N-input probably is mainly from more N2-fixing, the montane forest is more likely to receive depleted biological N input via deposition (Bauters et al., 2017). Unfortunately, we do not have data on N2-fixing species available for our sites, but it is well documented this process is more important in the subtropical woodlands, compared to the tropical forests (Hogberg & Alexander 1995)

Line 236: this sentence does not help with the logical flow points being discussed in the paragraph

We thank the reviewer for pointing this out and agree that this sentence is indeed out of place. We will remove this sentence in the revised manuscript.

Lines 237-238: Line 226-227 repeated? Again, as I mentioned above, low soil δ15N does not necessary indicate closed N cycle. The context needs to be discussed. To say whether N cycle is dominated by organic N, it needs additional measurement. Is there data for soil inorganic N concentration in each forest?

We agree that the whole paragraph contains too many repetitions. We will restructure the whole paragraph to have a better flow for the readers. We measured aquatic N exports for all the catchments and the montane forests exports slightly more dissolved organic N (67% of TDN is DON) than the lowland forest (61%). We will add this data to the manuscript.

Line 239: Edit ‘excess of available N’ as ‘excess N availability’. However, it is not correct to conclude that the forests have excess N availability only based on the values of soil δ15N.

This will be changed accordingly in the manuscript.

Line 240: It is amazing that the author did not provide data on N deposition for any of the sites

(including those from literatures).

N deposition data from montane and lowland forest are available from the literature and will be presented in the new version of the manuscript. Unfortunately, to our knowledge, no N deposition values are available for the Miombo forest.

Line 248: change ‘soil N’ to ‘soil δ15N’.

We will change this in the revised manuscript.

the discussion about effects of topography on soil δ15N is interesting, but it did not establish mechanistic relationship of topography with other factors known to strongly affect soil δ15N. The implication in discussion here is that soil δ15N is strongly affected by physical process (erosion) and the factors that control the erosion.

It is true that other factors than erosion influence soil $\delta^{15}N$ (temperature, precipitation and vegetation cover). However, these factors did not vary within our sites and only the physical processes were potentially influenced by slope gradients. We suggest that we address this issue shortly at the beginning of the paragraph.

Line 289: ‘samples’ or ‘sites?

We thank the reviewer for the attention to the detail. Sites is correct and this will be changed.

Few technical corrections /writing
Line 19: delete one of the ‘in’s
Line 65: Should be Craine et al., 2015a. Also check line 209. Figure 2: first letter in y-axis label should be capitalized Figure 3: first letter in x-axis label should capitalized Table 2: Is it important to have all those decimals for fixed effect Estimates? References Clarke et al., 2013 (Line 32) and Vitousek 1985 (line 40) are missing. The superscript in 15N or δ15N are not correctly written for many reference

We thank the reviewer for mentioning these technical and writing errors. We will amend all proposed changes in the new version of the manuscript.