Response to the interactive comment by the anonymous referee # 2

The manuscript quantifies the nitrogen (N) stocks and N isotopic composition of soils at three locations in the Congo Basin. The aim was to explore N availability in ecosystems across this poorly studied region, in the broader context of understanding N cycling in tropical forests. As a key macronutrient, the N cycle of these forests is a critical part of understanding how an ecosystem might respond to external drivers (changes in pCO2, climate, landuse). The study finds large contrasts in the stable N isotopic composition (d15N) between the sites, alongside changes in N stock, and seeks to link these to differences in environmental and geomorphic variables. At each site, the work explores how slope angle (and topographic position) influence d15N, building on some past work in Taiwan and Costa Rica, to explore how geomorphic processes influence N cycling. The study was well focused, succinct, and the theme makes it worthy of attention at SOIL. However, I found the discussion quite hard to follow, and it was hard to draw out the main findings. My main comments below reflect this, and make some suggestions for revisions:

We thank the reviewer for the insightful comments and constructive review. We address all the reviewer's comments below and believe that - through a revision of the manuscript- the MS quality will greatly improve.

1) Provide a clearer assessment of the potential controls on d15N in soil: This doesn't have to be more than a paragraph, as this has been done in other papers (from time to time), but the paper lacks a clear explanation of what controls the d15N values of soil N. This would be useful in the introduction, and then used to seed the structure of the discussion and help a clearer assessment of what best explains the patterns in the data. I would suggest something that talks about N inputs (and their d15N values), internal N cycling (plant to soil) and role of N losses (gaseous, dissolved, particulate) and how they may fractionate (or not) N isotopes in soil. Some of this is there in the manuscript, but its not that clear, and confused by the "open" vs "closed" discussion (see next point).

We thank the reviewer for raising this point. We agree that the description of the factors controlling soil $\delta^{15}N$ signatures would be well placed in the intro. We will lay this out in the intro and link back to that in the discussion. This will also help structuring the manuscript better, in retrospect.

2) The "open" vs "closed" explanation for d15N values: This seems too simplified now, as we recognise that we can vary several aspects of the N cycle in an ecosystem and arrive at the same d15N values. For instance: i) the comparison between the N stock (N/km2) and input and output fluxes (N/km2/yr) can play a role, as with any isotope mass balance; ii) the N inputs (deposition, fixation) can be fractionated (or not); iii) the N outputs (gaseous, dissolved, particulate) can be fractionated (or not); iv) and pedogenesis and timescales of soil formation can vary (giving different intergration periods for different sites, and over depth). So with this explanation at hand, the simple argument of closed vs open is simplistic. In fact, the open vs closed model (I think) implicitly assumes that all N losses are fractionating, and that the ratio of N stock to N fluxes are the same at every site. Both those assumptions are flawed. Instead, this study measures N stocks (and C/N, so relative to C). So it can say something about how this varies (and the paper doesn't use this information paired to the d15N data).

The 'open' vs 'closed' system approach is widely used in literature. It is one way to interpret the scarce data available. However, we agree that the explanation is far from perfect and all the points mentioned by the reviewer will also have an influence on the soil $\delta^{15}N$ values and need to be addressed accordingly in the discussion. We see that we need to improve the way of discussing our data and put more emphasis on all the possible processes influencing the measured soil $\delta^{15}N$ values.

The study doesn't measure plant d15N, NO3 in porewaters or streams, or any gaseous N (that is very rare to do). This means any discussion of these important features of the N cycle and their d15N

values would have to be drawn from other studies, and somewhat speculative for these sites. However, at the moment the paper doesn't discuss at all what these could be, and whether they could vary between the sites. By way of example, the lower MAP at the Miombo site could influence soil moisture – which is important for gaseous N loss (under saturated conditions) and NO3 loss (which can have a low d15N value). Thus, this could explain the shift in isotopic values: this site has less fractionating N losses. Or could it be simply a plant input (fixation) story. Another quick example, the montane and lowland sites have similar d15N values, but the lowland site has much lower N stock (but higher relative to carbon C/N). So, to get the same d15N depletion in the soil residue, one has to invoke that the N fluxes out of the system (which fractionate) are larger in the montane system, than the lowland (because to see a d15N shift, you need the flux to be larger). This text from me is somewhat off the top of my head. I could be completely off the mark here. But my point is that there are details to the dataset which are not discussed clearly, and the open vs closed discussion constrains this discussion in my view. A more structured discussion (see below) could also help.

We thank the reviewer for pointing out this unclarity, we highly appreciate this.
Indeed, we agree with the reviewer that a more detailed discussion about the different possible mechanisms which can influence $\delta^{15}N$ signatures is needed. The more depleted values in the Miombo woodland is most likely due to more N2-fixation, occurring in this ecosystem, compared to the tropical forests. The stable isotopic signature of soil N in the montane forest is especially in topsoil a lot lower compared to the signature in the lowland forest, this is indicating that N inputs are depleted and/or less fractionating processes (or more erosion) are happening in the topsoil. However, the different shape of the $\delta^{15}N$ profiles tend to indicate different N availabilities as the shape present in the montane forest is typical for an N-limited ecosystem (increasing values with depth) while the shape in the lowland forest is more typical in N-rich ecosystems (highest $\delta^{15}N$ value in an intermediate depth).

3) Discussion section: I would recommend restructuring this to either take a more site by site explanation of patterns. Or a process by process explanation of patterns (e.g. starting with potential N inputs – could these explain things; then differences in N stocks; then potential N outputs). This could help draw out the key take away messages a little better.

To improve the structure, we are glad to apply the suggestion by the reviewer and structure the section 4.1 by discussing process by process and then explain how every process influences $\delta^{15}N$ at each site.

Note – only having completed my review did I then read the comments already posted in the discussion. I found myself in agreement with queries flagged by the other reviewer.

We are glad that both reviewers are agreeing on the points mentioned. We addressed everything from reviewer 1 in a separate response.

Other comments (with line number):
19: maybe avoid the word "profiles" here – as the reader could infer you're talking about a soil profile, with depth.

We indeed mean the soil $\delta^{15}N$ distribution over the depth of the soil profile.

19-20: this sentence would be better linked to the variability in d15N values measured, and how they've been interpreted.

We agree with the reviewer that this sentence needs to be moved up and we will do so in the revised manuscript.

23: this sentence on montane forest was a little confusing following the preceeding sentences, and perhaps the order of information here needs to be revised.

To make it more coherent we will change this sentence to: “Despite the steep topography, slope angles do not constrain soil $\delta^{15}N$ in the montane forests, although this ecosystem experiences high variability in the stable isotope signature.”

44: can the sentence “it is important” be rephrased to better spell out what the knowledge gaps are?

With this sentence we wanted to emphasize that different tropical forests are highly variable in nutrient availabilities and that a generalization of tropical forests being rich in N and poor in P is not suitable. This statement summarizes the points made before in the paragraph and helps to reason our research.

46: the “openness” section of text. I wonder if you need a couple of sentences explaining the inputs of N to ecosystems, and the losses. And then the idea that the overall size of the pool and leakiness is conceptualised as open vs closed. This might be clearer to those not familiar with the N cycle in soils.

We agree that in this paragraph some more information on the different processes altering soil $\delta^{15}N$ signatures is needed. We then will use the open vs. closed conception to summarize all the input vs. output processes of the system.

60: I partly agree with that statement... But there is an important detail - Hilton et al., don’t invoke the open vs closed concept. Instead, they argue that the nature of the N loss varies with slope, and that physical erosion and export of organic N in solid form does not fractionate the N isotope pool. In that way, the isotope mass balance is different for sites on steeper slopes (N loss dominated by non-fractionating losses), vs shallower slopes which potentially have a greater role of fractionating N losses (dissolved N forms, N gas forms).

We agree that the concept of open vs. closed systems might not fit best for the studies cited here. We suggest that we change this sentence. The effect of physical soil loss on the $\delta^{15}N$ signature is described later in the introduction and is acknowledged by the reviewer below.

69: please expand on the “openness of the N cycle” comment.

As already stated above, this whole paragraph will be re-written, and the open vs closed system will be described in more detail.

70: yes this is exactly what I write above! I should have been patient. Anyhow, I think perhaps that means that the order and flow of content might need some edits here.

We will edit the manuscript in line 60 and don’t link the two cited studies with the open vs. closed concept.

105: experimental design seems good – and impressive range of sites across this setting. A quick Q – do you know the bedrock geology and whether it varies (and whether it could contain N?).

We thank the reviewer for acknowledging our experimental design and we will try to find information on the bedrock geology of our study sites.

Figure 1 – please add a note to the caption that the colours are elevation (I guess?) and perhaps make a note of the resolution of the DEMs shown here.

Indeed, the colors show different evaluations of the sampling sites. We will add this information and the DEM source to the figure caption.

Table 1 – is there a typo here? The lowland forest has the highest mean slope (22degrees) – which doesn't seem to fit with what you have shown in the histograms of slopes in Figure 1.

The observation of the reviewer is right, the values for the mean slopes of the lowland and montane sites are swapped. This will be changed in the new version of the manuscript

135: briefly detail the external standards used to re-calibrate the d15N values and their precision etc.,

We will add a more detailed description of the stable isotope measurement to the manuscript.

140: adapted or used?

We used the model calculation of Pelletier 2012 and we will change the text accordingly.

138: a bit more context on why this model was selected would be useful.

We assume that the reviewer wants more context on why the SEM model was selected. We used a structural equation model as possible dependencies between variables can be included. We will add more information to the manuscript.

Figure 3 B – how did you lump the sites together to get this erosion coefficient?

As described in equation 1, the EC is calculated from the slope, MAP and the LAI. To calculate average EC values for each site, the average slope of every single sample from each site was used to calculate the respective EC.

Section 4.1. – I found this hard to follow. There is some repetition of themes and information (especially in the final paragraph), and it was hard to take away the main discussion points the authors wanted to highlight. It might make sense to start with a discussion of the N inputs, and the top soil values (and their contrasts) and what that indicates about them. The discussion N outputs/internal cycling (and depth profiles) at each site. And try to draw together a somewhat coherent discussion. One of the striking things is how high the d15N values are in the lowland (and at depth in the montane) and I finished this section without a clear idea what that was being attributed to.

As already suggested above, we will restructure the discussion in section 4.1 and discuss process by process and their potential influence on the $\delta^{15}N$ signature at each site. The higher $\delta^{15}N$ values in the lowland topsoil is most likely to less depleted N inputs compared to the Miombo woodland (less N2-fixation). The montane topsoil in general experiences a lot more erosion compared to the lowland forest, thus the stable isotopic signature is more depleted compared to the lowland forest. The isotopic values of the $\delta^{15}N$ profile of the montane forest are steadily increasing with depth and this is a typical shape of profile of an N limited ecosystem (Hobbie and Ouimette, 2009). While rephrasing the whole paragraph, we will make sure that the underlying processes are more clear to the reader.

Table 2: I don't understand the "Estimate" values in this table, and struggle to follow what they refer too.

The values in table 2 show the results for all the fixed effects estimated by the linear-mixed effects model (Estimate) and the respective standard errors and P-values. A presentation like this lets the

reader easily find the mean values for each response variable and it is clearly visible which effects differ significantly from others. We will change the table from “Estimate” to “Effect size” to make it clearer for the readers

251 – “there are no steep slopes in the lowland forest” – this does suggest that Table 1 is incorrect.

As mentioned above the slope values in Table 1 are switched between montane and lowland forest and will be changed in the revised manuscript.

255: more about the controls on the EC output would be useful – as to why Miombo is so much higher. And how you computed the EC values for the literature data. And how Figure 3B came about (and the assumptions and limitations associated with it).

The driving variable behind the high EC value in the Miombo forest is mainly the low LAI, as we already stated in the text. The forest cover in the Miombo is less dense compare to the other forest ecosystems and thus the soil is less protected from the erosive force of rainfall events. In section 2.4 we described how we calculated the EC values and where we obtained the data for the values of the literature. However, we agree that we can describe the assumptions and limitations of the model more clearly in the text and will do so in the revised manuscript.

301: this note on N fixation was not clearly discussed in the main text – see comment above on Section 4.1

We discussed the possible influence of fixed N_2 on the $\delta^{15}N$ signature in the Miombo woodland in section 4.1 L 213-219. However, the whole section 4.1 will be re-structured as suggested and we will try to discuss this issue of N fixation more thoroughly.