EGUsphere, referee comment RC2
https://doi.org/10.5194/egusphere-2022-490-RC2, 2022

Comment on egusphere-2022-490

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Referee comment on "Soil organic matter diagenetic state informs boreal forest ecosystem feedbacks to climate change" by Allison N. Myers-Pigg et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-490-RC2, 2022

General comments

The manuscript still lacks in my opinion clarity and structure, and it is not easy to follow. In particular, the hypotheses being tested are described in just 4 lines, and it is very hard to see the connections of those hypotheses with the whole (rather long) introduction or their relevance. The whole manuscript seems written before the hypotheses, to be honest, and the hypotheses just added at the end as a patch to make some former referee happy and not as a logical tool. They are not even directly considered in the conclusions.

Concering Hypotheses 1: the diagenetic state of SOC seems aking to "quality" or "recalcitrance" of SOC (this relationship needs to be clarified in the intro, or at least explained better why diagenic state of SOC is relevant). This is the main determinant of SOC kinetics, but the stocks are not just determined by the kinetics. I alöso have concerns about how you tested such hypothesis, read below.

Concerning hypothesis 2: I do not understand well how you tested it, You state at line 369: "we expect soil C and N cycling are coupled across these forests in association with climate warming" (and then proceed telling you could not really link them). That doesn't seem testing a hypothesis to me, and I really do not get the experimental approach you followed to test Hypothesis 2. Plus: C and N are not necessarily coupled, you can definitely have variations in the C:N ratios of ecosystems (for sure of plant organs). For example: https://www.sciencedirect.com/science/article/pii/S0378112713004155, if tree species composition changes due to climate change (or, as in your case, just different biomes) also the C:N ratio will change.

Materials and methods are sometimes explained in paragraphs scattered across the MS.

The authors need to work on the structure of the MS and on the logical consistency of

what is being tested and how. The relevance of the results are also unclear (probably because of the above-mentioned lack of structure. Once you have an hypothesis to test you can also define its relevance in the introduction, before proceeding with the rest of the manuscript).

I also have some concerns about the study itself, which I will address here, while less specific comments.

In particular, my main concerns are:

- Methodological issue: about the LPDI index construction and its validation. It is stated that it was an iterative process, but the iteration steps are not described properly in M&M or they were not clear to me.
- Validation of the LPDI index extrapolated by the PCA model: I understood correctly, the only validation Is the comparison between your LPDI index (the first PCA component) and an NMR ratio. I have no idea what you refer to with "measured LPDI" since your LPDI is a PCA component, but in any case, the agreement between your LPDI and the NMR ratio should be shown in detail. I could not even understand what the agreement was between the two, and this is the only link with some sort of physical reality of your index. It is crucial. On top of that, a PCA model will likely be overfitted, and it would be best to have this validation on independent samples (you measure the NMR ratio on them, apply your PCA model coming from your study and different samples to derive the LPDI estimate, and then measure the R^2).
- Foundations of the experimental setup: the diachronic approach chosen in the MS might present a lot of issues, that are not discussed, while results are compared with synchronic approaches (warming on a single site). A climatic gradient IS NOT warming. Environments in different climates are likely already at equilibrium (more or less), while rapid warming resulting from an experimental manipulation brings the system far from its previous equilibrium state. I do not think the results are comparable, and I have doubts that a sequence over climates can offer information on climate warming in general.
  Successful warming experiments that have used climatic gradients that I am aware of have taken a sample from one location and physically moved replicates of the same sample over the gradient.
- Similarly, you should dig into the concept of equilibrium of SOC. An increase in inputs always results in an increase in outputs. Sure, it brings the equilibrium stocks up, but it's not a linear relationship. SOC decays universally as an exponential function (at least the vast majority of its variance is explained by it), so 10 tons more inputs are not going to result in 10 tons more stocks, but maybe 1 ton because the more the inputs, the higher the fluxes. You should probably try first to model the stocks you observed with such a relatively simple approach and then proceed to explain any eventual residual variance, if any.

Specific comments:

Paragraph 2.4: describe in detail the iteration steps (some details are later after line 275)

Paragraph 3.3: Introduce a detailed explanation of the validation approach in M&M, and describe with measurements the results of the validation here.

Discussion: here, you talk about some hypotheses. Describe all your hypotheses in the introduction, and then proceed to test them. Describe in M&M how you are going to test. Mention the result of the tests in the conclusions. This will add clarity.

Line 369-370: C and N cycles are not necessarily coupled one-to-one. C:N ratio can vary, and for example, a site can lose fertility as a consequence.

Conclusions: your last statement should be motivated. How do you think this measurement could reduce uncertainty in models? And how do you think it could increase our understanding of such feedbacks? Other than that, conclusions should tell the reader if the hypotheses being tested were verified or not.