

I carefully read the manuscript as well as the authors' reply to the previous round of referee comments. From my point of view, the authors have done a good job addressing the reviewer comments. The limitations are now better discussed. I just have some additional suggestions

- The manuscript is generally very lengthy
- Abstract sentences 1 and 2 are contradicting. Suggest to delete the first sentence
- Above ground biomass carbon, soil organic carbon, and total N were newly measured for this study. I was confused that the methods were not described and the results not shown in the main text, and only later found a description of the methods in the supplement. However, soil analyses are not defined appropriately. They are at once way too lengthy and on the other hand lacking any citation. Such basic measurements as above ground biomass, soil organic carbon and total N should be done according to standard protocols. This should be cited appropriately. I would suggest to move the methods section (in a more concise) form to the main text. Also, I would suggest to start the results section with these observational results (leaving out the modelling in a first table or figure). This will help the reader get to know these sites and how they respond to the different treatment, which are then the basis for interpreting the model.
- L. 223-234 P deposition is assumed to be negligible in this model. Actually, more and more evidence is showing that P deposition is just as important as rock weathering for P inputs to terrestrial ecosystems (see e.g. Aciego et al. 2017). This should be considered for further model development in the future
- to improve readability, I suggest to reduce abbreviations. Specifically, no need to abbreviate PFT
- L. 349, replace the coma with a period
- L. 628 authors state organic P release from SOM and immobilization are poorly represented in models and that they encourage further study to quantify these processes. I agree with these statements; however, from reading the manuscript I wondered if the authors were aware of the state of the art P flux measurements since the results are not discussed in light of measurement data? Several studies have actually measured organic P mineralization and microbial immobilization with radioisotopes, and would be relevant for interpreting the modelling results presented here. For example, Bünenmann et al. 2012 looked at mineralization fluxes in grasslands under NPK treatments and Schneider et al. 2017 calculated organic P fluxes in calcereous soil.
- General remark on over-selling: authors should be careful not to overinterpret their results stemming from modelling two grassland soils, especially given the limitations as discussed.
- L. 46 Wardlow is not a globally important C sink. Please delete this sentence, since it is not appropriate to extrapolate from two sites simulated here onto a global level
- L. 641 same here. It is inappropriate to generalize from the two grassland sites about ecosystems in general all over the world.
- L. 648 I don't consider N14CP to be "one of the first" CNP models. Many other models come to mind, some of which much older or much more developed: JSBACH, CABLE-CNP, CLM-CNP, ORCHIDEE-CNP, QUINCY, ForSAFE, ...

References

Aciego, S., Riebe, C., Hart, S. et al. Dust outpaces bedrock in nutrient supply to montane forest ecosystems. *Nat Commun* **8**, 14800 (2017). <https://doi.org/10.1038/ncomms14800>

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