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Biogeosciences Discussions

Interactive comment on "Carbon storage in phosphorus limited grasslands may decline in response to elevated nitrogen deposition: a long-term field manipulation and modelling study" *by* Christopher R. Taylor et al.

Anonymous Referee #2

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This manuscript investigates the effect of nutrient addition on grassland using a combination of process based modelling and observations from a manipulative experiment. The authors use the simple ecosystem model N14CP to simulate nutrient dynamics at two contrasting grassland sites in the UK and compare these results to data from a long term n and P addition experiment at the same sites. The study shows that P availability difference between the sites leads to differing limitation over time and differing effects of nutrient addition. The question of N and P limitation and co-limitation is very important and very topical, especially in the context of anthropogenic N deposition. Using

process based models in conjunction with manipulative experiments is a very useful tool, not only for validating models but for advancing our understanding of ecosystem processes.

One of the main issues of the manuscript in its current form is the way the observations are actually used to inform the model. As far as I can tell, the data is simply used to calibrate two parameters and then hardly ever mentioned again. The first problem here is the calibration itself: all experimental data is used at this step. This means that implicitly the model can represent observations from all treatments and lowers my confidence in the model's ability to predict responses to nutrient additions. I would suggest performing the calibration with the data from the control plots only, if there is sufficient data.

The second data issue is the lack of model data comparison beyond figure 2. Specifically for figure 4 I wonder if it would be possible to add the observations to the plots rather than just referring to a supplementary table. It might even be useful to just show the experimental period, currently in figure insets, to better show how the model compares to the experimental results. This would increase our confidence in the model and build up the argument towards the predicted long term trends.

I also find that the paper lacks a discussion of the model's short-comings. While I understand the usefulness of simple models in allowing easier process attribution and avoiding over-parameterisation, N14CP is lacking a number of processes compared to state of the art vegetation models and this needs to be acknowledged. Most importantly, model NPP does not appear to include a response to increased CO2. This is particularly important for predictions of long-term nutrient limitation as elevated CO2 has been shown to increase plant nutrient limitation and I do not see how we can have any model predictions that do not take this into account.

Other missing processes are less important, but would still need a paragraph in the discussion, especially the very simple plant pool structure and limited plant control on

nutrient demand and uptake. It is even unclear to me if there is a belowground plant pool that would determine the N and P uptake or indeed if NPP scales with biomass. I do want to stress that I am not suggesting that authors modify their model or that the model is wrong, but that the assumptions in the model structure need to be highlighted and discussed.

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