

Interactive comment on “Impacts of fertilization on grassland productivity and water quality across the European Alps: insights from a mechanistic model” by Martina Botter et al.

Edoardo Cremonese (Referee)

e.cremonese@arpa.vda.it

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This study addresses a relevant topic and could be interesting for a broad range of disciplines. Methods are sound and robust. It's very well written and presented. Below my main comments and a few minor suggestions.

We thank the Referee for acknowledging the relevance of the study and generally for appreciating the manuscript.

Note to editor and authors: I'm the PI of IT-Tor site. I've done my best to try not to be influenced by any "home-bias" in doing the review.

Main comments

1174, 1233-236, Tab3 and fig 2. I'd like to see an evaluation of NEE too. GPP is not directly measured by the eddy covariance technique; it's the result of a model as well. Why did you choose to show LE in fig2? I would have found NEE or GPP more informative. Consider the idea of adding H, GPP, and NEE plots like fig2 at least in S1.

We reported in Figure 2 the pattern of LE because latent heat represents the variable linking hydrology with the energy exchange at the land-surface. We will report the comparison between observed and simulated H, GPP and NEE as supplementary information in the revised version of the manuscript.

Please however note that we spin-up the carbon pools running the soil-biogeochemistry module for 1000 years using average climatic conditions and prescribed litter inputs taken from preliminary simulations with the soil-biogeochemistry module inactive. This solution brings the system to a dynamic equilibrium, while the actual system might not be in equilibrium with regards to carbon. This discrepancy will be reflected in the comparison with NEE, which is the most problematic variable to compare with observations when the history of land-use is not known, which is very often the case, see also our replay below.

1191-196: carbon and nutrient pools at least in some of the 9 sites (if not in all of them) could be easily obtained by site PIs. I recommend comparing the results of the spin-up exercise with actual values. Which is the fertility range (modeled and observed) of the 9 sites? What would have changed if rather than starting from the results of the spin-up exercise, the fertilization experiment would have been run on actual carbon and nutrient pools values? (cfr 1355-360).

We thank the Referee for raising this relevant point, which will be included in the discussion. We agree that total carbon and nutrient pools data could be likely included asking the Project's PIs. However, the measured values might not be representative of the entire “footprint” and most important total carbon and nitrogen will not suffice to initialize the 55 pools required by the model. For instance, we would need the subdivision of SOC in mineral associated organic carbon, particulate organic carbon, dissolved organic carbon and also microbial biomass separated in bacteria and fungi components. This might be available in certain experiments (e.g., Cotrufo et al 2020) but not at all sites. However, we will search for total soil C:N ratios at each site and compare them with what is simulated by the model.

fig4 is very important as it gives an idea of how good the model is in simulating structural grassland properties (biomass and LAI) and their temporal dynamics after grass cuts. Unfortunately, the figure is not very clear. It's hard to understand how well the model reproduces interannual variability, absolute values, temporal dynamics around cuts,... . I acknowledge that it's difficult to find another solution but it could be worth trying. I do not see snow depth data highly informative in this context (e.g. 1255-258 can not be depicted from the plot)

We think it is important to show the inter-annual variability but we acknowledge that also the intra-annual variability is important. We will show a few dedicated zooms on the supplementary material to highlight

intra-annual patterns. However, we prefer keeping the pattern of snow depth on Figure 4 as we think this is relevant to show the controls on the beginning of the growing season.

A phytosociological or botanical description of the sites is missing. Relevant community and structural differences exist, to my knowledge, at least in some of the sites included in the study. Many of the results you get (e.g. fig4 and fig5) could be seen and commented in the light of species composition and assemblage. To what extent the fact that species composition is not considered in the model could have influenced some of the results? @1404-408 can be articulated in more detail and with a broader perspective. We will add the botanical description where available and expand the comments in the discussion concerning the limitation of not considering multiple species but just an average grassland. However, it is very likely that the botanical heterogeneity effects will be smaller than the current precision in measuring carbon and water fluxes at ecosystem scale.

Minor

1112-114: I suggest using the "official" fluxnet codes IT-Tor, IT-MBo, ... CH-Fru, ... throughout the manuscript

We thank the Referee for the suggestion and we will use the Fluxnet codes in all the Fluxnet sites and use the TERENO coding for the German sites DE-Fen, DE-RbW, DE-Gwg.

1161: which are the "soil biogeochemistry parameters" considered fixed and homogeneous between sites? which are the site-specific parameters used?

We will make this distinction clear highlighting in bold the site-specific parameters in Table S1.

1168 selected for what?

We can clarify "selected for all the simulations".

1173 maybe evaluate model performance is better than confirm

We will follow the Referee's suggestion.

1184 what is the reference simulation? was is introduced before?

Reference simulations are meant as the simulations used for evaluating the model performance before running the different fertilization scenarios. We will clarify it in the manuscript.

1185 the implications of this unrealistic assumption must be further discussed and articulated in particular "thus guaranteeing a nutrient application, similarly to fertilization ... "

The assumption of leaving the cut grass on the ground is clearly unrealistic because it opposes the purpose of the grassland management (i.e., producing yields), but it guarantees a nearly closed nutrient cycle, thus performing the same function of fertilizers, or in other words provide the most target fertilization possible. While aiming at testing the model performance on the baseline scenario, we preferred introducing such an hypothesis instead of assuming a fertilization rate for each site to avoid excessive nutrient addition or excessive nutrient starvation or generally to treat all sites equally. Of course, a posteriori, with all the scenarios we run, we could have relaxed the hypothesis and select a given fertilization regime for each site. However, this would have been only possible a posteriori. We will better explain this hypothesis in the manuscript.

1189 maybe "flux tower footprint" is better than "below the flux tower"

We thank the Referee for the suggestion and we will change the text accordingly.

1243-253 and tab 4. do you get the same picture using measured data?

Please refer to the reply to the second main comment.

1270-275 is a reference to fig 5 missing here?

Yes, thank you for the suggestion.

1328-329: see also the previous comment. A more detailed summary of fixed and site-specific parametrization could be useful

We thank the Reviewer for the suggestion and we will introduce a more detailed list.

1333: "limitations in grass growth and thus LAI at low nitrogen availability" which is the result pointing in this direction?

This observation is supported by the results of the scenario analysis (Figure 6a), where for low-N scenarios grass growth was N-limited. We will clearly refer to this result in the revised version of the manuscript.

1342 "temporal drifts". references?

We added the references as suggested and Takruri et al., 2011 and Mittelbach et al., 2012.

1356 can you be more precise here? How big the differences between modeled management and true local management dates can be?

Difference will not be too large, but they can be of a week or two in certain years. We refer to management as the combination of manure application and grass cut. We simulate fertilization using a fixed amount of manure as reported in the literature, which is applied on fixed days of the year. Similarly, we assumed fixed days of the year for the grass cuts. In reality, the manure quantity and the days of manure application as well as the days of the grass cut vary from year to year (see SI in Fu et al., 2019). We will clarify this point in the manuscript.

1843 fig1 and tab 1. IT-Tor coordinates in table 1 are correct but the position shown in fig1 is wrong

Thank you for noticing this. We will correct it.

1859-867 fig4 I can't find panel references (i.e. a), b), c), ...) in the plot

Thank you for noticing this. We will correct it.

References

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