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.

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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

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

l174, l233-236, 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.

l191-196: 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 l355-360).

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. l255-258 can not be depicted from the plot)

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? @l404-408 can be articulated in more detail and with a broader perspective.

Minor

l112-114: I suggest using the "official" fluxnet codes IT-Tor, IT-MBo, ... CH-Fru, ... throughout the manuscript

l161: which are the "soil biogeochemistry parameters" considered fixed and homogeneous between sites? which are the site-specific parameters used?

l168 selected for what?

l173 maybe evaluate model performance is better than confirm

l184 what is the reference simulation? was is introduced before?

l185 the implications of this unrealistic assumption must be further discussed and articulated in particular "thus guaranteeing a nutrient application, similarly to fertilization ... "

l189 maybe "flux tower footprint" is better than "below the flux tower"

l243-253 and tab 4. do you get the same picture using measured data?

l270-275 is a reference to fig 5 missing here?

l328-329: see also the previous comment. A more detailed summary of fixed and site-specific parametrization could be useful

l333: "limitations in grass growth and thus LAI at low nitrogen availability" which is the result pointing in this direction?

l342 "temporal drifts". references?

l356 can you be more precise here? How big the differences between modeled management and true local management dates can be?

l843 fig1 and tab 1. IT-Tor coordinates in table 1 are correct but the position shown in fig1 is wrong

l859-867 fig4 I can't find panel references (i.e. a), b), c), ...) in the plot