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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The manuscript by Botter et al. presents results from a model of grassland dynamics, focusing on productivity and nutrient losses along gradients of elevation and fertilization intensity. The topic is interesting and suitable for the audience of Biogeosciences. The process-based model adopted for this study (T&C-BG) is also suitable to answer the main research questions - how does grassland productivity changes across sites in the Alps, and how are productivity and nitrate leaching affected by fertilization regimes? The chosen sites span a wide range of climatic, edaphic, and management conditions, and the model setup and fertilization scenarios in combination with the site-to-site variability allow tackling this question. I have a couple of comments aiming at expanding the scope and impact of the work, and several minor suggestions, listed below.
We thank the Referee for the appreciation of the manuscript and for the comments which will be used to improve the presentation of the work.

General comments

- The presented analysis is interesting and complete, but I wonder if it would be possible to run a 'climate change' simulation scenario. Higher temperature is expected to increase the ET/precipitation ratio and decrease soil moisture, which might shift nitrogen losses from leaching to denitrification, or might shift the partitioning of mineral nitrogen in favor of plant biomass unless water stress ensues. These interactions (also in relation to fertilization regimes) would nicely complement the current analyses, and they would increase the potential impact of the work. One option could be to simply use climatic conditions from a lower-elevation site to run simulation at a higher-elevation site, or increase temperature (at constant or variable relative humidity) at a given site. These would of course be rather 'theoretical' explorations, but not dissimilar to those set up to test the effects of altered fertilization.
We long thought about this addition and we excluded from the first version of the manuscript, as we did not want to make results and discussion excessively long. However, we agree that integrating a climate-change scenario analysis would increase the impact of the study and we therefore consider implementing it in the next version of the manuscript. We would like to integrate at least one climate scenario for each fertilization scenario, built assuming an increase of air temperature of +3°C and an increase of atmospheric CO_2 concentration of 300 ppm. As changes in precipitation will be extremely uncertain and likely within historical stochastic variability (e.g., Fatichi et al 2016), we deem that modifying temperature (and consequently vapor pressure) and CO_2 might suffice to highlight what can happen in a future climate.

- The metric used to characterize the efficiency of N conversion to biomass is the ratio of harvested N to N concentration in leachate. Typically, agronomic studies define N-use efficiency as a ratio of N in harvested products over N inputs (fertilization, deposition, fixation if N fixers are present). I wonder if such a metric would be more informative. It would allow comparing sites on a N input basis, and values are easily interpreted as 'partitioning coefficients' telling where the N inputs end up in the system.
We thank the Referee for the suggestion, we will compute the N-use efficiency as the ratio between the harvested N and the input N and add this information on top of the currently used metrics.

Specific and technical comments

General: the chemical formula for nitrate is NO3-, not NO3, so it might be worth adding the superscript minus throughout the manuscript
Thank you for noticing this. We will change it.

L15: "unprecedented" sounds a bit an overstatement

The unprecedented was referring to the combination of all these different model components but we agree with the suggestion of the Referee and we will delete the term "unprecedented".

L38: how about gaseous losses? Are they important in the nitrogen budgets of these grasslands?
Since we mention gaseous losses in discussion (L360-361) we agree with the Referee that gaseous losses should be mentioned also in the introduction of the study.

Question 3: this question is rather generic - we know that mechanistic models can provide guidelines, but are these guidelines relevant/applicable? I would actually skip this question altogether, as testing model-based guidelines in the field is outside the scope of the manuscript
The analysis provides some useful information which should be taken into considerations by legislators while setting guidelines for management. Under the additional climate-change scenario such results will be hopefully even more relevant. In the discussion, we do not provide actual guidelines but we rather highlight how mechanistic models can account for variability in soil and hydrological purposes not included in the current fixed-threshold guidelines. We will rephrase this third research question in order to clarify that the goal of the study is not to provide actual guidelines, but to explore possible improvement in the methodology that defines guidelines where mechanistic models could have a more important role.

L125: check terminology - water leakage or percolation; nutrient leaching (check throughout the manuscript)
We thank the Reviewer for this observation and we will make sure to be more consistent throughout the manuscript.

L136: it seems that standard meteorological data are enough to drive the model. Are eddy flux data necessary?
Yes, meteorological data are enough to drive the model, eddy flux data are only used to validate it.

L146: nutrient leaching
We will modify accordingly.

L168: water leakage
We will modify accordingly.

L181-182: this sentence is not very clear - what should be accounted for?
We will rephrase to enhance clarity.

L186: is this really unrealistic? Later it is stated that N applications follow grass cutting, so the modelled timing of N addition is right - is it the amount of added N that is "unrealistic"?
The practice of leaving the cut grass on the ground is unrealistic, but the replacement of the amount of exported nutrients contained in the yield, through some "sort of fertilization" is realistic. We will better explain this hypothesis in the revised manuscript.

Section 2.4: I see the point of running the model at steady state for each fertilization scenario, but I wonder if equilibrium is reached over time scales relevant for management. If the system reaches equilibrium after 500 years (just as an example), then we should perhaps focus on the transient dynamics after fertilization regime is changed - that is, a timescale relevant for management decisions rather than a timescale for ecosystem equilibration
We completely agree with the Referee observation. This choice is simply a pragmatic one. Knowing the land-use and management scenarios of last 500-1000 years is impossible almost everywhere and initializing the model with observations is not possible with current data of total bulk C and N only. Even assuming bulk C and N as representative for the model, we will require at least the separation in SOC components and in microbial types. Such limitation is already discussed in the manuscript (L356-358), but we will expand this section. Theoretically, we could run simulations where different scenarios are considered sequentially so to analyze transients rather than equilibrium condition, but such as solution will likely need many more model scenarios and combinations than what we currently present. We think it is out of scope for this manuscript, but it can be an interesting experiment for a future manuscript – the role of transient C and N pools on biogeochemical response.

L265-266: are the actual cutting times at the field sites available?
Unfortunately, not in each site, only for some.

L270-275: I would refer to Figure 5 in this paragraph
We thank the Reviewer for pointing out the lack of the reference to the figure, we will integrate it.

L332: check singular/plural "feedbacks. . . are realistic"
We thank the Reviewer for pointing out this inconsistency. We will correct it.

L399 and 446: verb "to take up", not "to uptake"
We thank the Reviewer for pointing out this mistake. We will correct it.

L432: how is "optimal fertilization level" defined? As shown in Figure 6, there are diminishing returns on N input, but how can an optimum be defined in these monotonically increasing harvested N vs. input N curves?
The optimal fertilization level is better identified by the maximum of the curve in figure 6d. We will better discuss this point it in the manuscript.

Figure 1: is the site Torgnon located in Valtournenche (Valle d'Aosta)? If so, please check the position of the site in this map, as it is outside of Valle d'Aosta, further to the south
We thank the Reviewer for pointing out this inconsistency. We will correct the location on the map.

Figure 3: would it be possible to highlight the growing season periods? What are the soil moisture sensors measuring during the winter, when the soil is frozen? Is it meaningful to compare modelled soil moisture (I assume liquid phase only) with measured values (affected by both liquid and solid phase) when the soil is frozen? I would focus these comparisons on the growing season only
From what we can understand from metadata, observations should be typically referring only to liquid content but in some case readings during "freezing periods" are problematic and lead to spurious values, we rarely see water content dropping even if soil is below zero degrees in the data. As freezing is occurring only in a few sites and for short periods, we compared the total simulated soil moisture (liquid + solid) with the observed "soil moisture". We will highlight the growing season in the plot.

Figures 4-5: I am not sure I understand why biomass data in Figure 4 do not cover the same year(s) as data shown in Figure 5
This incongruence is related to the different data sources for this study. Data for Figure 4 are provided by the managers of flux towers, while Figure 5 is based on published data of lysimeters and they cover a shorter time period. Unfortunately, lysimeter data for the same time period are not available.

Figure 6: "kg" not "Kg" in the axis labels; are the markers and lines indicating the median modelled values (shaded areas are explained in the caption, but I missed the explanation of the lines)?
We thank the Reviewer for pointing out the inconsistency of "Kg". Yes, the lines connect the median values, we will add the explanation in the Figure caption.

Table 1: would it be possible to add information on site slope/aspect (if not on flat terrain), and soil type?
We can add information concerning the soil type, while generally flux towers are placed on a flat area, with no slope to match the theoretical requirements to observe mass and energy fluxes with the eddy covariance system, therefore, the information would be redundant.

Table 3: is net radiation modelled (as affected by modelled energy partitioning at the surface?) or used as an input variable?
Net radiation is definitely modelled as the sum of the contribution of different land surface components. The value observed in the flux-tower is only used for comparison.

Table 4: are the mean values based on the periods with available flux data? Would it be worth including plus/minus standard deviation or some measure of the variability?

Yes, the mean values refer to the periods with available flux data. We thank the Reviewer for the suggestion and we will add the standard deviation of annual values (mean+- standard dev.).

References

Fatichi, S., Ivanov, V.Y., Paschalis, A., Peleg, N., Molnar, P., Rimkus, S., Kim, J., Burlando, P. and Caporali, E. Uncertainty partition challenges the predictability of vital details of climate change. *Earth's Future*, 4: 240-251. doi:10.1002/2015EF000336, 2016.