

## To editor

We thank the editor to let us to revise and improve our manuscript taking into account the reviewers' comments. Changes are highlighted in red in the text.

We also would like to inform the editor that ML Benot who was initially in the list of authors has asked us to be rather included in the section "Acknowledgements", considering that her contribution to this article was not enough significant. Although we highly appreciated her contribution, we have chosen to respect her decision.

## Response to RC1

*The manuscript is well written and the results are interesting and important for further grassland studies with reference to climate impact research. As the authors mention in the manuscript, to obtain reliable evidence of climate change effects on forage quality, it is important to consider all relevant site conditions and climate effects.*

AU: We thank the referee for his/her work, helping us to improve our manuscript.

*However this manuscript still raises some questions, especially in the methods section:*

*Line 160-167: I wonder how it was determined which was the most appropriate mixed model. Was the model always used as a full-factorial model with all interactions or was the model reduced for non-significant interactions? Were the models evaluated for their goodness of fit, e.g. based on the Akaike information criterion? And which covariance type was used (e.g. first-order autoregressive process)? I think the model application should be described in more detail.*

AU: We thank the reviewer for having noted this omission. All main factors and interactions were included in the model which was not reduced for non-significant interactions. For each variable analysed, data were subjected to three covariance structures: compound symmetry, autoregressive order 1, and unstructured covariance. The covariance that resulted in the smallest Akaike's Information Criterion was used. This information has been added in lines 170-172.

*Some minor things:*

*Line 78: what kind of extensively management, e.g. how many cuttings, fertilization regime?*

AU: The field site is a long-term fertile grassland (clover rich) managed by a combination of grazing (3-5 grazing periods) and cutting (1 cut year). One organic fertilisation occurred at the end of the winter period. This has been added in lines 81-82.

*Line 80-81: it may be of interest to know what the dominant plant species in the botanical composition are.*

AU: At the onset of the experiment, five species contributed to 70% of species composition, *Trifolium repens*, *Lolium perenne*, *Holcus lanatus*, *Agrostis tenuis*, *Alopecurus pratensis*. This information has been added in lines 84-85.

Line 286: I would not start the sentence with an abbreviation.

AU: This has been changed.

## Response to RC2

*The manuscript by Niderkorn and others explores the effects of climate change and extreme events on forage quality. They find that the effects of CO<sub>2</sub> fertilization and extreme drought on forage quality, especially the neutral detergent fiber:nitrogen (NDF:N) ratio, somewhat offset each other. The interaction between climate change and plant chemistry is important to understand; many studies seem to only want to study production quantity and not nutrient quality. From this perspective the study is highly timely.*

AU: We thank the reviewer for revising our manuscript and helping us to improve it.

*At the same time, the text seems to be put together rather haphazardly. As one of very many examples, the word 'associated' appears twice in one sentence on lines 19-20.*

AU: One of the two words “associated” has been changed for “applied”.

*Starting at the first sentence, 'the natural one' sounds awkward; this sentence could be rewritten 'This study was aimed at analyzing changes in botanical and chemical composition and the in vitro rumen fermentation characteristics of an upland grassland exposed to elevated carbon dioxide concentrations and drought'.*

AU: This sentence has been changed by removing the reference to “natural” light intensity, not necessary here.

*The text really needs quite a bit of work to become publication-quality and a simple automated grammar and language checker would go a long way toward improving the paper.*

AU: We thank the reviewer for this suggestion. For the revision, we sent the manuscript to a language editing service for an in-depth reading and improvement.

*Section 4.1 demands a bit more explanation. How did the control treatment become drier and warmer than climatic conditions?*

AU: The aim of this study was to measure ecosystem responses to elevated CO<sub>2</sub> and to a summer severe drought in combination to a heat wave (ECE), in the context of future climate change. For this reason, we applied from the beginning of the experiment and for all treatments less precipitation and higher temperature compared to actual climatic conditions (all monoliths were exposed to a climate scenario forecasted for 2050). This allows comparing effect of elevated CO<sub>2</sub> and ECE under these drier and warmer conditions. A clarification has been added in the Material and methods section for clarification in lines 73-75, and in the section 4.1 in lines 232-235.

*On line 225, are the differences in digestibility due in (perhaps large) part to differences in species that dominated in the different treatments? The results section, which was rather terse, could benefit from more detail on species-level changes that may or may not have occurred in the experiment with large implications for digestibility. These findings need to be described in a bit more detail to help the reader understand if findings are dominated by plant chemistry responding to climate variability and atmospheric change, or simply by the plant community that was growing in the different treatments.*

AU: The suggestions made by the reviewer are interesting and we have conducted additional statistical analysis to provide more elements to understand the changes in plant community chemistry and fermentation parameters. In particular, relationships between above-ground biomass characteristics, chemical composition and in vitro rumen fermentation parameters were analyzed with non-parametric Spearman correlation tests. The results are shown in the Table S4 and Figure S1, in lines 200-203 and 219-228, and some elements have been added in the discussion.

*I feel that there is enough material here that is novel to warrant eventual publication, but readers will want to know why plant community chemistry has changed (see also Lee et al. cited on line 55). Is it the community, the chemistry, or both? The information is available and whereas the authors note minor differences in species (Fig. 4) there are important differences among groups (Fig. 3) and I was not fully convinced - but could be - that these shifts make a minor impact on digestibility. Adding a relative abundance effect, or accounting for relative abundance to the statistics presented might help do so, or perhaps by simply expanding the results section to add more detail. Bonferroni corrections likely apply to the multiple statistical comparisons in Tables 1, 2, and 4.*

AU: See our response in the previous comment about our additional statistical analysis. Regarding the “important differences among groups” evoked by the reviewer, we realize that the figure 3, reporting data of abundances for functional groups, may have been confusing because the pie charts showed large visual differences. Actually, there was no significant statistically differences among treatments due to the large variability among replicates. Thus, we have chosen to remove the figure 3 to keep only the data in the Table S1 and the p-values in the Table S2.