Reply on RC2
Veronika Forstner et al.

Author comment on "Response of water fluxes and biomass production to climate change in permanent grassland soil ecosystems" by Veronika Forstner et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-100-AC2, 2021

We thank for the positive feedback and helpful comments. We agree that the manuscript in some parts is not easy to follow and that the key results are not sufficiently highlighted. When revising the manuscript, we will shorten sentences and highlight the key findings. Please see a list of key finding:

Elevated temperature reduces the amount of non-rainfall water and thus total precipitation particularly during the growing season. This is found both in the manipulative and the observational approach.

Elevated temperature increases actual evapotranspiration and aboveground biomass in energy-limited grassland ecosystems but has the opposite effect under water-limited conditions. This is most evident from results of the observational approach, but also found in the manipulative experiment during exceptionally dry periods.

Results of both approaches suggest that elevated temperature reduces seepage and thus groundwater recharge in energy-limited grasslands, but not necessarily under water-limited conditions.

Elevated atmospheric CO$_2$ is found to reduce both evapotranspiration and aboveground biomass in the manipulative experiment. Possibly, grassland productivity is adversely affected by reduced evaporative cooling of plants or adaptation of the plant species composition.

Manipulative and observational climate change experiments complement one another. Thus, the two approaches should be combined.

We intend to improve the discussion, in particular, with respect to the comparison of the two approaches. We also consider deepening the discussion of water availability, because the importance of the hydrological status (water-limited vs. energy-limited conditions) is a key finding (see the list above) of our work. To this end, we propose to address effects on soil water storage in section 3.3 and rename the section heading correspondingly (e.g. “Seepage and soil water storage”). Water availability is also addressed by indicators (in particular, QP and AI). We will clarify this both in the method part and the discussion of the manuscript.
Thank you for the detailed specific comments. We will consider all above-mentioned points in our revision.

We plan to deepen the discussion of the differences between the two approaches for estimating the impact of climate change (in particular, their limitations and advantages) as well as of (soil) water availability (see our above response to the second point of the general comments).

We agree that the sentence about the lysimeter solely enriched with CO2.... was not clear. The reviewer is right that for 2017 and 2018 data is available only for one lysimeter enriched with CO₂, whereas for 2015 and 2016 the average of two lysimeters is used. The average of the two lysimeters is meant here, and thus the sentence should read: "The lysimeters solely enriched with CO₂ ... "