

# ***Interactive comment on “Wintertime carbon uptake of managed temperate grassland ecosystems may influence grassland dynamics” by Genki Katata et al.***

## **Anonymous Referee #2**

Received and published: 11 October 2019

General Comments: In general, the idea to look at wintertime carbon dynamics in grasslands and to include (biological) processes considering also cold stress in process-based models is very important. However, in my opinion this study doesn't make full use of this potential and is not very innovative. The 'new' model is based on integrating an already existing grass growth model into the SOLVEG model. However, whether this leads to an improvement or not is not analyzed (comparison of SOLVEG with and without the coupling of BASGRA) although it seems like relevant observational data exist to evaluate model performance. The model is run with pre-determined parameters and running the model under two scenarios, where one is determined to more or less shut down grass physiology and photosynthesis during low temperatures,

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leads in this case – unsurprisingly – to less carbon uptake /productivity during wintertime. While the outcome for the other processes might be more interesting, they are simply the result of how the model is set-up in the first place... At least, this is discussed to some extent in the last paragraph.

Evaluating that the underlying processes of the model actually represent the 'true' ecosystem processes and calibrating the model parameters using an optimization process and the observational data, the model could be used to investigate differences between the study sites or to analyze different (climate) scenarios.

Specific Comments:

Line 65: 'Observational data are used...' – what data exactly? How is it obtained/measured?

Line 121 ff, 2.2 Empirical parameterizations for cold acclimation: why not use an optimization algorithm to determine the parameter(s) for the two sites and compare if there are significant differences?

Line 154/155: 'Since the lack of the data, most of the micrometeorological and hydrological parameters for SOLVEG runs are from previous studies conducted at the study sites' Lack of what kind of data? Studies from previous years? Are those comparable to the actual years?

Line 158/159: 'The unknown parameter, the threshold temperature for cold stresses [T<sub>ph</sub> in Eq. (2)], is manually determined' – based on which values? Are different values somehow compared and evaluated?

Line 187/188: it seems like that at Graswang there was more snow than in Fendt even during the warm winter. In general, are the climatic conditions comparable at the two sites?

Line 200: what about the results from Graswang?

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Line 202: 100 gC m<sup>-2</sup> – cumulative? per year? Per season?

Line 202: changes in leaf biomass in spring in the active scenario were lower because the starting leaf biomass was higher

Line 210/211: 'In particular, the model reproduced the low or near-zero CO<sub>2</sub> uptake during the normal winter period at the Graswang, regulated by the lowering of soil temperature due to snowfall'. In line 99 you write: 'When snow covers grasses, no photosynthesis is assumed to occur...' so this might actually (also) be a reason for the observed model output!!

Line 218: 'high CO<sub>2</sub> uptake rate at low altitude during winter was likely explained by high levels of physiological activity of grasslands' – what else could be the reason? Isn't the interesting question what influences physiological activity of grasslands during winter?

Line 222-226 this whole paragraph is not written clearly. Line 223: 'The study site' – which study site, Fendt or Graswang?

Line 230: 'annual mean air temperature (MAT) of less than 8 °C' should be 'more than 8 °C'

Line 247: 'increased annual GPP to 100 gC m<sup>-2</sup>': from Fig. 6 it seems higher, the bar just for spring shows already about 600 gC m<sup>-2</sup> (which seems quite much for a grassland)

Line 247f: 'increased annual GPP to 100 gC m<sup>-2</sup> due to cold acclimation in the active scenario': before you write that acclimation leads to a decline in photosynthetic capacity (see line 224-225).

Line 248f: 'but the above-ground biomass of the first cutting simulated in this scenario was less than that in the dormant scenario' – actually, in Fig. 5 you show that leaf biomass in the active scenario at the first cutting is higher or as high as in the dormant scenario... The change from March to May is smaller, because the value in March is

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already much higher for the active scenario... See also comment to Line 202.

Fig. 4: So the results for Fendt are always with  $T_{ph} = 1\text{ }^{\circ}\text{C}$  and for Graswang  $T_{ph} = 11\text{ }^{\circ}\text{C}$ ? How do the results look for the respective other simulation? Table 3 is more or less a repetition of Fig. 4; ME and RMSE could also be included in Fig. 4.

Supplement: 'Modeling grassland vegetation growth and development' – the whole paragraph contains a lot of repetition of what is already written in the main article's Material and Methods section.

Technical corrections:

Line 26: 'to a low' delete 'a'

Line 27: delete 'being dormant'

Line 33: 'it is necessary to understand the response of grassland productivity to changes in snow cover conditions in order to...'

Line 37: delete 'of winter stress'

Line 75: 'are' instead of 'were'

Line 83: delete 'either'

Line 91: 'consists of both'

Line 130: 'leaves' instead of 'leaf'

Line 131: 'layers'

Line 140: 'of the sites'

Line 141: delete ', which is an area'

Line 147: H<sub>2</sub>O subscript

Line 158: 'The threshold temperature for cold stresses...'

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Throughout the manuscript: 'study period' instead of 'simulation/calculation period'

Fig. 2: check panel labels

Fig. 3: check panel labels in the caption.

Fig. 4: Adjust axis min/max to the min/max of the data. Check panel labels (2 times panel d )

Fig. 5: leaf not lea f

Fig. 6: red lines? Black lines? Should be grey bars / orange bars

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