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Interactive comment on "Growth response of temperate mountain grasslands to inter-annual variations of snow cover duration" *by* P. Choler

Anonymous Referee #2

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The manuscript from Choler entitled "Growth response of temperate mountain grasslands to inter-annual variations of snow cover duration" shows a novel analysis conducted in the French Alps using satellite data and downscaled meteorological forcing to determine: first, the relative contribution of the growing season length and maximum normalized difference vegetation index (NDVImax) in determining the inter-annual variations of primary productivity; second, to evaluate the effects of snow-cover on phenology and productivity. Last but not least, Choler analyzes the sensitivity of the integral of NDVI to inter-annual variations of temperature and precipitation during the growing season. By using a hierarchical path analysis, the author concludes that inter-annual variations in the integral of NDVI are driven by year-to-year variations in the length of the snow-free period. The author also demonstrates that the period spanning from peak standing biomass to the first snowfall accounted for two thirds of NDVIint. The

article is clearly written, despite many typos that need to be corrected (please see minor comments below) and the analysis is well conceived. The result that the integral of NDVI from the peak of the season to the first snowfall controls the inter-annual variability of the productivity in these ecosystems is novel and interesting. As well as the combined use of the normalized difference snow index (NDSI) and NDVI to derive the length of the snow-free period. I strongly recommend this paper for publication in Biogeoscience.

However, I would encourage the author to describe better some of the assumptions, which can have important impacts on some of the results. For instance the assumption that the integral of NDVI times PAR is the productivity of the ecosystem is strong. In these ecosystems NDVI tends to be quite impacted in the senescence period (mainly due to the dry/green ratio and canopy structure), vegetation indices more related to the green biomass can better approximate productivity in the senescence period. Enhanced Vegetation Index could be in this case a good substitution of NDVI, as well as other indices based on the red-edge portion of the spectrum. An analysis showing that the integral of EVI and NDVI in the senescence phase are unbiased would be convincing. Also the assumption that the light use efficiency is constant across all the 121 sites used is strong. In my opinion using a light use efficiency model for this analysis, that links timing and integrals of NDVI, is probably not going to change the main outcomes, but I would encourage the author to mention that the direct translation between integrals of NDVI times PAR and productivity is not always robust. At page 20 line 8 the author assumes that LUE is constant in each polygon. But in my opinion here is maintained constant across all the polygons. Please discuss the limitations.

As mentioned before, the combined use of NDSI and NDVI is of great interest and a novel contribution to the field. The use of the criteria NDSI/NDVI < 1 to estimate the length of the snow-free period is arbitrary as almost all the thresholds applied in the phenology field. I suggest testing how much sensitive is the snow-free season length to different selections of NDSI/NDVI. Would be also beneficial an evaluation of

the threshold using data from high-resolution satellite data or in sites with phenological cameras where the snow-free period can be easily identified.

At page 15 line 25 the author writes

“Essentially, the two contrasting scenarios for the initial period of growth observed in this study were either a fast growth rate during a shortened growing period in the case of a delayed snowmelt, or a lower growth rate over a prolonged period following a warm Spring”.

The author discuss this statement as follow:

“Alltogether, these results strongly suggest that intrinsic growth constraints limit the ability of high elevation grasslands to enhance their growth under ameliorated atmospheric conditions. Other severely limiting factors – including nutrient availability in the soil – may explain this low responsiveness . . ..”

I fully agree with this explanation, but there are few papers recently published that showed that another explanation could be to the different phenologies of the different species/communities of the grassland. For instance, consistently to this study, Julitta et al., (2014) shows a lower rate of increase of the green chromatic coordinates (gcc) derived from digital repeated photography in springs with exceptional early snow-melt. However, Julitta et al found that the ecosystem-level phenology was the combined effect of the different phenology of the two main communities (forbs and grass) present at the site that respond in a completely different way to early and late snow-melt, and spring photoperiod and temperature. The interannual variability of gcc extracted for each community was instead less pronounced than the one observed at the ecosystem level. I would suggest to the author to add these considerations.

As a minor comment I suggest the author to double-check for spelling the article. For example: P2 line 15 “negligeable” P3 line 24 “seasonaly” P4 line 1 “reponse” P4 line 23 “reolution” P16 line 11 “Alltogether”

Just few examples but there are many in the text

Fig 3. Please use different colors/patterns for PG and PS. White and light grey are not really recognizable.

References

Julitta et al., 2014. Using digital camera images to analyse snowmelt and phenology of a subalpine grassland. Agricultural and Forest Meteorology. 198-199, pp 116-125

Interactive comment on Biogeosciences Discuss., 12, 3025, 2015.