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Interactive comment on "The rising productivity of alpine grassland under warming, drought and N-deposition treatments" *by* Matthias Volk et al.

Matthias Volk et al.

matthias.volk@agroscope.admin.ch

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Anonymous Referee #1 Received and published: 13 October 2020

> We are happy to read that the MS was received intelligible in general and we supply a comprehensive response to the issues that both reviewers identified. The reviewers' comments will help to substantially improve the MS. Below, please find the authors point by point replies. For ease of reading we quote the comments first, beginning with a '?' and start our responses with '>'

? First, I would like to see more information about the plant species composition of the experimental monoliths. Qualitative results can be informative too. This could be a few sentences in the methods. Photographs might also be helpful.

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> We added more information on species directly in the new M&M. But to keep the MS as lean as possible we refer the specialist to the Wüst-Galley et al. (2020) paper on functional group responses in the same experiment and do not add photographs.

? Generally I would be slightly concerned about the inference obtainable from the elevational gradient. If many factors change in a correlated fashion along the gradient, such as temperature, moisture, and historical human/grazing pressure, it is hard to tell which factor is the driver.

> Indeed, in a complex system with a high number of interacting factors, it is usually not possible to point out one driver. Here, the soil moisture and temperature, resulting from the Climate Scenario (CS) site at a specific altitude, both drive the plant productivity response (cf. Fig. 2 A/B), but the fertilizing N deposition does not (cf. P values in Tab. 3). The management history of the sites of origin is very similar, but in concert with the strong edaphic factors an effect on the present plant communities cannot be excluded. We regard this element of heterogeneity as an advantage, as it is a factor that supports the general applicability of our results.

? I understand the limitations of the design and I don't think it's necessarily a flaw, but this is something that should be addressed more openly. This is also the case when discussing how soil moisture integrates information on both temperature and moisture; this could also be viewed as confounding the effects of temperature and moisture.

> True, the moisture of a Climate Scenario is not independent from the temperature. For this reason we termed the sites at different altitude 'Climate Scenario' and analyzed the data accordingly. We assumed that the unavoidable temp $\times$ moisture interaction closely resembles true climate change conditions, rather than an experimental manipulation of temp or moisture alone. To create a situation where this confounding is resolved, we introduced the irrigation treatment (P = 0.012; Tab. 3), that generated temperature independent moisture conditions. This concept is mentioned in chapter '2.3 Irrigation treatment'.

? One other point I would like to raise about the inference is that the warming treatment is confounded with site of origin. For example, the communities subjected to highest warming were those that were moved from the highest elevation. Therefore it is difficult to say whether the different levels of warming, or the composition of plant and soil communities from each of the sites of origin, led to the different productivity responses. This should be addressed as well.

> Different from the referee's interpretation, we do not create a warming treatment by transplanting turf monoliths from origins of different temperature to a common site of uniform temperature. Instead, all sites of origin have very similar temperatures and altitudes. But the experimental site, where the turf monoliths are transplanted, contains 6 climate scenario sites along a c. 700m altitudinal gradient. Thus, six different climate treatments are established. This misunderstanding was already clarified in an earlier round of comment/response. We will pay maximum attention to avoid this misunderstanding in the future.

? For reproducibility, please make the code and data available in a repository so that readers can reproduce the results of the statistical analysis. This is especially important for the mixed model specification. Sometimes it is difficult for the reader to determine the exact model specification from the verbal description but it is easier if they can see the code.

> This is a difficult issue. We respond to the specific statistics issue of the 'gamma' statement below at the comment on l. 216. If there are specific credibility issues, or data and code were to be used for further analysis, we would be happy to provide the necessary information at the desired level of integration. But we believe that the code and the carefully prepared dataset to go with the code does not prove anything. Alternatively, the raw data from the field are incomprehensible without a very, very extensive manual. Ultimately, even with all the data at hand, an (overall) 6 year, landscape-scale, climate change field experiment is not reproducible.

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? 2 Line-by-line comments ? Line 10: The abstract does a good job of stating the results of the study but it does not do a good job of stating the motivation, novelty, or broader significance of the study from the outset. Please revise accordingly. > Helpful point. For the sake of brevity, we had opted for elaborating motivation, novelty and broader significance of the trial in the Introduction section. But we will add two lines in the Abstract that, together with the headline of the manuscript, will give the expert readers the necessary information to decide whether the article is of interest for them.

? Line 55: The claim that multifactorial experiments necessarily will improve predictions is debatable. Please expand on the reasoning behind this claim.

> The paragraph following our claim on l. 55 (l. 55-67) is dealing exclusively with the interpretation of multifactorial (or multilevel) vs. unifactorial experiments. It contains seven references to support the argument.

? Line 76: The hypotheses need to have a little more justification or explicit statement of the reasoning why the particular directions of the effects and interactions are expected. For example, are there other studies that show similar effects or are the expectations derived from first principles?

> Indeed, our hypotheses must be understood in the context of the Introduction, but the Introduction is not reiterated in the hypotheses. We aimed at making the Hypotheses the 'Conclusion' of the Introduction. For example, we hypothesize that '1) The effect of warming on plant growth would be beneficial at moderate warming levels, but detrimental at high warming levels.' (l. 76-77). As an introduction to the issue, the Introduction section mentions how warming at high altitude vs. lowlands reduces temperature growth limitations, rather than causing heat stress. Studies on the warming effect in cold environments (in part with inconsistent results) are discussed in detail (l. 44-54).

? Line 85: It is interesting that southerly exposed slopes were chosen for the study. They tend to be drier and warmer than slopes with different aspect at the same eleva-

tion. I would expect the plants living in these microclimates to be especially responsive to the warming treatment. Is this something worth briefly mentioning?

> True, southern exposure is warmer and drier. A uniformly southern exposure (and identical altitude) as opposed to different exposures was chosen to minimize differences in climate (temperature, moisture, radiation) between sites of origin. Also, in this region of the Alps the majority of southerly exposed slopes is used as summer pastures. In contrast, the more the slopes are pointing away from the sun, the more likely they are forested and not suitable for our grassland research. Surely one may assume that an adaptation to heat and drought has occurred in these plant communities, resulting in an improved tolerance against extreme events. This may make them more likely to outcompete plant communities from moist and cool habitats, as those grow warmer and drier. We do not see though, why plants from warm and dry habitats should be more responsive to warming, judged on a productivity increase/warming basis?

? Line 106: A picture says 1000 words. It would be great to have some photos of the environment at the study sites, either as a main-text figure or as a supplement.

> That is a hint we were waiting for ïĄŁ We would honestly love to show the site and the landscape it is set in. We will immediately start exploring the options. In a paperless publication environment, this should not be impossible!

? Line 116: Similar to above, it would be nice to have a picture of the experimental setup.

> Cf. above

? Line 150: Is there a justification for the threshold for growing degree days being set at 0C? The same goes for the 40% soil volumetric water content threshold.

> Indeed, the many degree day baselines for crops or other individual species are usually higher. They are empirically established to describe plant development stages that are important for plant cultivation or study. We chose a 'generic' 0°C baseline, because

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in a mountain environment already low amounts of thermal energy play an important role. Also, lacking a single target species to focus on, but working on a multi-species community instead, none of these 'personalized' baselines appeared applicable. The SWC < 40 %-threshold does not imply plant growth limitation. Instead, it is an empirically developed contrast for differences in the soil moisture status between the CSs and between years. More time below the threshold simply means a drier period drier than less time below the threshold. This is also described in l. 158-161.

? Line 196 (statistical analysis section): I'm not sure I understand the reasoning behind assigning CS as a fixed effect but site of origin as a random effect. From my reading of the methods those are the same thing. Can you please clarify this?

> As clarified above, CS represents the climate scenario treatment along the altitudinal gradient at the AlpGrass experimental site. Because we wish to make specific tests and statements of the effect of the CS (or even one specific CS), this treatment has to be specified as a fixed effect, similar to the irrigation and the N-deposition treatments. By contrast, 'site of origin' represents the grasslands of the region where the monoliths were excavated and later transplanted to the AlpGrass site. We are not interested in and do not make any tests and statements on the effect of the grasslands of origin, therefore this variable should be fitted as a random factor.

? Line 216: Please include some details on the GAM fitting procedure, such as functional form of splines, etc. Were the defaults from the mgcv package used? If important inference is drawn from the GAMs, it would be good to assess the sensitivity of the results to choices made in the GAM fitting process. As written, it is not reproducible.

> Indeed, we used the defaults from the mgcv package, which one exception. The 'gamma' statement of the gam() function has been increased slightly to increase the degree of smoothing (to result in a smoother fitted line). This, however, did not (or only marginally) influence the inference and conclusions drawn from the model, i.e. P values for smooth terms reported in the main text and Tables S4 and S5 were highly

significant in either case. To improve clarity, we provide some more information on the GAM specification (including the type of splines) in the Supporting Information.

? Line 277: Because all columns of Table 4, besides the two leftmost, are in the same units (mean and SE of aboveground biomass yield), it might be better to convey the information in this table with a figure. Currently it is difficult to visually extract the most salient patterns from the table. If you do not want to use a figure maybe another possibility would be to use colors or cell fills to show where the highest values in each year were recorded.

> We actually visualized the response at the different CS, but for clarity reasons we did not divide the data by years. Please compare figure 1. Plotted over the altitude of the respective CS it show yields, aggregated by N deposition treatment (Fig 1A) and by irrigation treatment (Fig. 1B). Figure 2 shows productivity response per CS and per irrigation treatment over degree days (Fig. 2A) and the soil moisture (Fig. 2B) proxy. We believe that four panels displaying the productivity of the different CS is already a lot. Therefore, we opted to use a table for the year by year information.

? Line 281: I am confused why -7.7% is described as an increase, is it a negative or positive change?

> When viewed on the computer monitor l. 281 always says +7.7 % ('plus7.7 %'), not -7.7%, in all versions of the document. Thus 'increase'. Maybe there is a .pdf > printer problem, in case you worked on a printed copy?

? Line 289: Refer to the statistical test result (I am assuming this is Table A2?) that supports the statement that there was no significant interaction between N treatment and CS or irrigation.

> Thanks for drawing attention to this. We added the reference. It is actually Table 3, as referred to in the previous line, where the non-significant single factor N treatment is reported.

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? Line 316: "climate scenario warming" is a confusing phrase. Do you mean warming consistent with some particular climate scenario?

> We tried to improve clarity and the sentence now reads 'We found a substantial and significant positive effect of climate scenarios, equivalent to warming of up to + 1.8 °C (Apr. – Oct. mean) on aboveground biomass of subalpine grasslands (up to +44 % yield).'

? Line 390: I found this paragraph to be a little confusing. Are you referring to results from the present study or previous studies in the literature? Also, because you mention specific species responses to N addition from other species, it would be more interesting if you would draw a more direct connection with the present study. Were there any individual species that you can point to their responses?

> Sorry for being unclear. We improved the wording of the two paragraphs concerned. With the paragraph you mention, our discussion of N-effects in previous studies is contrasting single key species responses with whole plant community responses, discussed in the previous paragraph. In the next paragraph a direct connection with the present study and specifically with the response of Carex ssp. is made, to explain the lack of an N-response in our experiment.

? Line 425: I am not sure what the grounds are for stating that subalpine grassland productivity will increase with warming. Is it necessarily the case that climatic conditions will "move up" in elevation – maybe there will be novel and unpredictable combinations of temperature and moisture not tested here.

> We state that subalpine grassland productivity will increase with warming, because we found that yields in our experiment were increasing with increasing climate scenario mean temperatures (warming). The future climate may actually show those 'novel and unpredictable combinations . . . ' you mention. But even though there is no way to prove it, we indeed assumed that climatic conditions will 'move up' under global warming conditions. The reason is that we considered it the most conservative choice, to keep

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the number of necessary assumptions as low as possible.

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