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Interactive comment on "Atmospheric N deposition causes carbon balance gains in a seven year field experiment in subalpine grassland" *by* Matthias Volk et al.

Matthias Volk et al.

matthias.volk@agroscope.admin.ch

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Response to RC1 We are pleased to read that the reviewer (R) concludes that our manuscript (MS) is well written and that the logic leading from the data to conclusions is clear, with the topic matching the scope of BG. Many comments will help us to improve the MS.

Response to general comments: 1) Mentioning lacking or only marginal statistical significance the R is touching the soft spot of this study. It was certainly not our aim to mislead the reader towards believing statistical support for our interpretations was stronger than it actually is. We would nevertheless appreciate to get the opportunity to present these data, because we think that both the similarity of response patterns found in soil organic carbon and the CO_2 balance, and the analogous results quoted from the literature support our attempt to present plausible arguments towards a mechanistic explanation for the observed changes. We agree to double check the text to make sure that no unintended implication of statistical significance is made. 2) We think the within treatment variability of the responses found comes from the biological heterogeneity of our experimental units. The turf monoliths we worked with were randomly collected from a ca. 2 ha area of (sub-) alpine cattle pasture. The plant community in the monoliths contained 107 vascular plant species (Bassin et al., 2013) alone and developed in a terrain where minute differences of topography have a huge effect on the microclimate and soil development. This makes it likely that starting conditions, including carbon budget, were quite heterogeneous and resilient towards changes resulting from the experimental treatments. Analysis of plant C yield (Table 1), representing the much more dynamic response of the vegetation exposed to the treatments, shows a highly significant N effect and N $\times$ N interaction. If deemed necessary we may introduce a short paragraph on this issue in the MS.

Response to specific comments: 1) Title well supported? We alternatively suggest 'Subalpine grassland carbon balance during seven years of increased atmospheric N deposition' as an alternative, to avoid misleading readers. 2) - Here, 'mature' means that centuries of similar use and six decades of almost identical management have led to a well-established, little dynamic plant and animal community. It implies a steady state situation with respect to the C budget. - Yes, we consider both atmospheric N and O_3 deposition as air pollutants. - To be more clear we suggest to change to 'and that effects of air pollutants are similar for plant yield, net ecosystem productivity (NEP) and SOC content, leading to . . .' - We think the hypotheses are clearly stated at the end of the Introduction. We would prefer to keep the short form in the Abstract, if possible. 3) What does the R mean by 'quantifying' cumulative plant yield? Like all other responses that we report in the Abstract, cumulative plant yield is given as a proportional change, compared to the control treatment. Absolute values can be found in Results. 4) We can add 'non-significant' here. 5) Certainly our conclusions go one step beyond the presentation of plain results. They contain interpretations deriving from the discussion and are based on the evidence presented in the MS. This evidence is critically assessed on the background of the relevant literature cited and found to form a plausible line of argument. Therefore we think it is reasonable to come to the conclusions as stated here. 6) The R states that, different from the Alps, N deposition is not lower at high altitude in other mountain ranges. The source of this statement is not further specified, but we are aware that in other well researched mountain areas like the Colorado Rockies Front Range local effects like differential heating at low altitudes, associated with convective thunderstorms at higher altitudes, create an uplift of airborne (mostly) NH_4^+ and NO_3^- from nearby metropolitan areas and power plant sites (Burns, 2003). This leads to higher N deposition at higher altitudes. Such local effects are likely to occur elsewhere, too. We consider this an exception from the general rule that we stated (referring to the European Alps) in the Introduction. But we assume that the R refers to a situation where, on a larger than local scale, an increasing amount of precipitation (with altitude) creates an overcompensation effect for the declining N concentrations that results from altitude and/or distance from large sources of atmospheric N. It would be very interesting to get further information on that. 7) CO_2 enrichment is part of the comprehensive review originally quoted here. But there is other evidence from 'no CO_2' experiments, too. We now quote Neff et al. 2002 instead. 8) We appreciate how carefully the R deals with the statements we are trying to communicate. All three suggested points for a new formulation of the hypotheses can be found almost literally in the original text. The two-paragraph system we chose, instead of the suggested three paragraph solution, reflects the general design of the manuscript. It distinguishes time or interannual changes (mostly weather driven) on one side, and air pollution driven effects on the other side as organizing criteria. In order to remain consistent here, we ask R and editor to accept the original version. 9) We have C/N analyses data on the eleven most frequent species (60% of the cover) and random 'whole canopy samples' of the monoliths. For the purpose of estimating the plant biomass contribution to total ecosystem C content we used an averaged plant material C concentration of 47% for above- and belowground plant material, irrespective of treatment or year. For improved clarity of this paragraph in the MS we suggest: » Aboveground plant biomass was cut annually at peak canopy development (end of July), at 2 cm height. The harvested material was oven dried and weighed to yield dry matter mass (DM). For details please refer to Bassin et al. (2007). Belowground root biomass was assessed from soil cores covering a subset of the monoliths (Volk et al. 2014). DM masses were expressed as g C m-2 based on plant biomass C concentration (C/N elemental analyzer measurements (Bassin et al., 2015)). Masses of four plots each were combined and averaged to match the lumping rules developed for soil sampling (compare below). Tests for effects of N- and O3 deposition on mean plant C concentration yielded no results and a common value of 47% was assumed. « 10) Temperature and precipitation data is in (Volk et al., 2014). We refer to it in M&M. For Fig. 1 we had earlier versions containing more meteorological data, but they turned out extremely crowded. Therefore we decided to use soil temperature alone. We think this is a good compromise. Precipitation data make sense with high temporal resolution (1 week). This would have required c. 360 more data points in the figure. Soil moisture is not a variable in our flux parameterization. Lacking a complete set of soil moisture data, we use global radiation and soil temperature instead (compare M&M). 11) Testing whether the 'hump' is a statistically significant result of a N $\times$ N interaction requires more than three treatment levels, independent of statistical significance of the N effect per se. To be clear about this we formulate: » Under air pollution treatment we found an unimodal/hump shaped response pattern of NEPcum (Fig. 3), but the significance of the N $\times$ N interaction is not testable with three treatment levels. «

Response to specific comments: 1) parenthesis corrected 2) deleted 3) corrected 4) done as suggested 5) and 6) We are aware that scientific writing requires a maximum of clarity, which includes avoiding redundancies. Here we decided to make an exception and introduce <1 line of extra text to allow readers to quantitatively compare our results to their own or to literature values, without applying pencil and ruler to our figures. We did this in a few selected cases throughout the MS. 7) The use of 'marginally significant' as a synonym for p values < 0.1 is indeed a matter of debate. Some consider $p \geq 0.05$ marginally significant. Since the sentence quoted contains a reference to the table where the p-values are documented, the reader can easily make his/her own decision. As to whether it indicates a trend or not, I must say we did not do a trend analysis. To my knowledge two points in time are not yet sufficient for a trend analysis. 8) Sub-heading added as suggested.

âĂČ Literature cited:

Bassin, S., Volk, M. and Fuhrer, J.: Species Composition of Subalpine Grassland is Sensitive to Nitrogen Deposition, but Not to Ozone, After Seven Years of Treatment, Ecosystems, 16(6), 1105–1117, doi:10.1007/s10021-013-9670-3, 2013. Burns, D. A.: Atmospheric nitrogen deposition in the Rocky Mountains of Colorado and southern WyomingâĂŤa review and new analysis of past study results, Atmospheric Environment, 37(7), 921–932, doi:10.1016/S1352-2310(02)00993-7, 2003. Volk, M., Wolff, V., Bassin, S., Ammann, C. and Fuhrer, J.: High tolerance of subalpine grassland to long-term ozone exposure is independent of N input and climatic drivers, Environmental Pollution, 189, 161–168, doi:10.1016/j.envpol.2014.02.032, 2014.

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