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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.

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Response to RC2 (Veldkamp)

We are pleased with the great attention the reviewer (R) has given the manuscript and are confident that dealing with the constructive criticism will result in a better paper. Indeed all points raised touch relevant issues and are taken very seriously to give the reader a clearer picture of the experiment.

2nd §The R obviously is right in the assumption that containing the monoliths in drained plastic boxes has an effect on the water regime. The lack of connection with the surrounding soil excludes both capillary drainage under moist conditions and capillary up-

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lift under dry conditions. This issue is also part of the Discussion in (Volk et al., 2011). The R then goes from SWC effects to T effects and finds that higher T (resulting from lower SWC) might be responsible for C losses. This might indeed be a mechanism causing higher C losses through increased ecosystem respiration at higher temperatures. Unfortunately we have no soil moisture content values to compare between monoliths and the surrounding grassland. But we did soil temperature comparisons between the soil at the nearby site of origin and the soil temperature of the monoliths at the experimental site to test for potential microclimatic differences. We found that the monoliths were 0.25°C cooler on average (n.s.). This value is also in the Results section of (Volk et al., 2011). We will include this important information in the Methods section of the submitted MS and mention the issue in the Discussion. The minute cooling of the monoliths compared to the original site supports the Rs suggestion that temperature differences may be responsible for C stock increases. Our study found that soil temperature is by far the largest single factor determining the C balance of the mountain grassland. But judging from the sensitivity analysis mentioned already in the NEP part of the Discussion, we are confident that the temperature difference is responsible for a small effect on soil C stock only, if any.

3rd §As the R points out, also the amount of water used to apply the N treatment may have contributed to the increased C stock. In twelve applications per growing season we used 200 ml H2O each on the monolith surface of ca. 0.1 m2. Compared to the mean annual precipitation during the same period (853 mm) this is equivalent to an extra 24 mm or 2.8% precipitation. We assume this small amount did not exert a substantial effect.

4th §1) We will change wording ‘soil density’ to ‘bulk soil density’ as suggested. 2) Indeed soil bulk density is often decreasing in parallel with the input of fresh organic matter. This would imply a higher than assumed bulk density in 2003 and a smaller difference between SOC stocks in 2003 and 2010. Finding suitable literature values to estimate the degree of potential underestimation of bulk soil density at the start of

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our experiment is difficult. Carbon stocks in mountain (alpine) soils receive a certain attention today, but the bulk of the literature refers to land use change in the context of restoration measures for seriously overgrazed areas (e.g. (Li et al., 2007)) or forest regrowth after abandonment (e.g. (Guidi et al., 2014)) or afforestation (e.g. (Hiltbrunner et al., 2013). Also, many studies do not analyze concentration and density before and after the experiment, but only test for effects after the treatment period, assuming all observed differences to result from the treatment (e.g. (Martinsen et al., 2011)). We would be grateful to receive more specific hints to reports of density changes in comparable grassland if available.

To assess the potential error in a thought experiment instead, we assumed a 10% density reduction to coincide with the C concentration increase. In this scenario the 2010 0-20 cm depth sampling campaign would cover only 90% of the soil mass present in the 2003 sample. The resulting error comes from the fact that the 0-20 cm sampling of the reduced density soil of 2010 only goes to a soil layer that was at ca. 18 cm in 2003. Consequently, to compare C content changes in the 2003 soil mass-equivalent after seven years, the 2010 sampling volume would have to include an extra 10%.or 2 cm depth. At our site the C concentration at 20 cm depth is $\leq$ 4% (Leifeld and Fuhrer, 2009). Including this extra volume of low C concentration soil into the sample would have reduced the mean concentration from 6.7% to 6.43% (18 cm soil column of 6.7% and 2 cm of 4%). In parallel with the lower C concentration, the C stock of 2010 would have to be reduced by factor 0.96, resulting in a 7.2 kg m-2 C stock. The remaining stock-increase from 2003 to 2010 is only ca. 0.30 kg C m-2, compared to 0.6 kg when equal density is assumed. But at the same time the extra soil volume of a density of $\geq$ 0.76 would have increased mean bulk density such that the resulting stock would be 7.4 kg C m-2. Using this alternative calculation, introducing a density correction as suggested by the reviewer, the 0-20 cm carbon gain would be c. 20% less, only about 0.5 kg C m-2 instead of 0.6 kg C m-2. If we have the reviewers consent, we would like to introduce a condensed version of the above paragraph into the Discussion section of the MS to cover the sensitive bulk density issue.

5th §Wording issue with less specific ‘time’ vs. ‘interannual’ effects: We do not agree that ‘time’ is used inappropriately here, because changes summarized under ‘time’ are driven by ‘everything except’ N and O3 deposition. This includes weather, management and plant community effects. Therefore the use of an unspecific term is fully intentional here. But we follow the reviewer in so far as ‘time’ may unnecessarily irritate the reader and should thus be replaced with ‘interannual’.

6th §1) We will lump the mini paragraphs for better readability and remove the excessive part of the numbering. 2) The formulation ‘strong, yet statistically insignificant’ we use in the Abstract is actually wrong. We will omit this expression. As stated correctly in the Discussion, the statistical test for an N $\times$ N interaction is simply not meaningful, based on three treatment levels. But we would like to keep mentioning the shape of the cumulative NEP response to N deposition in the Discussion, because the parallel development of CO2 balance and SOC contents under experimental N deposition is important evidence for the conclusion that C sequestration is not consistently increasing with the rate of N deposition. The same response has recently been reported from alpine grassland by (Fang et al., 2014).

We avoid to mislead the reader about statistical significance of results and some may refuse to take notice of anything that is not statistically significant. But we believe that the careful design and the long duration of the experiment make this a valuable piece of evidence to be put at the readers judgement. âĂČ

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