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# *Interactive comment on* "Comparison of inorganic nitrogen uptake dynamics following snowmelt and at peak biomass in subalpine grasslands" *by* N. Legay et al.

**N. Legay et al.**

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We would like to thank Anonymous Referee #1 for his comments. We have addressed all the remarks and our responses to individual comments can be seen below.

General comments:

The reviewer made general comments which we addressed as follows:

1. Although the science question is interesting and relevant to the scope of Biogeosciences journal, the approach of comparing N dynamics at snowmelt period in 2010 with peak biomass period in 2005 seems not appropriate to justify the extent of gen-



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eralized discussion in relation to the seasonal differences in microbial and plant N dynamics in subalpine grasslands.

Reply: We agree with the reviewer that it appears difficult to accept the validity of seasonal variations study with measurements realized at different years. We acknowledged this limitation regularly throughout out the text so that the reader is warned that generalization should be taken with care (P11, L229-234; P15, L302-306). We also now explicitly detailed that we presented such comparison based on the work of Edwards and Jefferies (2013) which showed that intra-annual differences observed between snowmelt period and peak biomass in terms of nutrient pools and fluxes were higher than inter-annual differences. We added references to this previous work when needed to better support our comparison. Finally, we reworded the objectives section of the introduction (P5-6, L 110-113) to clarify the purpose of our work.

2. The N deposition from snow, environmental conditions (temperature, rainfall, wind, humidity, etc.) of post snowmelt period and the botanical composition of the grassland are key factors for N partitioning at the peak biomass period. None of these were discussed or taken into account for their conclusions.

Reply: We agree with this remark. Indeed, many biotic or abiotic parameters might have affected study sites on the short or the long terms. Yet, our study sites were all located in close vicinity (same sub-watershed, same bedrock, same land owners, similar rainfall, temperature, wind, humidity and N deposition (Clement et al., 2012)), therefore limiting some of this variability. We acknowledged this limitation in the text and added a sentence to highlight these potential effects (P15, L302-306).

3. In general manuscript was difficult to understand and the results presented in the text were hard to follow. The manuscript should have been concentrated mainly on the findings of snowmelt period with a conceptual comparison to peak biomass period using the data of Robson et al (2010). There is repetitive discussion based on speculative pathways illustrated in Figure 5. The Table 2 should be accompanied by the measured



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averages and the data from Table 1 of Robson et al (2010).

Reply: We agree that the previous version of the discussion was difficult to follow. We used most of the suggestions from reviewer 2 to improve the discussion and the global quality of the manuscript. To limit the repetitive discussion, we suppressed a sentence and modified another one (P21, L433-434). The results and discussion from our original work during the snowmelt period are central to the paper. Concerning the pathways illustrated in Figure 5, we checked the text and made sure that they were referred to for original and distinct purposes (5 times). We do not think that Figure 5 is speculative as it is primarily based on results from seven published studies to which we added our own results. Regarding Table 2, most of the data of the measured averages are available in appendices C1 and C2 as well as in the Figures. Adding these values to Table 2 would results in an overloaded table. Finally, we do not think that the incorporation of Robson et al (2010) data would be allowed because these results have been already published elsewhere.

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Interactive comment on Biogeosciences Discuss., 10, 8887, 2013.