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Biogeosciences Discussions

Interactive comment on "Nitrogen retention patterns and their controlling factors in an alpine meadow: implications for carbon sequestration" *by* X. L. Xu et al.

Anonymous Referee #2

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The manuscript „Nitrogen retention patterns and their controlling factors in an alpine meadow: implication for carbon sequestration” describes a field study focusing N-input in a highly sensitive ecosystem. Moreover, the hypothesis was investigated that the N-species (ammonium or nitrate) is important for the retention of the nitrogen in the ecosystem. In general field investigation in high mountains areas are less often then in other ecosystems and thus our knowledge must be improved with respect to the sensitivity and the protection of these high mountain ecosystems. This manuscript is the 4th publication in a set of publications which describes this field study (Xu et al. 2003, Xu et al. 2004a and b). Due to destroying of one of the three plot only two plots are investigated which restrict the generalisation of the founded results. Fur-

thermore, the statistical design of the investigation is inadequate, since state of the art statistic for such investigations are randomised block designs. With respect to the described design it is impossible to distinguish between treatment effects and spatial heterogeneity in the ecosystem regardless the fact that the authors assure that “a block uniform in species composition and cover was selected”. In addition, it must be noted that correlation between two parameters doesn’t point out a causal relationship between these two parameters. Therefore the part “controlling factors” of the title must be reconsidered. In general the title is not appropriate for this manuscript. The relationship N-input and carbon sequestration is demonstrated in work 2 (Xu et al 2004a) in detail, but no new knowledge about this point is presented in the current manuscript. In my opinion this manuscript should be focused on the unpublished 15N measurements after 4 years. The time course of the pools and their 15N abundance must be analysed and then calculation of fluxes between the pools can be carried out. This will may be confirm the hypothesis that the N-form of nitrogen input is more important on the short time scale then on the long time scale, as described in the abstract. I am assumed that the reconstruction of the measured pools by a mathematical model will be helping us to expand our knowledge of the N-cycles in this ecosystem. More information about N-fluxes into this ecosystem must be presented So, overall, I am left with a feeling that this paper contributes little to our real knowledge about the N cycles in this ecosystem and the risk by atmospheric N input. It shows that ammonium and nitrate have different manner in the soil, but that is already commonly accepted.

Special comments

I think Figure 1 can be deleted, because the soil moisture is described in general in the text. However, if the soil moisture is available in this high resolution over the full 4 years, then this data seems to be very interesting and should be kept in the manuscript as a figure.

Figure 2 and 3. It is more interesting to present the time course of pools and the 15N

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abundance in the pools than this correlation after 2 weeks (why after 2 weeks and not with the data after 4 or 8 weeks or after 1 or 4 years?)

P2648 l 26 Soil temperatures must be decreased with increasing altitude, or otherwise the data presented in Table 1 is not consistent with Figure 2

P 2649 L 11-17 This paragraph is a description and no discussion

P2653 L 10 to 20 There are to many assumptions and no calculation errors. Is this the long term trend? E.g., is the calculation right if the N-loss increases with increasing input over long time? There are a lot of uncertainties in this calculation and therefore this is not acceptable for me in the present form.

Interactive comment on Biogeosciences Discuss., 4, 2641, 2007.