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Interactive comment on "Initial shifts in nitrogen impact on ecosystem carbon fluxes in an alpine meadow: patterns and causes" *by* Bing Song et al.

Anonymous Referee #1

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This study addresses potential responses of different ecosystem C fluxes to gradual increases in N fertilization. The main findings of this study provide evidence that N saturation of ecosystem C fluxes can occur in a short period of time (just over 2 years since the start of the N fertilization experiment). Key findings are shown in Fig 2 where differences in NEE and ER are clear between years and along the N fertilization treatment.

Despite the results indicate that N saturation may occur at increasing N fertilization levels, the underlying mechanisms explaining why C fluxes might get saturated with N inputs are not clear. The authors suggest that decreases in NEE and ER under greater N fertilization are due to decreases in plant aboveground respiration and soil microbial respiration. Looking at Figs 3 and 4, this interpretation is not really supported by results whereby plant aboveground respiration (in 2015; Fig 4a) seems to increase rather than

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decrease at N8,16,32 treatments compared to N0,2,4. Similarly soil microbial respiration does not seem to decrease much under N8,16,32 treatments (Fig 3d) and actually might increase under N32 compared to N16. My point here is that although NEE and ER trends are relatively clear, the mechanisms invoked here to explain these changes are not really supported by the results. There is a problem with results interpretation here that the authors need to deal with (see my comments below).

I think the authors should either better demonstrate that soil microbial respiration might play a role in mediating the N saturation effect or that other mechanisms are at play. It looks like that soil respiration in general decreases more convincingly under higher N treatments than soil microbial respiration. Also the explanation that greater standing litter might reduce plant aboveground respiration through reduced light availability makes sense but is not really supported by the results in Fig 4e for example.

Also in relation to results interpretation, the authors need to acknowledge that variability in their findings could be related to their very short-term study, which may not capture key changes in NEE and ER and the underlying mechanisms involved. I would expect that it will take 3-5 years of N fertilization to better clarify these.

Overall, the manuscript needs a thorough editing in relation to sentence structure and language especially abstract and introduction but in general all throughout the manuscript.

Discussion

I am not sure whether the explanation that: “The N saturation responses of ER and thus NEE are mainly caused by the decrease of aboveground plant respiration and soil microbial respiration under high N addition treatments in 2015” (page 11, lines 8-9), is well supported by the results. If I look at Fig. 4e I see an increase in aboveground plant respiration (i.e. Rabove) in 2015 under the N16 treatment and a slight decrease under the N32 treatment, which is however still higher than the N8 treatment. What I can see is an overall decrease of Rabove across all treatments in 2015 when compared to

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2014. Even the 'assumed' decreases in soil microbial respiration are not clear in Fig. 3d, actually it looks like that Rmic almost increases between N16 and N32.

Page 11, lines 9-11. I might agree with the statement that: "The decrease of aboveground plant respiration under N32 treatment is primarily due to that N addition stimulated plant growth and thus standing litter accumulation after plant senescence (Fig. S1)", but again this is not clear from the results shown. Fig. S1 might provide evidence of litter accumulation but is this the only treatment (N32), which was associated with an increase of plant litter? What about N16?

Again on pag. 11, lines 17-19, the authors suggest that: "The relationships between ER and soil microbial respiration (Fig. 6c) indicate that the decrease of microbial respiration contributes to the reduction of ER under high N addition rates in 2015", which is not really what is shown in Fig. 6c. This figure shows an overall positive relationship between Rmic and ER but this has not to do with increases in N addition rates. The role of N fertilization here is not clear mainly because there is no distinction between N treatments (al points are the same). The authors should show where the high N-addition-treatment points are positioned in this graph to make their explanation convincing.

Page 13, lines 16-21. This section does not provide a clear view of some potential mechanisms involved in the N saturation effect. I think the authors need either to make a more convincing case for a reduction of soil microbial respiration under N additions.

Conclusions need to be rewritten after a better interpretation of key results.

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