

Interactive comment on “Flower litters of alpine plants affect soil nitrogen and phosphorus rapidly in the eastern Tibetan Plateau” by Jinniu Wang et al.

Anonymous Referee #2

Received and published: 19 May 2016

The authors conducted a well-designed experiment to explore how flowering litter influences soil nitrogen and phosphorus status. I think this is an important topic that has not been well studied and deserves publication. However, I would also suggest the authors seek assistance with English grammar and translation where appropriate, as the impact of this paper is currently obscured.

The introduction of this paper is scattered and somewhat confusing. I think spending time re-organizing/framing this section will provide clarity for the results and discussion sections. Perhaps the authors could introduce the topic of flowering bodies and their higher litter quality (N content), then discuss how aboveground litter quality influences belowground biogeochemical cycling through microbial subsidies, and conclude with a

[Printer-friendly version](#)

[Discussion paper](#)



section discussing alpine ecosystems and evolved traits.

Lines 137-142 provide specific research questions that will be addressed by the authors. Currently they are a little unclear and seem to set up questions that are not directly tested. I would suggest refocusing on the major comparisons being made- is flower litter of higher quality than leaf litter? do these traits facilitate faster decomposition? does the time of litter fall influence ecosystem productivity?

The methods/materials section is generally clear. However, the authors do not provide details regarding their litter collection method (makes question 1 difficult to assess). The number of replicates and control treatments are sound. In my personal opinion I think it is important the field moves beyond litter bag experiments and mass loss. Litter bags exclude fauna and litter fragmentation, which contribute greatly to litter decomposition. While the authors used mesh bags with two layers of differing mesh size, the smaller mesh actually surrounding the litter still excludes faunal decomposers and minimizes biophysical perturbation. Since the study is focused on nutrient cycling more than soil organic matter mass loss/formation I think the litter bag approach is okay, but in the future it would be good for us to move beyond these techniques.

A-P is never defined in the manuscript. Correction factors for microbial biomass C and N are commonly employed, but are highly specific to soil mineralogy/sorption. Direct testing of recovery efficiency at a particular site should be assessed before a correction factor is applied.

Line 357: clarify which species were used to compare decomposition rates between flower and mixed litters.

Line 397: lignin/N and C/N ratios are commonly accepted as good indicators of decomposition rates under short time frames, but there is little evidence lignin is preferentially preserved in soils, compared with bulk soil, over long-time periods (Cotrufo et al., 2015 & 2015, Mikutta et al., 2005 Kleber et al., 2007 etc).

BGD

Interactive
comment

Printer-friendly version

Discussion paper



Line 467: microbial community composition was not directly tested (no sequencing/PLFA analysis etc) so it cannot be concluded that flowering litter increases nutrient status and therefore changes microbial assembly. There is support that MBC and MBN pools increase, but that could be due to faster turnover or growth, not necessarily to a change in species composition.

Line 499: the impact of this paper is significant and should be re-stated clearly in the conclusion.

Comments regarding tables: Make sure to clearly define variables tested in each caption. Table 1: it is not clear how species dominance is assessed (Y/N). Line 303, 324, 329 etc.: the authors are assessing N pools, not fragments. Table 3: DNN/DHN are not necessary; although defined as such in the text, NO₃⁻ and NH₄⁺ are clearer. Table 4/5: Define TP and A-P: α values of total phosphorus (TP)... Table 6: Mean values (not comparison medium values)

Comments regarding figures: Figure 1: It is very difficult to identify where the sampling sites are on the map because the elevation shading is so dark (either increase shading transparency or make text and symbols larger) Figure 2: Include the mean (n=X). Figure 3: Explicitly state the statistical analysis used (are the bars 95% confidence intervals/SE, or quantiles)? If the whiskers represent SE it seems impossible that the flower litter vs. leaf litter means are significantly different from each other. What are the values (mean, n=X)? Figures 4 & 5: Define the variables in the figure caption (dissolved inorganic nitrogen (DIN), dissolved organic nitrogen (DON), etc). mean, n=X. What do the boxes represent? What does deviation from the 0 line signify (significantly different at what level)? Figure 6: letters indicate significant differences (at what level, p=0.05)?

Comments regarding cited literature Overall, the authors seem well read on these topics. There are some citations that do not seem relevant to the paper the way it is currently written (for example, findings from tropical, agricultural, and Arctic sites). I think these findings are important because they show flowering litter quality influences

[Printer-friendly version](#)[Discussion paper](#)

soil nutrient status across ecotones, but this needs to be made explicitly clear. The authors also touch on soil organic matter formation/stabilization processes. There is a highly relevant body of literature that could be incorporated to strengthen these points (Kogel-Knaber, Sollins, Cotrfo, Kleber, etc).

Overall, I think the quality of this study is excellent. I would highly suggest a thorough language editing review is taken before the paper is published. Currently it is a little difficult to read and I think this obscures the interesting science.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-68, 2016.

BGD

Interactive
comment

Printer-friendly version

Discussion paper

