

Interactive comment on “Seasonal and interannual dynamics of soil microbial biomass and available nitrogen in an alpine meadow in the eastern part of Qinghai-Tibet Plateau, China” by Bo Xu et al.

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Overall, this is a largely descriptive project, but it is well presented and the overwinter data are valuable as those types of measurements are rare. The authors might work on describing which parts of their study are most novel to help the study be better found and cited within the literature. Response: We thank referee for the helpful comments. After discussing with co-authors, we thoroughly revised the manuscript and listed in supplement. I have some suggestions below on which topics to emphasize. The data are also remarkably "clean" for soil nutrient data with less heterogeneity of variance between dates than usual and no unusual "hot spots" of activity. The authors might discuss whether quality control measurements may have eliminated such points and

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if not, why the numbers are so consistent, which is not always the case for these types of studies. $n = 15$ is a reasonably large sample size so I do recognize that that is part of it. Response: In our study, three adjacent sites approximately 100 m apart were sampled, and five replicates at each site were collected. So fifteen soil samples were collected at each sampling time, and then the mean values of soil nutrient were calculated ($n=15$) (Page 6 lines 5-7). We thought these fifteen samples themselves would represent the heterogeneous soil nutrient status in the alpine meadow, and it might be the main reason that why you found the soil nutrient data with less heterogeneity of variance between dates. Actually, we did not take quality control to eliminate any points, and the numbers were so consistent because fifteen samples were collected at each sampling time. Abstract is solid. No complaints. Response: Thank you for your comment. INTRODUCTION I recommend the authors work to define their knowledge gaps better. There are several possible areas to discuss including location of study (including why it may or may not be different from other sites), the rarity of the overwinter measurements (there are probably just a handful of studies with this type of data), and finally, the microbial cultures are not often done in association with these types of seasonal nutrient measurements so that is worth mentioning too and describing which other studies if any have done this. The authors do mention these topics, but don't zero in on specifically what is not currently known and why it is important that we know that. I'm not saying this wasn't done at all—just that it can be done more and better. Response: Yes, we revised the introduction according to your comments (Page 4 lines 10-13), and we rewrote the research questions as “1) What are soil microbial and available N dynamics during the growing and non-growing seasons in the alpine meadow? 2) What are interannual patterns of soil microbial and available N dynamics in the alpine meadow? 3) What environmental factors affect these dynamics? 4) What are relationships between soil microbial biomass and available N pools in the seasonal frozen ecosystems?” (Page 4 lines 17-18 and Page 5 lines 1-4) L 15. I recommend removing these correction factors as it's widely understood that they are very ecosystem specific and hard to

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apply to sites in which they are not explicitly calibrated. Response: Thank you for your comment. But, we did not know the L15 in which page. Three parts of the meadow were measured. Some discussion is warranted as to the spatial configuration of the sampling and why they were pooled for analysis as a single site ($n = 15$). Response: Considering the soil spatial heterogeneity in the alpine meadow, we selected three adjacent sites for soil sampling, and five replicates at each site were collected at each sampling time. Thus, fifteen soil samples were collected at each sampling time, and then statistical analyses of soil microbial and nutrient dynamics in the alpine meadow were performed on these samples at each sampling time ($n=15$) (Page 6 lines 5-9). Figure 3, Fig. 7. Fig. 6B. These figures all show results that are already shown in the more detailed time courses. The authors can maybe report some of those values in the text if needed and eliminate these figures. If the authors feel this leaves the paper a little thin on figures, I would recommend exploring the relationships among the measured variables and environmental covariates using an approach such as a scatterplot matrix of correlations on a per-sample basis (ie one data point per sample, not averaged by date). Along these lines, providing the raw data as a supplement or as a link to an online repository would add value to the study. Response: We thank referee for the kindly and helpful suggestions. But we thought Fig. 3, Fig. 7, and Fig. 6B were indispensable for our study, because they intuitively and detailedly showed the intra- and interannual patterns of microbial and nutrient dynamics in the alpine meadow. I'm curious as to why the soil N numbers are so low-variance (particularly inorganic N). Were outliers eliminated before analysis? These types of measurements typically show substantial right skew and hot spots. Also TDN and MBN are often an order of magnitude higher than the inorganic constituents, but that is not the case here. These points warrant discussion. Response: We did not eliminate any points before analysis. The standard error (s.e.) was used for figure drawing might be the reason why you found the soil N datum with low-variance. In other ecosystems, the TDN and MBN are often an order of magnitude higher than the inorganic constituents, may because relatively high microbial activity will lead to

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high MBN and TDN accumulations in the soils. But in the alpine meadow ecosystems, low temperatures and N limitations may largely restrict microbial activity, causing relatively low MBN and TDN accumulations in the soils. Furthermore, alpine plants may largely uptake DON during the late growing season as inorganic N is exhausted. We think these reasons may lead to the TDN and MBN are not an order of magnitude higher than the inorganic constituents in the alpine meadow. The results section is serviceable but kind of boring with its descriptions of seasonal trends and what is "significant" or not sprinkled with un insightful p-values. I'd like to see more of a narrative structure tied to some hypotheses (eg hypothesis that there will be a crash in N availability at beginning of season as seen in other studies, a hypothesis that would be supported). Response: We thought you provided another paper writing habit that contain results and discussion together. But we preferred to separate the results from the discussion. This study would benefit from a photograph of the sampled sites. Response: Yes, a map of the study site was added into the revised manuscript (Page 6 lines 4 and Fig. 1). The paper is completely readable and generally well written. Still, it could use a onceover by a native speaker to fix the most challenging issues for non-native speakers such as proper preposition choice, a few cases of singular/plural mismatch, etc. Response: Yes, we will send the revised manuscript to a professional language editing company for the language modification during the final revised period. Conclusion: keep it focused on the seasonal questions and trends. Climate change is not really addressed in any way in this study and so it's not worth mentioning here. The study's value is in its contribution to basic understanding of soil nutrient cycling seasonality. Response: In our study, we found that the year-to-year variations of soil temperature and water condition were the primary environmental factors driving the interannual dynamics of soil microbial biomass and available N pools. Furthermore, the alpine ecosystems are sensitive to the future climate change. So we thought it was necessary to mention the climate change in the conclusion.

Please also note the supplement to this comment:

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<http://www.biogeosciences-discuss.net/bg-2017-66/bg-2017-66-AC1-supplement.pdf>

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