

## ***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.***

### **Anonymous Referee #4**

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#### General comments

This paper describes intra-annual and inter-annual patterns in soil nutrient availability (inorganic and organic N) as well as microbial biomass and community structure in alpine tundra. The investigators sampled soils monthly over a 3 year period, including both the frozen and unfrozen periods. This is an impressive data set and I'm not aware of another published data set that is nearly as comprehensive. For this reason alone I encourage the authors to continue to work towards the publication of this data set. There are some aspects of both the methods and the interpretation of the results which I question and these aspects in particular require more attention by the authors before publication of this paper. See more specific comments below.

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## Specific comments

Referencing: Some of the references are inappropriate. Specifically, there are many citations which are used to support statements about alpine systems which were not conducted in alpine ecosystems (E.g. Page 2 line 17 and Page 4 line 8 Edwards and Jefferies, Page 3 line 6 Buckeridge and Grogan, Page 15 line 4 Henry and Jefferies). Some references are missing (Page 14 line 3: reference for Alaskan tundra is missing) and others did not examine the phenomena they are used to support (e.g. Edwards and Jefferies did not examine the survival of microorganisms surviving in thin water films (Page 3 line 1)).

The methods are lacking some necessary details. The description of the 3 sites were vague: The sites are described as being at the “top middle and bottom of the meadow”. Were there elevational differences between the sites? How far is the distance between them? Further, were the soils collected in the winter kept frozen into analysis? Finally, was TDN measured only after chloroform fumigation? This is how it is described, but then it would be impossible to measure MBC and MBN. It would also be good to report days below -5C rather than just below 0C: -5C is often reported as when microbial activity significantly slows.

I also question the methods used to determine changes in microbial community structure. The authors used total colony forming units of bacteria, fungi and actinomycetes using a plate dilution method. However, this only allows culturable bacteria to be counted. Further, they were all incubated at 25C regardless of season, when the winter samples likely should have been incubated at colder temperatures. Also, how were these #s compared over time? The results state which dates are significantly different from each other – were they pairwise comparisons? If the authors plan to use these methods to describe microbial community structure I would like to see citations indicating they are appropriate, as well as further description of the limitations of these methods.

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Statistics: Because the same sites/plots were sampled repeatedly, a repeated measures ANOVA would be more appropriate than the 2-way ANOVA. Further, the description of the Pearson correlation analysis is not clear. I would like to see more of the results for this correlation described than just the  $r^2$  (Table 2). Also, throughout the results section I would like to see the actual statistics stated rather than just  $p < 0.05$ . Finally, is it possible to define a “peak” time for MBN or DON in the season when MBN did not vary seasonally? (Page 9 line 5).

Interpretation: Some of the interpretation of the results goes beyond what the results actually indicate. For example (Page 12 line 17) High microbial biomass does not mean there is high activity. Also see a reference to activity on page 14 line 16: this study did not contain any tests of microbial activity. Other conclusions require further elaboration. For example, The section on page 13 line 16 needs elaboration – Why would the decrease in MBC at thaw be related to the higher productivity and SOM in this site compared with others? Finally, there isn't direct support for many of the overall conclusions of the paper – this study can describe correlations, but not the types of conclusions described (e.g. soil microorganisms play a crucial role in accumulation of inorganic N pools)

#### Technical comments

The paper could use a thorough editing for English grammar: E.g. Community compositions should be community composition (Page 1 line 16) E.g. Change “Consistently increasing trends of MBC” to “Trends of consistently increasing MBC” E.g. Substrate transports should be substrate transport (Page 2 line 4)

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