

Interactive comment on “Controls on the distribution of the soil organic matter in mountain permafrost regions on the north Qinghai-Tibet Plateau” by C. Mu et al.

Anonymous Referee #3

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Unfortunately this manuscript falls short of delivering what is in the title. The authors present a very comprehensive and valuable dataset from deep boreholes. This data certainly warrants publication, but it also warrants more careful scientific analysis and context. The strength of the data is the deep boreholes, but the authors have failed to address how long term accumulation may affect SOM.

As the authors themselves point out, high-latitude regions are highly dynamic and sensitive to environmental change. Therefore the basic hypothesis that SOM at 20 m depth is controlled by the present day surface vegetation community seems rather implausible and needs further justification.

The boreholes used in this study were drilled into thick alluvial and colluvial deposits

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in areas of accumulation. It is highly likely that various geomorphic process have affected the erosion as well as deposition/sedimentation of sediment in the uphill areas of these locations, together with vegetation dynamics, surface hydrology, active layer dynamics etc. has exerted a strong control over SOM distribution and chemistry. These processes have been acting over many millennia and to state that present day surface vegetation controls the SOM distribution to depths of 20 m is a gross oversimplification.

The authors should consider addressing the issue of different landforms/depositional environments instead. Table 1 provides an idea of the type of geomorphic characterization the authors can pursue to analyze these issues. The authors also show interesting analyses of the influence of soil texture on SOM and briefly mention the possibility of changes in vegetation communities over time affecting the stable isotope ratio of carbon. Pertinent follow up questions are: How is soils texture linked to landforms? What is the age of different investigated strata? Is there any link between vegetation and soil texture and/or slope stability?

I would recommend that the authors pursue these results in more depth. While vegetation seems like a useful proxy, especially since it is easy to map and scale, the authors present no evidence to support that the vegetation has remained the same in these sites over the long times when these sediments accumulated.

The statistical analyses performed show that the basic SOM chemistry follows patterns described by many other authors. They do not, however, yield any new insights into the controls of SOM in the north Qinghai-Tibet Plateau. At a more detailed level, I am also concerned that some of the unexpectedly high C:N values may be due to very low N% values possibly close to detection limit? In some cases I also wonder about the rationale behind analyses? There is no deeper mechanistic explanation provided for the correlation of % water content to SOC. Also, I would strongly recommend that the data be made available together with the final publication of this data.

With the present analyses I would not recommend that this paper is published in

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The Cryosphere. The statistical analyses are limited and seem more spurious than hypothesis-driven. While an extensive dataset is available, I find that the authors provide little conclusions or results to significantly increase understanding of SOM accumulation or development in the north Qinghai-Tibet Plateau. I recommend that the authors take the opportunity to re-examine their extensive dataset and look critically at which landscape processes have led to the interesting SOM dynamics we see in these boreholes.

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