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Discussions

Interactive comment on "The organic carbon pool of permafrost regions on the Qinghai–Xizang (Tibetan) Plateau" *by* C. Mu et al.

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General comments

The paper represents a great attempt to summarize all the past research on soil carbon stores and distribution in different ecosystems (cover types) of the Qinghai-Xizang Plateau (QXP). The authors have reviewed many published works/research done by the Chinese colleagues over the past several decades. It is timely and would contribute to our understanding of the relationships between carbon stores and vegetation covers. This paper has the potential being a monumental work if the authors can tie the physiography, soils, vegetation, permafrost to define the ecosystems of the QXP and how these ecosystem attributes affect the OC store on the QXP.

But, there are some shortcomings must ne addressed before being consider for publication. I would recommend the manuscript be accepted after major revision.

I am confused if the objective of this paper is to estimate the organic carbon (OC) pools only of the permafrost regions of the Qinghai-Xizang Plateau(QXP) or the whole QXP. The authors calculated the total area of the permafrost regions of the QXP is 1.35x 106 km2. But the total area of QXP is 2.5 x 106 km2. Therefore, 46% of the QXP are seasonal frozen grounds which also exist beyond the boundary of the QXP.

The authors estimated the total carbon stock of the QXP based on vegetation cover types; alpine desert, alpine steppe, and alpine meadow. The forest area in SE QXP was shown in the map but no carbon data was presented. To my understanding, except the forest area, each of the land cover types includes both permafrost and seasonal frozen ground. In general, the carbon contents in permafrost-affected sites are higher than those without permafrost (because of soil moisture contents). The authors claimed that the database relied on 706 profile sites distributed among these three cover types. I question if all these sites have permafrost within measurable depths? Except some broad basins, there are large areas with steppe and desert vegetation communities have very shallow soils due to the shallow depths of weathering. These sites cannot be called permafrost sites or permafrost soils. The carbon contents in wet meadows are generally much higher than those well-drained Kobrecia meadow soils.

There has never before a comprehensive study or review of the carbon pools of the QXP. Because of this, it is crucial for the authors to scrutinize the dataset. In order to support the information in Table 1. The authors need to submit a supplement file that contains all the cited source of carbon data, site (pedon) by site as %C, bulk density, land cover type (meadow, steppe, desert, etc.), active layer depth, soil texture, sampling depth, elevation (asl) if these information is provided in the original source.

One of the coauthors, Dr. GD Cheng has studied the distribution of permafrost of the QXP for many years. Why not overlay the land cover type map over his permafrost

map to separate the carbon pools in the permafrost and non-permafrost of the same cover type? This approach would improve the accuracy of the estimate. If this paper is to represent the whole QXP. Then the forest regions in SE QXP should be included. Professor Zhang Wan Ju of the Chinese Forest Sciences Academy studied forest soils including the LinZhi District in the 1970 to 1980 period and published the book Forest soils of China by Ministry of Forestry. Another reference is the Qinghai Province Soils by Bao Xingqui of the Xining Alpine Botany Institute.

Next, an important question is about the uncertainty or variations caused by different lab procedures. The OC of the 11 deep sampling sites were determined by high temperature combustion with pre-treatment to remove inorganic carbon. But most of the carbon contents data in published Chinese literatures were determined by wet oxidation (for example, dichromate oxidation) and or loss on ignition. There could be 10-18% difference among the 3 procedures. I have not seen any study in China dealing with the calibration among these three analytical procedures. But at least the methods used in each cited source of data point should be noted in the supplement

My next concern is the extrapolation of the deep OC stores (3-25 m). Please note even in the 2-3 m OC store estimation in the North Circumpolar Carbon Database, there is wide disparity and very high uncertainty. When Strauss, Schirrmeister and their collaborators studied the OC store in the Yedoma deposit, they gave a definite geographic distribution base on geomorphology and lithological data (stratification) in that the Yedoma OC store does not extrapolated to the whole Arctic Coastal Plain. Thus, I'd urge the authors be discretionary as how far (in area extent) can this deep OC data can be extrapolated. Geomorphic and or geological information would help such extrapolation.

I'd strongly encourage the authors to go back to the cited dataset or data points and reorganize the data and consider the comments and synthesize all the previous work. Only after that the authors would be able to find the gaps in the previous work and pointing to the needs of future research.

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