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Interactive comment on "Permafrost soils and carbon cycling" *by* C. L. Ping et al.

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General comments. This paper gives a comprehensive review of literature on specific features of permafrost environments and soils and their impact on carbon cycling. In some cases, this impact is quite evident and can be easily illustrated. In particular, this concerns cryoturbation as a specific mechanism of "cryosequestration" of organic carbon in the deep mineral layers through burial of surface organic horizons. In other cases, the impact of cryogenic processes on the organic carbon storage and cycling is not so evident and requires further explanation. Thus, the relationships between the behavior of organic carbon and the formation of cryogenic soil structures have to be explained. It is probable that structures are important in the context of the thermal and water regimes in microloci, and, hence, in the context of potential activity of biota and microbiota. Anyway, it will be interesting to see definite links to carbon cycling in such

sections. This is not always easy. It might be reasonable to add a paragraph devoted to unsolved problems and challenges for future research.

After reading the paper, a reader might conclude that permafrost-affected soils with active cryogenic processes favor the organic matter storage in the soil profiles. On one hand, this is true. The role of permafrost soils in carbon sequestration is great, and these soils store more carbon (in % of the global soil carbon pool) than their own percent in the soil cover of the planet. On the other hand, it is not quite correct to directly link soil carbon storage with cryogenic processes. As truly mentioned by the authors, Gelisols of Antarctica are very poor in organic carbon (in fact, except for some ornithogenic soils). The high storage of organic carbon is typical of the Gelisols in the northern circumpolar region and is explained not only by the influence of permafrost and cryogenic phenomena. Other factors, including evolution of these soils in the Pleistocene, are no less important!

There are two major effects of permafrost and cryogenic processes on carbon cycling. First and foremost, this is impeded organic matter decomposition because of cold temperatures in the soil profile, short duration of the active season, and, often, reducing conditions (even in subarid and arid ultracontinental environments due to minimal internal drainage of the profiles). At the same time, heat, moisture, and nutrient supplies of the upper horizons (in the northern circumpolar region) are sufficient to ensure significant (relative to decomposition) phytomass production. The resulting carbon balance is positive. The second effect is mentioned in the paper, but, from my point of view, should be better articulated and requires more attention of researchers. Cryogenic processes (including thermokarst) are responsible for the high spatial heterogeneity of carbon distribution with the creation of diverse ecological niches of carbon accumulation in the soil profile (often, not only in the surface horizons) and in the landscape; they also create the zones of carbon depletion (e.g., during extrusion of the mineral mass from the deep horizons onto the surface). Thus, they make the ecosystem structure more diverse, and this is an important mechanism ensuring sustainable functioning of

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the entire system in the severe environments.

I agree with most of the already published interactive comments. In the list below, some additional comments are listed.

Specific comments. p. 711, line 23 - "Cryozem in the Russian system (Shishov et al., 2004)." No, this is an erroneous (though common) perception of Cryozem in the new Russian soil classification system (2004). In fact, this system has no equivalent to Cryosols/Gelisols/Permagelic soils. Cryozem is just one of the representatives of permafrost-affected soils in this system. There are many other soils (Gleysols, Palevye Soils, Cryometamorphic soils, etc.) that may meet the requirements of Cryosols. In the new Russian system, the presence of permafrost (1 or 2 m) and cryoturbation are taken into account at lower taxonomic levels. Permafrost-affected soils is a group of soils without definite taxonomic "weight".

In the previous "factor-based" system (its structure was most clearly explained by E.N. Ivanova in her abstract to the 6th ISSS Congress in Paris (1956)) the notion of permafrost-affected soils (in Russian, merzlotnye) was widely applied and the corresponding term (merzlotnye) was added to soil names. In English translations, it often appeared as "frozen" soils (taiga frozen soils, tundra frozen soils, etc.). However, the group of "frozen" (permafrost-affected) soils was not recognized at the highest level of the taxonomic hierarchy. "Zonal" soil names were higher. Thus, though the notions of permafrost-affected soils and its narrower "relatives" - Gelisols/Cryosols - exist in Russian literature, their taxonomic positions are not strictly determined; in general, they are lower. These are informal soil groups rather than strictly specified taxa (in the official soil classification systems).

p. 717, line 6. "When the water of ground ice in fine-grained sediments exceeds the pore space of the soil," - probably, "volume of ground ice". However, this is still not quite clear. Ground ice is formed in soil pore space (and makes it larger!); it cannot exceed the volume of soil pore space. Though, it can exceed (and, probably, almost

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always exceeds!) the volume of soil pore space after soil thawing... Better, rephrase.

p. 719,line 11 "promote formation of soil biotic crusts" - a longer explanation is required. Ice, surely, participates in soil structuring, including specific structures of biotic crusts. In some cases (in arid environments) it can serve as the source of water and thus promote biota. As for admixture of the SOM at the surface, this seems to be the product of biotic crusts rather than their "starter".

p. 718, line 11 "soil respiration during the shoulder winter season" - I cannot understand "shoulder" - colder? Or something else? The relationship between zero curtain, its "thickness" and duration, and soil respiration in winter is an interesting question.. Probably, we need more data.

p. 722, line 6: Change Kerzhntser to Kerzhentsev

p. 723, lines 29, 30 - p. 724, line 1. I'm afraid, I do not understand exactly about the proportion of cryoturbated carbon - its percent of the total soil carbon storage? What is the reason for a decrease in the portion of cryoturbated carbon in the southern part of the transect - less active cryoturbation, or higher carbon storage in the surface organic horizons?

p. 726, line 2-4. "organic matter that is typically more decomposable than that of non-permafrost mineral soils" - I guess, potentially decomposable is better...

p. 726, line 14. Yakutian alases as "a depression with deep peat" (Smith et al., 1995). Unfortunately, I'm not familiar with this paper. There are different versions about etymology of the word 'alas' in Yakutian. As I know, meadow, glade, or open space are more adequate than "deep peat". It is better to delete "deep peat", as it gives an erroneous view of alases. Detailed data on alases can be found (in Russian) in the monograph by R.V. Desyatkin (2007). Some illustrations from this book were included in the "Northern Circumpolar Soil Atlas." Alas is a "mature" stage in the evolution of thermokarst depressions (earlier stages have their own names in Yakutian). Thick

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peat is not the major feature. Alases are under grassy vegetation or under intermittent lakes; limnic deposits (sapropel type) are more common in them than peat accumulation proper.

p. 728. first paragraph. Overestimation of organic carbon determined by chemical oxidation methods (Walkley-Black?) compared to modern ignition methods (dry combustion?). The methods need to be specified. The statement itself is open to argument. Coefficients higher than 1 (1.28 to 1.41 and more!) are often used to recalculate C obtained by Walkley-Black method into dry combustion results [Amacher, M.S., R.E. Henderson, R.N. Brupbachir, and J.E. Sedberry. 1986. Dichromate-oxidizable and total organic carbon of representative soils of the major areas of Louisiana. Commun. Soil Sci. arld Plarzt Atzal. 17(10): 1019- 1032.; Gillman, G.P., D.F. Sinalir, and T.A. Beech. 1986. Recovery of organic carbon by the Walkley and Black procedure in highly weathered soils. Commun. Soil Sci. arzd Plant Anal. 17(8):885-892.]. Large datasets on the organic carbon determination by Tyurin's method (generally analogous to Walkley-Black) and by dry combustion methods were analyzed by B.M. Kogut, who concluded that, in most cases, dry combustion gives higher results. For weakly decomposed organic matter of litters the coefficient 1.2 was applied [unfortunately, I only know about publications in Russian. Some information can be obtained from a paper by Rojkov et al published by IIASA (WP-96-060_IIASA)]. Thus, overestimation of Corg by chemical methods (as in the case of the soils studied by Michaelson et al (2013)) cannot be considered a general rule and requires more detailed consideration. The chemical methods usually do not oxidize all the organic matter in the sample and, hence. underestimate rather than overestimate its amount. However, overestimation is possible, if the sample is rich in reduced forms of elements (bivalent iron compounds), such as in the gley horizons. In this case, their oxidation can be "misinterpreted" for the organic matter oxidation.

Interactive comment on SOIL Discuss., 1, 709, 2014.