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

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This manuscript provides a comprehensive and timely review of permafrost soils and carbon cycling. The authors summarize literature in a topic where recent decades has seen marked scientific progress and many new studies emerging. To review this field is challenging, but the authors are in a position to provide a valuable and authorative overview. I view this manuscript as a welcome and strong addition to the topic of permafrost soils and carbon cycling. I recommend it for publication following minor reviews.

I think this paper has the potential to become a future reference work on this topic, but I am concerned about a few points that need more attention. The larger issue that concerns me is that some sections of the review, and the cited literature, is overly focused on studies from the North American Arctic (specifically Alaska) while fewer

studies from Eurasia are discussed. I feel that there is an imbalance here that should be adjusted if the review is to give an unbiased overall view of work on permafrost soils and carbon cycling. See specific comments and suggestions below.

There are also several cases where I urge the authors to provide clearer formulations or definitions of statements that appear oversimplified or generalized. See specific comments below.

Specific comments:

Page 710 lines 25-26: note that this figure cites the ice-free, soil-covered land areas. Water and rockland are excluded from this number.

Page 711,lines 9-11. I would suggest the first batch of references are placed after the word thawing.

Page 711 lines 19-20. Could the authors clarify if they would recommend a distinction between these two terms?

Page 712 line 9. This seems like an oversimplification. There are many occurences of Gelisols in well-drained soils. The authors should clarify that poor drainage often favours near-surface permafrost but that well drained soils may also be Gelisols .

Page 712 line 21: "There are two general types of permafrost…". It would be helpful to define these two types directly in this introductory sentence

Page 713 line 13 change to "is subject to change under contemoporary…"

Page 713, lines 17-18: clarify that volumetric changes associated with the phase-change of water are important in this process of frost heaving

Page 714, lines 1-2. Three layers in a soil profile: Should not the fourth layer of true permafrost be included here? These often contain buried or palaeo-horisons as well. I realize you are citing another source here, but I think it is important to clarify that much of the deeper permafrost deposits have also undergone soil formation before being incorporated into permafrost.

Page 717 section 2.4 Thermokarst. This section is rather brief and does not provide the same level of detailed discussion as did the previous section. Would the authors care to identify a previous review which gives detailed discussion of process understanding when it comes to formation of these mentioned forms of thermokarst?

Page 717 lines 19-22. This section feels out of place here? I recommend it be moved, removed or rewritten.

Page 718 line 6. By which mechanisms are anaerobic conditions favored by low temperatures? High moisture content is understandable and the link to anaerobic conditions is well known there, but I was unaware that there is a connection between temperatue and oxygen demand. This statement should be clarified and supported with a reference.

Page 718 line 15: This is again unclear to me. Should not saturated conditions promote anaerobic conditions that limit oxidation?

Page 718, line 29 “upon in contact”. Rephrase

Page 718 line 26. Should not sub-Antarctic soils also be included here? Or perhaps just simplify to cold-region soils?

Page 719, line 9-10.” SOM at the surface can be mixed with the surface few centimeters of mineral soil a. . .”. I do not follow the meaning of this sentence?

Page 720, line 18. “. . .in permafrost soils other processes, such as cryoturbation, deformation by massive ice growth, and intermittent burial and syngenetic permafrost growth, result in the storage of large quantities of this surface and near-surface produced SOM at depth.” I don’t agree with this statement. The mentioned processes result in a translocation of near-surface SOM to depth in the soil profile. The storage is a function of combined translocation and retarded decomposition/combustion in the subsoil.

Page 724 lines 12-13. “Thus, three different soil types develop across this microtoposequence:. . .”. I suggest you rephrase to: "three different main soil types have been observed to develop across polygons. . .". One cannot reasonably expect all the worlds polygons to exhibit this general pattern.

Page 724 lines 16-20. This describes one possible development but it is phrased as if it were the only possibility. There are many other potential developmental pathways for such polygon structures. In many circumstances, high-centre polygons can form due to peat accumulation in the polygon centre, without the ice-wedges degrading.

Page 725, lines 8-9. Perhaps this oversimplified statement regarding thaw-lakes could be refined? Many parts of the Arctic and boreal regions do not contain thaw lakes. The occurrence of thaw lakes is strongly linked to the depositional origin of unconsolidated sediments and the occurrence of massive ground ice and previous climate cycles.

Page 725 lines 9-10. This general statement regarding ice-wedges and thaw lakes applies only to some type of thaw lakes but is not applicable in all cases. Thaw lakes can also form in landscapes where there are no ice-wedges. Commonly occuring thaw lakes associated with degradation of palsas and peat plateaus in sub-arctic peatlands is one example of this.

Page 725 lines 10-11. The statement about a cycle of 3000 are local/regional estimates for Alaskan thaw lakes that does not apply to other regions and this must be clarified. This cyclicity is likely to be very different depending on lithologies, permafrost initial conditions, climate and land use (applies to the Siberian landscapes where human land use is important for alas landscapes).

Page 725 lines 15-16. “accumulate, as high as 90 $kgCm^{-2}$ from 0–3m deep..”. Rephrase this sentence.

Page 725 lines 27-28. “and froze once air temperatures dropped below zero.. . ..”. This statement is an oversimplification which should be taken out or elaborated. Presumably they froze when the ground temp dropped below zero, not the air temperature. Do the authors mean that permafrost aggraded as mean annual air temperatures dropped below zero? Much of the yedoma has been shown to accumulate under syngenetic conditions when permafrost was already present so presumably it was deposited into an environment that already experienced sub-zero temperatures for most of the year.

Page 726 lines 12-13. This statement of alas meaning peat-filled depression is not correct. Most Yakutian alases are characterized by grass growing in mineral soils rather than deep peat. While there are many different developmental stages and some alases form deep peat deposits, it is not correct to state that they are all filled with deep peat. The word alas is also not exclusive to peat-filled thermokarst depressions but refers to characteristic thermokarst depressions with steep slopes and flat floors. See e.g. work by Czudek et al. (1970), Veremeeva and Gubin (2009) or Morgenstern et al. (2013).

Page 726, line 25-26: If this aims to be an exhaustive list of important studies regarding the influence of permafrost on soil OC storage I am concerned that it is biased towards Alaskan studies without giving due credit to other work. I would argue that the work of Tarnocai and Lacelle (1996) should be viewed as pioneering when it comes to soil C stocks in North America (and indeed the whole Arctic). Hugelius et al. (2010) also provide a SOC inventory from a poorly studied part of Canada.

It is also surprising that the authors limit themselves to naming only one study from Eurasia. Eurasian studies on permafrost and OC stock interactions by: Becker et al., (1999), Kuhry et al. (2002), Mahzitova et al. (2003), Rodinov et al. (2007), Gundelwein et al. (2007), Hugelius and Kuhry (2009), Klaminder et al., 2009, Hugelius et al. (2011), Schirrmeister et al., 2011, Hugelius (2012), Zubrzycki et al. (2013) could be mentioned in this context. I am aware that I mention many of my own papers and I am not suggesting that you necessarily have to cite these. I just wish to point out some parts of a significant body of literature that may have been overlooked which the authors could familiarize themselves with to provide a more balanced circum-Arctic review.

Page 727 line 11. The proper reference here would be Hugelius et al 2013 a and b.

Page 727 line 14. Note that the final, accepted, version of this paper has slightly different SOC stock numbers following adjustments of the methodology.

Page 727 lines 19-20. Note that these pedon numbers only apply to deeper soils. For 0-1 m depth the database is ca 1700 pedons adapted from Tarnocai et al., 2009. See Hugelius et al., 2014 for details.

Page 729 lines 2-5. Here I would wish to point to the study by Hugelius et al., 2011, who used the highest resolution upscaling to date of any permafrost SOC study (2m resolution) and discusses local scale heterogeneity versus regional scale upscaling. Hugelius (2012) provide in depth discussion regarding the topic of upscaling accuracy and spatial resolution in upscaling.

Page 731. Lines 27-28. Please note the interesting study by Kaiser et al. (2007) which examines mineralization in cryoturbated soil horizons.

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