The Cryosphere Discuss., 8, C1890–C1897, 2014
www.the-cryosphere-discuss.net/8/C1890/2014/

The Cryosphere Discussions

TCD

8, C1890–C1897, 2014

Interactive Comment

Interactive comment on "Low soil organic carbon storage in a subarctic alpine permafrost environment" *by* M. Fuchs et al.

M. Fuchs et al.

matthias.fuchs@awi.de

Received and published: 24 September 2014

Reply on comments from Anonymous Referee #2

Thank you for your comments on our paper. We hope to address all your comments and suggestions in this response. You bring up a lot of points including several suggestions that are useful to improve the manuscript. We wish to initially give our views on the general comments provided by the referee. More specific responses to individual issues are given below.

General comments

The referee objects to the fact that the word soil appears in the title when the paper contains no map of soil or classified pedons. We understand that a reader may expect to find more detail on pedology in a paper with this title and we will strive to accommodate this. We propose that a revised version of the manuscript includes a supplement summary of the pedon classifications related to land cover classes and SOC storage.

The referee suggests that we have “stuffed” a number of non-soil related aspects into the paper which has led us to lose sight of the true objective. On the contrary, the initial objective was always to provide an integrated landscape study of this valley, which included both land cover, geomorphology and permafrost characteristics. We consider land cover and geomorphological characteristics to be relevant indicators of soil types and many studies have shown that land cover can successfully be used to estimate soil organic carbon storage. If our aim had been to carry out a pedological study, we would have focused on detailed studies of fewer profiles and applied a pedological analysis. With our initial aim of a complete landscape study in mind we designed a field sampling campaign that aimed to cover as many land cover types at different elevations as possible.

The referee also complains that the paper discusses permafrost in detail despite that fact that we did not encounter permafrost during soil sampling. Our study area has permafrost and is located within the northern circumpolar permafrost region (Brown et al., 1997). In recent decades, several studies have emphasized that soils of this region are very rich in SOC and many studies also emphasise a potential increase in greenhouse gas fluxes from permafrost SOC under a warming climate. We include permafrost mapping to very clearly demonstrate that for this particular region there is no potential for a strong positive permafrost-carbon feedback with climate warming. Our paper includes discussion and comparison to other studies of alpine SOC as well as a direct comparison showing the large differences between our estimates and what was estimated using the Northern Circumpolar Soil Carbon Database (Tarnocai et al., 2009) for this same study area. Within this context we maintain that a careful analysis of permafrost conditions is relevant and interesting.

Specific points following the page numbers used by Referee 2

TCD

8, C1890–C1897, 2014

Interactive Comment

Page 3493. The referee states that the title does not represent the content of the paper. This statement is based on the three points which we address separately:

“First, the soil organic carbon is presented on a land cover basis (mainly vegetation), not on a soil map.” We have upscaled our SOC estimates based on a land cover classification. Vegetation or land cover has been successfully used in many previous studies of SOC in high-latitude and high-altitude ecosystems (see e.g. Kuhry et al., 2002; Ping et al., 2008; Hugelius and Kuhry., 2009; Horwath Burnham and Sletten, 2010, Hugelius et al., 2010; 2011). We maintain that land cover based upscaling is a suitable choice for the present study. We acknowledge that a soil map would have been a nice complement to this study; however that is outside the scope of this study. Hugelius (2012) provides a more detailed discussion on the relative merits of land cover or soil maps for this type of thematic upscaling. With this said changing the title to “Below-ground carbon...” instead of using the word soil is a suggestion we will consider.

“Secondly, no permafrost was encountered in any of the soils that were sampled.” We agree that no permafrost soil was sampled for SOC. But we maintain that Tarfala Valley is nevertheless a permafrost environment. As stated in our study area description, the active layer is documented to ca. 1.5 m in the upper part of the valley while the bottom of the valley is likely permafrost free. It is also interesting to emphasize that we are studying a permafrost environment that actually has very limited extent of permafrost in soils. This dichotomy is especially interesting in the context of the permafrost-carbon climate feedback (see our response to the general comments above).

“Thirdly, there is no big surprise about the low soil organic carbon since none of the soils are affected by cryogenic processes.” We agree that the soils are relatively unaffected by cryogenic processes, especially compared to e.g. High Arctic soils of Pleistocene-aged landscapes. Regardless, we argue that the referees lack of surprise at our results is not a valid argument for removing the discussion of permafrost from the manuscript.

Page 3496, lines 23-26 and Page 3497, line 10. For clarity we start by stating that the short description of soils given in the study area is based on a combination of the 56 sampled profiles as well as an independent field survey by the authors. This will be clarified in a revised version of the manuscript. In the case of the soils of the upper valley, the combination of strong patterned ground formation with permafrost in the top two meters leads us to classify the soils here as Turbic Cryosols. The observation of active layer depth is from the PACE borehole as cited in the Study Area description (p3497, lines 7-10). Note that we describe "no signs of SOC burial by cryoturbation or solifluction" in our study. That does not mean there is no cryoturbation in the soils. There is extensive cryoturbation and frost heave of stones and mineral gravel/loam in the upper reaches of the valley. But because there is no measureable SOC in this material it does not affect the SOC storage which is what we were studying.

Page 3498, lines 24-26. We fully acknowledge that it would be preferable to base all analyses on direct measurements with an elemental analyser. Because of financial and logistical constraints we were able to carry out elemental analyses on 96 samples, while the remaining samples 199 samples were analysed using LOI. Such constraints are unfortunate, but a part of reality for most researchers. The paper by Pribyl (2010) which the referee mentions is entitled "A critical review of the conventional SOC to SOM conversion factor". As the name states it is a critical review of the commonly used conversion factor to estimate SOC from loss on ignition (1.724). We agree with this conclusion by Pribyl (2010) and we did not use the LOI values with a fixed conversion factor but instead developed a local transfer model. We had the capacity for 96 samples with the CarloErba NC2500 elemental analyser and got the %C values for these samples. With these 96 values we applied a statistical regression analysis with the LOI values of these 96 samples and applied the resulting third order polynomial function on the LOI results of the remaining 199 samples.

Page 3503, lines 12-13. We are aware that these results are likely not surprising to most readers. In the absence of pedogenic processes that accumulate sub-surface

SOC it is indeed common to have the highest SOC content (density) in the upper soil horizons. There are however many cases where the SOC density is actually higher in the mineral sub-soil (see e.g. Harden et al. 2012) than in surface O-horizons. In this context this statistical analysis of our data seems justified.

Page 3504, lines 5-13. We agree with the referees comment that the 56 different profiles span across a range of different soil development.

Page 3506, lines 15-22. We fully agree that a revised version of this paper would benefit from a deeper discussion of the pedogenic processes that lead to the formation of SOC stocks in cold-region soils. We will strive to accomodate this suggestion in a revised version of the manuscript.

Table 1. We believe that the referee has misinterpreted this table. It does not show individual pedons but aggregated mean values for different classes. The sub-headline "Profile Site" is under the headline "Mean Profile Depth", therefore the values in this column represent the mean profile depth for the sites for the different LCC. So it is not the case that the profiles were only sampled to 40 or 56 cm. These columns below "Mean Profile Depth" indicate only the mean values and not the maximum values of sampled depth. There were profiles to a depth of 80 cm, but it is true, most profiles were rather shallow, but this was due to shallow soils. In most cases, the underlying bedrock was the reason for shallow profiles. In our calculations, we considered bedrock as zero carbon. So we calculated to a depth of 1 meter even though the profiles were shallower. So the values for 1 meter do not indicate that the soil was 1 meter deep. On the other hand, which reference depth, beside 30 cm, should have been chosen instead? There was no uniform soil and/or sampling depth. There would have always been some bedrock included in the calculations. Therefore we thought it is reasonable to take the common reference depths of 30 and 100 cm The SOCC for 0-100 cm indicates how much carbon is present in the first meter, but it does not say that this carbon is distributed uniformly in this first meter. For a comparison, the mean SOCC values of 0-30 cm and 0-100 cm are given in Table 1.

The last column “Sites in Permafrost” indicate how many of the profile sites in a particular land cover class are situated in the different mapped zones of continuous, discontinuous, sporadic, or isolated patches / no permafrost.

Figure 1: The purpose of Figure 1 is not only to show the areas where we collected the soil samples but also to show and introduce the entire study area. Therefore, we think it is important not only to show maps A and B. The size of the map already covers an entire page but we will try to make it a little bit wider. If the map was presented in the A4 format rather than the screen format of TCD it would likely be larger and easier to read.

Figure 3: The variation in bulk density is difficult to explain. But one reason might be the sampling technique. We sampled by hammering down a steel pipe into the soil. Normally the sample with known volume stayed in the pipe. In most cases, this worked fine, however in a few wet, sandy spots, it might have happened that the sample volume got disturbed which would explain the high bulk densities >2 g cm-3. On the other side, the carbon values were very low for these sandy samples. The elemental analyses showed carbon values below 1%. Nevertheless, we wanted to show the complete data set analysed with the CarloErba NC2500 elemental analyser, even though some bulk density values are questionable. In the supplementary we show the properties of two individual pedons where we have the radiocarbon dates as well.

Figure 4: Thank you for this comment. Indeed, the figure is maybe not so relevant for the individual sampled pedons, but we maintain that it is highly relevant to demonstrate how unlikely it is to find permafrost SOC in this permafrost environment. The figure is also useful in the context of the future trajectory of the C cycling in the Tarfala Valley as discussed in chapter 5.2.

References:

Brown, J., Ferrians Jr., O. J., Heginbottom, J. A., and Melnikov, E. S.(1997) Circum-Arctic map of permafrost and ground-ice conditions, 1 : 10000000, Map CP-45, United

TCD
8, C1890–C1897, 2014

States Geological Survey, International Permafrost Association

Harden, J. W., et al. (2012), Field information links permafrost carbon to physical vulnerabilities of thawing, Geophys. Res. Lett., 39, L15704, doi:10.1029/2012GL051958.

Horwath Burnham, J., and Sletten R.S. (2010), Spatial distribution of soil organic carbon in northwest Greenland and underestimates of high Arctic carbon stores, Global Biogeochem. Cycles, 24, GB3012, doi:10.1029/2009GB003660.

Hugelius, G., and P. Kuhry (2009), Landscape partitioning and environmental gradient analyses of soil organic carbon in a permafrost environment, Global Biogeochemical Cycles, 23, GB3006, doi:10.1029/2008GB003419.

Hugelius, G., Kuhry, P., Tarnocai, C. and Virtanen, T. (2010) Soil Organic Carbon Pools in a Periglacial Landscape; a Case Study from the Central Canadian Arctic. Permafrost and Periglacial Processes, 21, 16-29. doi: 10,002/ppp.677

Hugelius, G., T. Virtanen, D. Kaverin, A. Pastukhov, F. Rivkin, S. Marchenko, V. Romanovsky, and P. Kuhry (2011), HighâĂŘresolution mapping of ecosystem carbon storage and potential effects of permafrost thaw in periglacial terrain, European Russian Arctic, J. Geophys. Res., 116, G03024, doi:10.1029/2010JG001606.

Hugelius, G., 2012. Spatial upscaling using thematic maps: an analysis of uncertainties in permafrost soil carbon estimates, Global Biogeochem. Cy., 26, GB2026, doi:10.1029/2011GB004154.

Kuhry, P., G.G. Mazhitova, P.-A. Forest, S.V. Deneva, T. Virtanen and S. Kultti (2002), Upscaling soil organic carbon estimates for the Usa Basin (Northeast European Russia) using GIS-based landcover and soil classification schemes, Danish Journal of Geography, 102, 11-25.

Ping, C. L., Michaelson, G. J., , Jorgenson, M. T., Kimble, J. M., Epstein, H., Romanovsky, V. E. and Walker, D. A. (2008) High stocks of soil organic carbon in the North American Arctic region, Nature Geoscience, 1, 615-619.

Tarnocai, C., Canadell, J., Mazhitova, G., Schuur, E.A.G., Kuhry, P. and Zimov, S. (2009) Soil organic carbon stocks in the northern circumpolar permafrost region. Global Biogeochemical Cycles, 23, GB2023, doi:10.1029/2008GB003327

Interactive comment on The Cryosphere Discuss., 8, 3493, 2014.