Solid Earth Discuss., 7, C1434–C1442, 2015
www.solid-earth-discuss.net/7/C1434/2015/

Solid Earth Discussions
Open Access
EGU

Interactive comment on "Stability and biodegradability of humic substances from Arctic soils of Western Siberia: insights from ¹³C-NMR spectroscopy and elemental analysis" *by* E. Ejarque and E. Abakumov

E. Ejarque and E. Abakumov

elisabet.ejarque@wcl.ac.at

Received and published: 29 November 2015

We would like to thank the three reviewers for their valuable comments, and appreciate the interest that they have shown in our topic. However, we understand the concerns they raise and we agree that our text will greatly benefit from their suggestions, which we will implement in the next revised version of the manuscript. First, we address some aspects that they all commented, and after that, we respond to the specific suggestions

of every reviewer.

The three of them pointed at one main common issue, which refers to the structure of the paper. We agree that a reorganisation of the information will improve the readability and clarity of the paper; therefore we will include their suggestions in our updated manuscript. Most remarkably, we will work on separate sections for the results and the discussion. We also agree with the reviewer #3 that our manuscript in the current form does not underline sufficiently the novelty of our contribution, and we think that by developing a separate discussion section, and by rearranging the introduction, we'll be able to make this point more clear.

As a technical aspect, here we would also like to clarify that the soils were 2-mm sieved, and not 1-mm sieved as it was stated in the manuscript.

We hereafter respond to the specific comments of each of the reviewers:

Comments of referee #1:

Page 3022 Abstract Introduction Line 23-25: I sugest to complete this part with the information provided by these papers: "Sedimentological characteristics of ice-wedge polygon terrain in Adventdalen (Svalbard) – environmental and climatic implications for the late Holocene (http://www.solid-earth.net/5/901/2014/se-5-901-2014.pdf)", Soil processes in cold-climate environments (http://www.solid-earth.net/5/1205/2014/se-5-1205-2014.pdf) Future avenues for permafrost science from the perspective of early career researchers (http://www.the-cryosphere.net/9/1715/2015/tc-9-1715-2015.pdf)
Page 3024 Line 4-5: Please show theses studies. I suggest this here: Oliva, M. Ruiz-Fernández, J. (2015). Coupling patterns between paraglacial and permafrost degradation responses in Antarctica. Earth Surface Processes and Landforms, 40 (9):1227-1238.

We appreciate these suggestions and we will take these references into account.

SED
7, C1434–C1442, 2015

Line 19-3(Page 3025): This part should be placed in the description of the study site, not in the introduction

We agree that this paragraph is more suitable for the section devoted to the study site, therefore it will be accordingly reallocated.

Page 3025: Line 4: Delete “general” Line 10” Delete “More specifically”
Methods Page 3026: Line 9-11: This is a repetition, please delete it.

We thank these suggestions which will improve the quality of the expression of our text.

Line 21: Provide the citation of the guidelines that you used for soil classification. It was WRB?. Do it here and elsewhere in the paper.

Indeed, we followed the WRB guidelines for the soil classification. This will be made more explicit in our new text.

Page 3029: Line 23-24: Have you observed if data respected the homogeneity of the variances and the normality before use the parametric Pearson correlation coefficient? Parametric tests only can be used if data follow respect these requirements. At which p level, the correlation(s) were considered significant?

We agree with the referee with the need to test for normality and homoscedasticity before applying a parametric test. Indeed, all variables presented normality according to a Shapiro test at a p level of 0.01 (using shapiro.test from base R). Also, the presented models passed both the Breusch-Pagan and White tests of homogeneity of variances at a p level of 0.01 (using bptest from lmtest package for R). This will be explicitly stated in the updated manuscript.

SED
7, C1434–C1442, 2015

Line 25: Which multivariate model you used?

The sentence should read "multivariate linear model". This will be changed in the new manuscript.

Page 3031 Results Line 23: What this means?

By "molecular complexity" we refer to the content of aromatic compounds. We will make this expression clearer in the new text.

Page 3032 Line 1-2: Show the coefficient of correlation and the p<value. Delete "df = 8 F = 8.035". Is this the r2 or the adjusted r2? Page 3034: 13-15: Show the coefficient of correlation and the p<value Delete "df = 8, F = 10.86". Is this the r2 or the adjusted r2?

r^2 *values refer to the adjusted* r^2 *according to Wherry's formula. The expression of these results will be rewritten according to the reviewer's suggestions.*

Page 3036 Conclusions Line 10-18: Please synthesise your results and do not discuss them here. Rearrange this part of the conclusions

This will be rearranged when developing the separate sections for the results and the discussion. We will take this suggestion into account and make sure that all discussion material will be moved into the Discussion section.

Figures
Figure 2: Increase the size of the font of the picture letters Figure 3: Please do this figure in colour. I presume that the values in parenthesis represent the soil depths. Mention this in the figure caption. The figure caption should be changed. Change "diagram" by "scaterplot"

SED
7, C1434–C1442, 2015

Figure 4 and 5: If possible do these figures in colour. Like this is very difficult for the reader to understand it.
Figure 6: Rearrange this figure and use colors. Some information is overlapped. Like this some important information can be lost or misunderstood.

We appreciate these comments to improve the quality of our figures. Therefore these suggestions will be implemented in our new version.

Comments of referee #2

A major problem appears in this section with the concept of permafrost and active layer (and related terminology, e.g. permafrost table/border). These ideas should be clarified here and along the text (see French, 2007).
p. 5 line 8 – change to "depth". And it is It is the active layer of the permafrost. Maybe it could be permafrost table, but I am not sure about that. You need to continuous monitoring (longer than 2 years at least) to state that the permafrist table is placed at 90 cm.
p. 5, line o "border of the permafrost", what does it mean? I will not detail all the mistakes related to permafrost concept along each of these sites. This MUST be changed.

Although there are no permanent CALM monitoring sites in our study region, the presence of permafrost table on the investigated plots was confirmed during the two consecutive expeditions performed on 2012 and 2013.The term "permafrost border" was a mis-expression of "permafrost table" and this will be changed in the updated text.

p. 2, line 3 Polar? I don't think Antarctic soils could be included in this sentence. This is true for Arctic soils, but not for polar soils (including Antarctica), very poor in OC. For further informarion about this check: Bockheim (2015). The soils in Antarctica. Springer.
p. 2, line 15. In line 15 you mention Arctic. Please homogenize along the text when it's

SED
7, C1434–C1442, 2015

clearly “polar” and when it is “Arctic”.
p. 2, line 18 “Arctic” here and throughout the text

We agree that there is some confusion throughout the text when referring to Polar or to Arctic soils. For clarity, we will narrow our discussions only to Arctic soils.

p. 2, line 5 “Quaternary” I am not sure to understand the meaning of this sentence. The Quaternary period has recorded tens of glacial-interglacial oscillations. But you mention here that this accumulation is only due to “cold”. Please rephrase. I guess you should mention here the role of permafrost preserving old organic-rich sediments from other glacial-interglacial phases.

Indeed, the idea we wanted to reflect here is the role that the permafrost had in preserving the organic content of sediments. This will be rephrased to make it clearer.

p. 3, line 16-27 This is description of the Study Area. IT should be reallocated in this section.

We agree with the reviewer and this section will be reallocated accordingly.

p. 4, line 20 missing “sediments deposited during the...”
p. 4, line 21-22 minimal/maximal change by minumum/maximum I’m not a native speaker but I don’t think “Where” is the appropiate conjuction here
p. 4, line 22 change to “air temperatures remain positive”
p. 4, lines 25-26 mentioned before, delete

We appreciate these suggestions to improve the clarity of our expression; therefore these changes will be included in our new text.

p. 4, lines 27- only, several international reports suggest a significant higher increase.

These values will be updated with information from the 4th IPCC report (2007) which predicts a temperature increase between 2 and 7°C in the Arctic by the year 2100.

p. 4, lines 29-21 to page 5 line 2 – unnecessary information for a scientific paper. Besides, the last sentence is methodology.

We agree with the reviewer. This information will be deleted as it is already appearing in the Acknowledgements section.

p. 5, line 12 landscape are overmoisted? Please do not mix landscapes and soils!

This sentence will be rephrased to avoid confusion.

Comments of referee #3

p 3021, title. Replace "humic substances" with "humic acids", as the fulvic and humin fraction were not considered.

We agree with the reviewer that humic acids can lead to confusion as we did not analyse the fulvic and the humin fractions. However, as we do present results from both the bulk SOM and the humic substances, we suggest to rather use the generic term "organic matter", which may be more inclusive. Therefore the title would be "Stability and biodegradability of organic matter from Arctic soils of Western Siberia: Insights from ^{13}C-NMR spectroscopy and elemental analysis".

p. 3023, lines 1-10. I would suggest to consider that warmer temperatures at high latitudes are already resulting in unprecedented permafrost thaw, leaving large organic C pools exposed to fires (and especially smouldering) for the first time in millennia (Geochim. Cosmochim.Acta, 137, 2014, 134–146; G. Rein, Smouldering fires and natural fuels, Chapter 2, 2013).

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

We appreciate this suggestion and we will consider the inclusion of this idea in our new version of the manuscript.

p. 3024, line 17. Replace “humification” with “mineralization”
p. 3028, line 5. Replace “690 nm” with “665 nm”
p. 3036, line 12. I would avoid using the word “profile”, as, in most of the cases, the Authors studied only the first 5-to-10 cm of depth.

These suggestions will be replaced accordingly.

p. 3031, paragraph 3.2. Did you calculate the extraction yield of HA?

Yes, we calculated this information and it will be added to the results of the new version of the manuscript.

p. 3031, lines 20-30. Is there a correlation between H/C and E4/E6? Both are possible indices of humification.

We agree with the reviewer that, according to the general interpretation of H/C and E4/E6, both can be indicators of humification degree. However, in our results they did not show a correlative relationship (r2=0.137 and p>0.05). Moreover, beyond this lack of statistical significance, the scatterplot of both variables rather shows a contradictory trend, in which higher H/C values (hence, higher alifatic character) coincide with a decrease in the E4/E6 index (higher aromatic character). Therefore, we decided not to focus on this relationship for two reasons: 1) lack of a clear and statistically significant relationship, and 2) E4/E6 relates to bulk SOM while H/C relates to the humic acid fraction only, therefore a direct comparison may be misleading.

p.3032, lines 7-9. Please consider that the C/N ratio is affected also by the origin

SED
7, C1434–C1442, 2015

(e.g.,botanical composition) of the soil organic matter.

We agree with the reviewer that the C/N ratio is a characteristic which also depends on the precursor material of humic acids. In our reasoning we assumed that the vegetation cover has been constant during the last decades and therefore, we attributed the C/N changes to humification transformations. However, we agree that this is a point that should be further discussed and this will be included in the updated text.

p. 3036, lines 18-24. I would suggest to compare your findings with other studies, even though they were conducted in different environments. For example, in a previous study carried out in a peatland located in Northwest Territories, Canada (Org. Geochem., 42, 2011, 399-408), the low degree of humification of the organic material was also reported, as well as the influence of permafrost on the low content of HA in this peat.

We appreciate this suggestion and we will use it to further develop our discussion.

Table 1. I would suggest to reduce the number of decimal places.
Table 2. I would suggest using only one decimal place. What about S?

These comments will be taken into account in the version of the tables. Unfortunately, S was not analysed and therefore, cannot be included.

Figure 3. I would suggest to try plotting together also the H/C and the O/C ratios.

The scatterplot H/C vs O/C was not included in the manuscript because of it being little informative, as it was already stated in P3032 lines 19-20.

Interactive comment on Solid Earth Discuss., 7, 3021, 2015.