

Interactive comment on “C / N ratio, stable isotope ($\delta^{13}\text{C}$, $\delta^{15}\text{N}$), and *n*-alkane patterns of bryophytes along hydrological gradients of low-centred polygon of the Siberian Arctic” by Romy Zibulski et al.

Anonymous Referee #1

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Title. why not use brown mosses or equivalent instead of bryophytes?

Number of samples: In line 20 you speak about 400 samples from 10 species, and in line 22 ‘six of these taxa’, it not clear how many samples (from those 400) were investigated for n-alkanes.

Table A2: why is there so much variation in the number of samples analysed (n)? n for ^{15}N is lacking

Line 35 is ‘... as a function of microbial symbiosis’ also an assumption? If not from which data is this concluded?

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The Title and Abstract suggest that you investigate the effects of a hydrological gradient on ^{13}C , ^{15}N , C/N and n-alkane distribution, but from the paper it remains unclear to me if this really has been done, i.e., it seems that certain species are labelled meso-hygrophilic/xero-mesophylic, and compared as such, but the same species may occur in different habitats (water levels) and compared according to habitat (as is suggested by the title). I would recommend to make this explicitly clear, already in the Abstract but also throughout the ms. So, make clear how you define meso-hygrophilic mosses, are these well-known dry habitat mosses OR are you looking at the difference between individuals in a certain habitat?

Lines 37-39. Be more specific. 'Isotopic and biochemical signals' are mentioned, I recommend using C/N and n-alkanes instead of biochemical. Also 'certain moss taxa' could you mention them?

Lines 37-39. I would be more careful in extrapolating the results to be useful in paleoenvironmental studies, this is not the subject of the ms, and the results do not enable such discussion because peat or SOM is mostly decomposed and a mixture of several species, it is therefore not valid to state that the effect of hydrological conditions on the here analysed proxies is dominating that of species, or decomposition. Also line 52-55, I think that the ms should not focus on paleoenvironmental interpretation, as no such data are provided and it is thus only distracting from the subject; I recommend not to focus on this, but mention in the Conclusion that this may be a problem for the interpretation of these proxies in paleostudies and the need for future research on this.

Line 37 '. . . .) and intermediate (C25) chain length, respectively.'

Line 49. Do you mean mosses in general with 'their'? the abundance of mosses in peat and permafrost is probably more related to ecology (cold wet conditions), instead of to its intrinsic low decomposition rate.

Lines 51-52. I don't think that little is known about stable isotopes and 'biochemical characteristics' of mosses, the effect of this study (habitat) is very interesting and in-

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deed not much of it is known. In the next sentence (line 56-58) you say that these are the most commonly used parameters, isn't that contradicting?

I recommend to use 'n-alkane and C/N ratio' instead of 'biochemical characteristics' throughout the paper. Because the term biochemical characteristics is much broader.

Line 60 microbial material instead of activity?

Line 75. Explicitly mention this point earlier (already in the Abstract), because mosses and peatlands are often associated with Sphagnum. Indeed this study is more novel especially of the focus on non-sphagnum mosses, but this is not clear from the beginning and may also solve the other problem mentioned above.

Line 104. This is a very good idea, I would mention this already in the Abstract, see earlier comments. But then I see the Methodology in lines 124-131 and the question arises if this really has been studied because mean values are used to determine its hydrological habitat, thereby losing important information on the effect of water level. Relevant information is missing on how many plants were studied for each of these means and how is the variation within each group?? It is again not clear to me how the two groups (xero-meso and meso-hygro) are defined, on the basis of species or hydrological habitat?

Line 135 abbreviations have already been used

Line 178 perhaps individual instead of single?

Line 188 Fig. 3b?

Line 190. If the alkanes are meant to use as a proxy for species or water level, then the data of absolute abundance must be presented as well, as this is highly variable like you indicate here. In a mixture of species like in peat and soil, a certain distribution can only be related to the species (or water level) if differences in absolute abundance is taken into account.

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Line 195 I don't understand this. According to the figure there is not really a species-preferred position all a-d have a -3 and 10 for example. If you look at the highly variable water levels within each species in Fig. 4, then I would not say that the species can be separated into those groups of xero and mesophilic. It must be more explicitly mentioned how these groups are defined, based on what.

I would delete the enter between lines 196 and 197

Lines 225-226. Now I understand it better. This is a pity. It would be very interesting do such a study, correlate the water level to the plant chemistry, at the plant detail, not the plot. It should be made very clear in the Method Section, now I had the impression that it was done on the individual plant level.

Line 228-229 this must be mentioned must earlier.

Line 297 but there is a large variability. See line 195

Line 329, I do not understand the word choice 'individuals' in the context of the sampling design, see earlier comments

Line 333 no fossil material was studied, so this conclusion cannot be made

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