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The Cryosphere Discussions

Interactive comment on "Distinguishing between old and modern permafrost sources 1 with compound-specific δ^2H analysis" *by* Jorien Vonk et al.

Anonymous Referee #2

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I have read the manuscript "Distinguishing between old and modern permafrost sources with compound-specific d2H analysis" by Vonk et al. The study assesses the use of n-alkanes and n-alkanoic acid distribution patterns and δ2H values to inform on the sources of sedimentary organic matter derived from different types of permafrost melt. They apply this method in combination with bulk organic geochemistry, and compare this approach with a source identification method based on δ13C and radiocarbon from bulk sediments. The authors identify strengths and weaknesses of each approach, and conclude that each approach has its own merits and drawbacks. Overall I found the manuscript to be adequately written, the experimental design and execution to be sound, and the analysis and interpretation to be supported by the data.

I have a few comments that I've outlined below where I think that some clarification would help, and a few stylistic suggestions, but otherwise I have no major critiques. The topic is relevant and within the scope of the journal and I recommend publication following consideration of mu comments.

General comments:

I think the abstract is too long, and that it goes into too much specific detail. I think that it could and should be made more succinct.

The phrase "molecular-bulk upscaling challenge" is used without enough introduction/definition. I understand what you mean by it, but I think that it would be better to explain what this is exactly in a bit more detail.

A general comment about the structure of the discussion is the separation of the 13C-14C data from the bulk geochemistry. Why are these measurements not included in this grouping? If you measure 13C or 14C on a bulk sample, isn't that "bulk geochemistry"? You might be able to circumvent this issue just by renaming the bulk section to "bulk elemental geochemistry" or something like that.

One thing that I think is also missing from the discussion is some mention of the possibility that the terrestrial sampling density may have missed some of the possible heterogeneity in permafrost chemistry. I realize that it's not easy to sample in this part of the world, but is there any reason to think that the results might look different if you had soil samples from 50 more sites? Why or why not? This would apply to the δ2H data, as well as the other data.

Specific comments:

Line 78 – change "into" to "in"

Line 99 – Personally, I'm not a fan of non-standard acronyms like this (ICD in this case). They require an elevated level of buy in from the reader, which I think takes away from the accessibility of the manuscript. That's just my opinion, there's plenty of precedent for this kind of thing of course.

Line 137 – At the introduction of the δ2H discussion, it might help to frame the study better if you begin by saying that you propose the new tool, as well as evaluate the performance using a suite of other geochemical data including the aforementioned δ13C-radiocarbon method.

Line 142-143 - These are nice papers, but they aren't really the best references to support the assertion that "the isotopic value of local precipitation is a function of local climate"

Line 149 - If you mean to give the maximum range you could point out that precip in east africa can be upwards of +50 per mil, while the SLAP2 (Standard Light Antarctic Precipitation 2) standard is -427.5 per mil.

Line 162-168 – The end of the introduction falls a little flat in my opinion. At the moment you say what you do in your study, followed by a general statement about why it's important to study these types of questions. What's missing to me is a statement that directly comments on how what you do with this study will help with these important questions. As it is currently written it doesn't setup the next section so effectively.

Line 195 - I think it is better to replace your internal lab sample codes with something more straightforward when reporting the results (things like "CH DY-3A" are meaningless to the reader and hard to remember). Include them in a data file or something if you want to be able to cross-reference with Vonk et al., 2013, but for presentation purposes I would simplify.

Line 232 – Remember to define acronyms at first use.

Line 243 – Check super/subscripting for H3+.

Line 244 – Give units for H3+.

Line 248 - The "methylation effect" language is odd to me, since it makes it sound like

what was quantified was the difference in δ2H values between the derivatized and non derivatized standard, rather than the δ2H value of the hydrogen in the added methyl group. Since the magnitude of the "methylation effect" will be different depending on what the δ2H value of the covalently bonded hydrogen in the methylated fatty acid is in addition to the chain length, you want to do the correction by mass balance. Probably that is what you did, but the language doesn't make it sound that way.

Line 251 – This call to table 5 is out of order since you haven't called tables 2-4 yet.

Line 259 – Not sure what you mean exactly by "with mean and standard deviations obtained from the literature values".

Line 296 – I might add a few words to the start of the sentence that begins on this line to make it clear that you are discussing distributions within individual samples, and that you are still talking about topsoil samples only.

Line 318 – This call to table 3 is out of order.

Line 354 – spell check.

Line 417 – change "proxies" to "proxy"

Line 452 – This is the first mention of results from the shelf-slope samples. In the methods you point out a reference for more information on the sampling procedures, but what about the laboratory analyses and results? This should be included in the earlier sections.

Line 454 - I like how you use the individual n-alkanes rather than arbitrarily averaging them together.

Lines 481 – 500 – Somewhere in this section, or somewhere else in the manuscript if it fits better, it would be good to discuss how variability within an end member might impact the results. This is important for both the δ2H and the δ13C-radiocarbon approaches, but it seems like it would be especially important for the radiocarbon. In addition to the acknowledged aging along the transect won't there be different ages within a topsoil permafrost? How might this impact the results if melting/erosion occurs at different depths/ages within a site?

Line 568 – As with the end of the introduction, I think that the end of the conclusion could go a little further to bring this study back together with the big picture goals. Remind us how "increasing our understanding of the fate of thawing permafrost in the coastal environment" will help us and why we should care about it.

Figure 2 – I would add the color legend to this figure that you already use on the other figures. I would also list n values in the caption or on the figure.

Figure 5 - I would list the modern/ICD labels as headers rather than within the data.

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