The Cryosphere Discuss., 8, C2596–C2599, 2014
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Interactive comment on "Strategy of valid ¹⁴C dates choice in syngenetic permafrost" *by* Y. K. Vasil'chuk and A. C. Vasil'chuk

Anonymous Referee #2

Received and published: 6 December 2014

This paper is mostly a review of 14C chronologies from permafrost regions, with a special emphasis on a few sites from Arctic Siberia. I've read through the paper twice now, and I have to admit as a series of brief case studies, I remain unconvinced that an overall strategy is forthcoming from what the author's present.

The paper needs substantive editing and reorganization along with a consideration of perhaps focusing on more detail on fewer examples that may have a simpler message in terms of the overall strategies of 14C selection (which is the main focus of this paper).

General points

1. The authors seem to accept all dates that have been published as being reliable

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indicators of when that organism died They mention a couple exceptions related to thermokarst lakes in Alaska, but overall they accept that most dates are reliable and the problems lie with remobilization of older (or curiously younger- which I return to below since I don't follow their point) organic material. At no point is there any discussion of the mechanics of 14C, importance (and sensitivity) of pre-treatment protocols and dating specific fractions (fulvic, humic acids, high temp/low temp combustion etc- a rich literature- see Brock et al. 2011 Quat. Geoch, 5, 625-630), or the problems that can creep into datasets.

For example, in the last 14C intercomparison something like 10% of 14C dates were simply wrong for a myriad of reasons from incomplete pre-treatment, sample labelling and errors in the lab, changing background detection limits, etc. I suspect many of the ages that are reported in this paper are actually non-finite with some young 14c contamination. This has certainly been our experience, that dates that are ca. 35,000-45,000 14C years BP are in fact non-finite and have some younger contamination because of poor handling of samples and microbial growth, poor background estimation (and thus subtraction of the blank) or the fact that blanks are still not known from most laboratories that service users. Almost no papers report background values of the blanks which is critical to understanding the reliability of 14C dates, especially as you approach 14C background (which again is not discussed).

Keep in mind that as little as 1% young carbon contamination in a non-finite sample results in an age of ca. 38,000 14C years BP. This is a huge problem and most studies do not report background values for their blanks or mass-dependent background. With small sample masses blanks (and thus non-finite samples) can be reported in the 20,000 year range. If you don't known the blank this will seem as a solid date. The paper by Kennedy et al. (QSR, 29, 217-225) has some discussion of this problem. That paper also has quite an extensive discussion of the importance of choice of material for dating, such that fragile macros and those that are ecologically coherent with the environment that one is dating are typically younger and well-preserved and robust

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macros (including spruce needles and wood) tend to be older and reworked.

The authors also enter into some discussion of bone dating – which is another complete literature on the importance of things such as bulk methods of collagen extraction (Longin method) vs. ultrafiltration (Brown et al. 1988; Stafford, 1996), vs. single amino acids, etc. that can lead erroneous dates. I'm not suggesting the authors be exhaustive, but rather define their scope early- at present the paper tackles far too much- driftwood, marine sediments and modern reworking, etc. etc. . .

2. In the abstract it is stated: 'due to the very good preservation of organic materials in permafrost conditions and numerous re-burieals of the fossils from ancient deposits into younger ones the dates could be both younger and older than the true age of the dated material'. I'm afraid I don't follow this point. How could younger organic materials be incorporated in older sediments in syngenetic permafrost? This could happen, and does happen with pore waters through the active layer that accumulate at the top of the permafrost table, but for the most part these waters would not be able to carry organic material with it (that isn't DOC I suppose) or through cryoturbation this could happen with active layer mixing. Is this what the authors mean? This point is made on p5590, L5, and p5594, L3. Without some explanation, this is a strange point to make.

3. 'subaerial-subaqueous deposition'. What do the authors mean by subaqueous deposition? The authors appear to follow the loessal origin of yedoma in Siberia (p5590, L23) such that I don't follow what they mean by subaqueous. By definition it means 'underwater'. And if that's what they mean, how does permafrost survive under a body of water? Please clarify this point through the text. There are many ways that syngenetic permafrost can aggrade- due to loessal inputs (the main North American model for syngenetic permafrost in eastern Beringia- see Schirrmeister et al. 2014 Encylopedia of Quaternary Science), colluvial inputs, aggradation of peat and vegetation, or perhaps fluvial inputs. Each of these has their own setting and challenges.

4. Case studies not especially clear. Many of the case studies simply refer to a series

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of ages and accept that the youngest ages are correct and therefore reliable. See point 1, but there are many ways for 14C dates to be wrong and it's only through rigorous laboratory methods coupled with excellent sample selection (and ideally understanding the ecology of the samples that are being selected) such that they form a coherent set of samples with robust ages that one can move toward confidence in the dating. I would prefer to see a couple of case studies with stratigraphic logs an dmore clear discussion of why the dates are reliable rather than many paragraphs that are difficult to follow without going back to the earlier papers.

Perhaps focus on Duvanny Yar and the problems that site still presents (you might look at the dates reported in Willerslev et al. for that site in addition to what is presented here). I would assume there are 50+ dates for Duvanny Yar, while only 4 are reported on Table 2. That site could well warrant a serious discussion of the issues of true 14C background, reworking of old material, importance of macrofossil selection (i.e. ground squirrel nests vs detrital material), etc. As it stands, one would think the site only has a brief handful of ages available.

Maybe the Fox permafrost tunnel- which is still a challenge- but also see the paper by Lachniet et al. and maybe Wooller et al. in addition to those noted.

Overall, the paper has some merit and with considerable reorganization and perhaps a better framework for the discussion could be an addition to the literature, but as it stands, it needs considerable re-thinking before it can be published.

Interactive comment on The Cryosphere Discuss., 8, 5589, 2014.