The Cryosphere Discuss.,
doi:10.5194/tc-2016-160-RC3, 2016

The Cryosphere Discussions
Open Access
EGU

Interactive comment on "Radiocarbon dating of glacier ice" *by* Chiara Uglietti et al.

Anonymous Referee #3

Received and published: 30 September 2016

The manuscript "Radiocarbon dating of glacier ice" by Uglietti et al. aims to give an overview of the actual 14C dating method applied by the authors group (i.e. at the Paul-Scherrer-Institute (PSI) for absolute dating of glacier ice via 14C analysis of the water insoluble organic carbon fraction WIOC. After the presentation of the dating problematic, an overview of the method development at PSI over the last 10 years is given in the introduction. This is followed by a description of the actual analytic procedure and comparison with the previous method is presented. Dating uncertainties are estimated, followed by an overview of different validation attempts for the 14C dating method performed by the authors group over the last years. Finally, a potential application of the method is provided by compilation of 14C dated non polar ice cores in which the discrete 14C ages are aligned with basic glaciological or empirical fits to retrieve continuous age depth relationships for the respective ice cores.

The application of the 14C method is an important topic within the challenge of ice core

Printer-friendly version

Discussion paper

dating and it is suitable to be published in The Cryosphere. The scientific level of the manuscript is already good, however I would have some suggestions and comments, which the authors might consider to further improve the quality of the manuscript.

The manuscript seems to slightly hover between a historical review and very specific descriptions of recent methodological improvements and validation exercises done by the PSI group. Since the manuscript is strongly focused on the activities of the authors group, I think the title can be somewhat misleading and I would suggest to adjust it in an adequate way. If the manuscript should keep the general review character of the 14C ice core dating topic, as the title implies until now, it could benefit from a slightly broader view and discussion of radiocarbon dating methods for glacier ice that have been developed by other groups in the past. This could for example include dating of the dissolved organic carbon fraction but also a more detailed discussion on potential dating difficulties e.g. due to reservoir effects introduced by dating of material which was already aged at deposition etc. A few references to the work of other groups would be very helpful in this case.

Specific comments:

The two-parameter model (Section 6) is only a fit to the data by adaption of the two variables, no independently derived age-depth-relationship and should possibly also be discussed like this. Nye's flow model can only be reasonably applied in the vicinity of ice divides of the polar ice sheets, where no additional horizontal flow component e.g. by a tilted bedrock, occurs. For cold Alpine glaciers frozen to bedrock it will systematically underestimate the age in larger depths. Also the assumption of a net accumulation to be constant in space and time will not hold for most Alpine glaciers. Therefore the fitted results will only give a very rough estimate of the age depth-relationship. Could you comment on that in the manuscript?

Fig. 1: I think this figure is highly redundant with the data shown in Tab. 2 and provides no additional information. The probability distributions are shown in very small size and

do not show any special wiggle features of the calibration curve, so in my opinion this figure could also be omitted.

Fig. 3 and Section 5: I think it is critical to infer general conclusions on the accuracy of the method for any application from only seven data points shown in this figure. It is not a priori given that this validation will work in the same way for ice bodies in all kinds of different environments and glaciological settings. I think it is good to state the successful applications discussed in this manuscript, but I would be careful to draw general conclusions for future applications of all kinds. I recommend to slightly weaken the paragraph in this respect.

Fig 6: This figure seems to be problematic to me. What is the goal to show all the ice core chronologies on one timescale? Is it just a methodological compilation of all dating applications so far? Then in my opinion it should also be discussed in a purely methodological sense. At the moment, the figure is a compilation of ice core chronologies from glaciers with very different glaciological and geographical settings. It could be highly misleading in a fact that all the very different ice bodies are assumed to show comparable age-depth-relationships, which is not the case as also mentioned in the text. Could it be a possibility to compare only the glaciers with common features in one plot? Like only the cold ones or the three that have been fitted with the two-parameter model? Or separated geographically? In this context I have another question: What is the reason for the different basal behavior of the Illimani and Tsambagarav ice cores compared to the others? The basal chronology sections of these two cores differ significantly from the others. Thus, I think presenting all chronologies in one figure with only a very short glaciological description is insufficient. Either the chronologies should be discussed in a more methodological sense or the glaciological description and evaluation of the 14C-Data (section 6) needs to be extended with respect to the specific dating problems (e.g. ambiguity of volcanic horizons for the Alpine cores) of each sampling site.

Tab. 2: The radiocarbon ages derived by the Sunset method seem to be systematically higher than the ages derived by the THEODORE system. What is the reason for that,

can you comment? Like Referee #2, I also noticed the discrepancy between the F14C value of Bel2_THEODORE and the given radiocarbon age. I calculate an uncalibrated 14C age of 6847 yBP for that sample, thus younger than the comparable Sunset-sample. Please check.

L 11: Please specify what you mean by upper part of ice cores. How is the upper part characterised, which features separate it from the lower part?

L 16: Note that Steier et al. 2006 also published a method for micro POC 14C-dating of glacier ice. Could you give a reference here?

L 30: I think the statement about the potential of the method for dating every piece of ice is a little too general. Can you specify the prerequisites for application of the method, like concentrations of organic carbon and knowledge about the sources of the organic material?

L 41f: Could you give references for the seasonal variations of the trace components e.g. Preunkert et al. 2000 for ammonium and mineral dust components?

L 45: Change nuclear to radiometric.

L 50: Please also reference Wagenbach et al. 2012 for layer thinning and the non-linear age depth relationship of Alpine glaciers.

L 75f: Can you be a little more precise here? Not only enough carbon mass is needed, but its needs to be ensured that the dated material represents the age of the surrounding ice and was not already aged at the time of deposition. Please add a comment on that.

L 78: DOC can be extracted not only by wet oxidation but also UV-irradiation like it was done in May et al. 2013.

L 85f: Is it possible to give a reference for the fact that OC has a lower probability for built in reservoir ages than EC?

Printer-friendly version

Discussion paper

L 109: How was the stainless steel band saw pre-cleaned?

L 121: Why does the HCL has to be removed by additional rinsing with ultra-pure water?

L 128: Typo: Of

L 142f: I think the statement on graphite target measurements becoming “needless” is maybe a little too harsh. Gas measurements can only complement graphite measurements in cases where not enough material is available at the price of lower precision. Please reformulate.

L 157: Can you describe or give a reference for the protocol Swiss 4S?

L 159f: Did you take into account potential loss of material by the additional rinsing step? Did you investigate the filtration efficiency of the system in general before and after introduction of this step? This could provide information about the characteristics (size, etc.) of the retained particles. What kind of standard materials did you use? Have they been treated exactly like the ice samples?

L 201: Could you move the description of the procedural blanks from line 215 to this section? Here the term “procedural blank” occurs for the first time.

L 202: Do the given masses and F14C values only refer to the OC fraction or the total blank contribution of the system on all carbon fractions? How have the results been corrected for these blank values? Please clarify.

L 207: Not only the size but also the age has a large influence on the counting statistics and the uncertainty. Please amend.

L 210: You state that “solid grains” of the standard materials have been used. How have they been combusted? In the Sunset system or in the standard AMS preparation routine using an EA? Could you please clarify?

L 220: Why do you merge both procedural blanks of the THEODORE and the Sunset

system into one number? I think this is not reasonable, because each sample should be corrected with the blank values of the respective system. Could you explain?

L 270f: Can you give a glaciological scenario which could explain such a large age increase (ca.1000 years) in only a few cm of depth increase (below the plant fragment layer) in such a small scale, low altitude and probably temperate ice body? Do you have any information on ice temperature? Could the samples JUV0_3-JUV0_8 also be influenced by basal sediment and thus produce a significantly higher age? Please comment on that, I think only a slightly larger depth in this order of magnitude is not a sufficient explanation for the observed age increase.

L 288f: How long in depth were the subsamples? Can you provide information on the (estimated) annual layer thickness in the respective core depths and thus the expected time span covered by the subsamples? This could help to evaluate if the assumption of same 14C-age of the adjacent samples is realistic for the two upper sampling depths. In turn, can you asses if the large age increase in the basal section is realistic in terms of covered core depth of the samples? Also the grey symbols in Fig. 5 are very small and hard to distinguish from the background.

L 325: Note that especially for sites in the European Alps volcanic eruptions can be masked significantly by frequent inputs of Saharan dust (see e.g. Preunkert & Legrand 2001) and thus the signal can be highly ambiguous. Could you please comment on that?

L 335: What do you mean by “purely empirical approach”? Please clarify. Can you quantify that approach?

L 341: See comment to L 325. Please differentiate the different types of absolute horizons and their respective uncertainty.

L 349: Can you be more precise here? What are the exact prerequisites for ice bodies to be dated by the method? Is it also applicable for temperate ice, where meltwater is

Printer-friendly version

Discussion paper

present?

L 354: Please add a reference to Wagenbach et al. 2012 for the bedrock d18O-anomaly. L 357: Because of the low depth resolution, the fact of mixed ages contained in one 14C-sample holds for almost every core depth (depending on accumulation), not only for the basal layer. Please clarify.

L 379: In section 2 you stated that in total 600-800g of ice are needed for decontamination. I think this number should also be given here.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-160, 2016.

Printer-friendly version

Discussion paper