Comment on tc-2021-375

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Referee comment on "Using ice core measurements from Taylor Glacier, Antarctica, to calibrate in situ cosmogenic ^{14}C production rates by muons" by Michael N. Dyonisius et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-375-RC2, 2022

Basically, this paper describes measurements of cosmic-ray-produced C-14 in subsurface ice and shows that they are a lot lower than predicted by existing estimates of subsurface production rates by muons in ice.

This is a bit of a problem because production by muons in ice is fairly analogous to production in quartz (SiO2) in subsurface rocks. The latter has been reasonably well established by laboratory measurements of interaction cross-sections and also by measurements of cosmogenic C-14 in subsurface rocks, which is essentially the same thing as what is being done here, only in rock rather than ice. The concentration measurements in subsurface rocks are, so far, more or less consistent with the laboratory estimates. However, if one takes the production calculation that appears to work in rock and applies it to ice (of course, ice is a rock too but you get the idea), it implies that there should be about 3 times as much C-14 as is measured in this study.

Thus, the workflow in this paper is as follows:

1. Measurements of C-14 concentrations in subsurface ice.

2. Predictions of the expected C-14 concentration based on an ice flow model and existing estimates of muon interaction cross-sections.

3. Comparison of the two, which shows that the measurements are ~30% of what is predicted.

4. A conclusion that previous estimates of muon interaction cross-sections are wrong.

Starting from the top here, I don't have the expertise to evaluate whether or not the measurements were done correctly. I have never personally attempted to extract C-14 from ice. So for purposes of this review we will have to accept the measurements. Certainly the depth-dependence of the results look as expected, so it appears unlikely to impossible that the measurements are totally wrong. Whether the extraction procedure results in a systematic loss that would maintain the expected form of the depth profile, I can't evaluate.

I did replicate the authors' calculations of predicted C-14 concentrations. As discussed in the paper, these start with a flowline model that predicts changes in sample depth over the past several thousand years, so the assumptions involved are (i) the time-depth model, (ii) the calculation scheme from muon production, which is standard and taken from several papers by Heisinger, and (iii) the muon interaction cross-sections. I used my own code for this and got close to the same result as they did, specifically that the measured values are about 30% of the predictions.

This is very hard to explain. The reason it is hard to explain is that there are a lot of measurements in quartz in rocks that tend to validate the laboratory-measured muon interaction cross-sections. Specifically, there exist quite a lot of data, unfortunately many of which are not yet published, from moderate depths (like, a couple thousand g/cm2) that require that the Heisinger/Lupker negative muon capture cross-section for C-14 from O be approximately correct. These include data from cores (the Lupker paper and some other unpublished data from the Beacon Heights core) as well as a lot of measurements of the C-14/Be-10 ratio in bedrock surfaces covered by meters of glacier ice. If the negative muon capture cross-section was 1/3 of what we think it is, then the measured 14/10 ratios would be impossible. Thus, the negative muon capture cross-section, at least when SiO2 is involved, can't be a lot less than the Heisinger/Lupker estimate. Fast muon interaction cross-sections are not well constrained by any of these data, so they could be lower. Of course a huge advantage of ice over rock is that you can increase the amount of ice almost without limit to make accurate measurements at very low concentrations, which you can't do for rock, so measurements in the fast muon production zone will never be as precise for SiO2. But, as the authors here point out, you can't match the C-14 in ice data just by lowering fast muon production, you have to lower both by a a lot.

The authors here don't have a good explanation for this, and neither do I. There does not appear to be any physical explanation for why essentially the same calculation for SiO2 and H2O would be approximately correct in one case and quite wrong in the other case. Of course Heisinger's measurements were in SiO2 not H2O, but unless I am completely missing a major area of particle physics knowledge as to why hydrogen would suppress this reaction, the conversion should be straightforward. Of course it is nearly certain that I am missing some particle physics knowledge, but in this case Heisinger and his PhD committee would have to also miss the critical information, which is less likely. This leaves three options.

First, the measurements could be wrong. As noted above, I don't really have the expertise to evaluate this, but there is no obvious way that I can think of that they would be systematically incorrect by a factor of 3-4. However, as noted below, I think it is worth examining this more critically.

Second, the muon interaction cross-sections for production of C-14 from O could be wrong. This is the solution the authors favor. This could be the case for the fast muon interaction cross-section, because if we accept the measurements here, it is well-constrained by these data but not very well constrained by data from rock cores. However, it cannot be true for the negative muon capture cross-section, because that is fairly well constrained, or at least limited on the low side, by geologic data. As noted, unfortunately many of these data are not yet published. So this possibility is not likely to be a complete explanation.

Third, the time-depth estimate from the glacier flow model could be wrong. Unfortunately, it would have to be very wrong to totally explain the mismatch -- I experimented with this a bit and found that the average depth would have to be order 2x higher than proposed to reconcile predictions with measurements, which would maybe exceed the thickness of Taylor Glacier. So even though this is, in general, a potential source of errors because there are a lot of assumptions involved, it doesn't look like it can be wrong enough to completely explain the difference. However, the inferred negative muon capture cross-section probably is quite dependent on the ablation rate during the last part of the age-depth history, so I think there is a possibility that incorrect assumptions in the later stage of the time-depth history could explain some of the mismatch. An experiment to investigate this would be to assume the Lupker/Heisinger value for negative muon capture likelihood and try to improve the fit by varying both the fast muon cross-section and the ablation rate in the last couple of thousand years.

Similar reasoning applies to the comparison with the van der Kemp data. As I understand what they did there, their conclusion that the muon production rates were overestimated was based on the assumption that the long-term ablation rate obtained from depth profile fitting should be the same as the ablation rate measured by surface stakes. This doesn't make any sense to me, because I see no reason that there could not be factor-of-two-plus variability in the ablation rate during the last few thousand years. Is there something I am missing here? So if this assumption is relaxed, these data would not, I think, be inconsistent with expected muon interaction cross-sections. In any case, I don't think the argument that the van der Kemp data support the conclusions from this study is very persuasive.

So, to summarize, this paper shows a contradiction between observations and predictions, that the authors can't resolve, so they end up by concluding that the independently determined muon interaction cross-sections are wrong. I disagree with this -- this may be a partial explanation but is not a complete explanation. A problem with this paper, however, is that basically this conclusion is the same as saying that the only part of the research not done by the authors is wrong, which tends to discourage critical examination of the part of the work that they did. Thus, I would lie to see this paper try harder to explain the discrepancy by looking at all the parts of the picture more critically. For example, could we assume that there is, in fact, 25% C-14 loss into organic carbon, lower

the Heisinger negative muon capture cross-section by 10%, and see how close to the measurements we can get by adjusting the fast muon cross-section and the ablation rate?

Another way of saying this is that before changing all the existing muon interaction cross-sections, which were measured in high-quality lab experiments and are at least in part supported by geologic data, this paper needs to make more of an effort to exhaust alternative explanations for both these and the van der Kemp data. I see that just hacking the cross-sections by ~70% is very simple and gives what appears to be a good match to the data, but even though this is simple I don't think it's anywhere near sufficiently persuasive to disregard a lot of preexisting data which do not appear to have anything wrong with them. I strongly encourage the authors to back off on this in the conclusions of the present paper and be more critical of all aspects of the calculations and measurements.