Geochronology Discuss.,
https://doi.org/10.5194/gchron-2019-2-AC1, 2019

Interactive comment on "Chlorine-36/beryllium-10 burial dating of alluvial fan sediments associated with the Mission Creek strand of the San Andreas Fault system, California, USA" *by* Greg Balco et al.

Greg Balco et al.

balcs@bgc.org

Received and published: 20 June 2019

Both reviews of this paper were very positive and recommended publication with minor revisions, but highlighted a number of sections of the text that were unclear or lacked needed details. We appreciate the careful attention to the paper by both reviewers, and here we respond to the comments in the first review by Irene Schimmelpfennig.

Note that this review included a number of comments that referred only to typographical errors or minor technical corrections. We have corrected these in the revised text and do not respond to them specifically here. Review comments are highlighted below in italics and our responses follow.

Specific responses:

Line 22 – page 2, line 1: maybe clarify that "sediment is exposed to the surface cosmic-ray flux during erosion" refers to processes in the source area of the material and also includes bedrock.

This is correct. We clarified it.

Page 2, 7-9: It would be appropriate to be a bit more specific, i.e. to give examples of the methods that you compare the cosmogenic nuclide burial dating with and add references. Do you mean to imply here that burial dating is only useful in arid regions?

We did not mean to imply this. We have clarified this section of the text.

Page 3, line 17-18: Bierman et al., GCA (1995) attempted to use cosmogenic 36Cl produced from Cl in fluid inclusions in quartz to quantify erosion rates and exposure ages. I think it would be appropriate to cite this study.

We agree, and we appreciate the reviewer's reminding us of this study, which we had totally forgotten about.

Page 5, line 28: "dated to 0.5 – 1.1 Ma" using which method?

This age assignment relies on a variety of lines of evidence, including magnetostratigraphy, tephrostratigraphy, and stratigraphic correlations, that are described in detail in the paper by Kirby and others that is cited in this sentence. As the present paper is not about the Ocotillo Fm. and the purpose of this sentence is simply to note that previous mapping has assumed that the alluvial fan sediments in our field area are Plio-Pleistocene because they are generally similar to known Plio-Pleistocene sediments in the region, we have not added any additional discussion here.

Page 5, Line 33 – Page 6, lines 2: I guess the samples from the three stratigraphic levels were taken along a horizontal transect and not on a vertical transect due to issues of accessibility?

The specific sample sites for each stratigraphic level were mainly chosen so that we could sample from sites where there was evidence for rapid erosion, thus minimizing the possibility of significant cosmic-ray exposure during exhumation of the sites. We were best able to achieve this by sampling from a series of rapidly eroding stream banks and valley walls located along a channel incised into the alluvial fan system.

Page 7 line 14: "a double-isotope-dilution method": I guess you refer to the routinely used isotope dilution technique that is described e.g. in Ivy-Ochs et al, NIMB (2004) and Desilets et al., Chemical Geology (2006)? Otherwise please clarify.

This is correct in general terms, although we are not certain that our procedure is identical to theirs in all respects. Thus, we did not refer to a single previously published description, but instead described our laboratory procedure in detail, including the characteristics of the isotopically enriched spike that we used. This description is in lines 15+ of page 7 of the submitted text. In addition, the supplemental material contains additional characterization of the spike as well as all the isotope dilution calculations.

Page 8, line 7: in which SGS lab the aliquots were analyzed?

SGS Mineral Services, Lakefield, Ontario, Canada.

Page 9, line 6: Please note that thermal neutrons are directly produced during spontaneous fission of U, but indirectly during decay of U and Th through alpha,n-reactions (see Gosse and Phillips, 2001; therein called "nucleogenic" instead of radiogenic 36Cl, which would be probably more correct)

Correct. "Nucleogenic" is more accurate. Corrected in revised text.

Page 10, lines 1-2 and 16: Please quantify the "small effect" of these different muon-related scaling differences. E.g. according to the models you use for the calculations, what are the muogenic 10Be and 36Cl contributions in the range of altitudes of the considered catchments?

In the submitted paper, we did quantify the uncertainty in burial ages associated with not knowing the source elevation of a sample, in line 30 of page 16 of the original text. In the revised text we have added a reference to the later discussion.

Page 11, line 8: for completeness it could be added that 36Cl production from K spallation and 10Be production in quartz are assumed to scale identically.

This was stated in the submitted paper in line 17-18 of page 10.

Lines 9-11: Here again, please quantify the "significant fraction" of 36Cl produced from muons in the considered depth range. This sentence seems in contradiction with lines 1-4 of the same page, where you imply that the muon-related 36Cl production and associated inaccuracies in the considered subsurface are low enough to be insignificant. Please clarify.

We agree that this section of the text would benefit from some clarification, and we have done this in the revised text. Basically, there are two issues here that we have mixed up.

If we can assume that a sample that we are trying to burial-date was initially exposed at the surface, then uncertainties in muon production rates are insignificant, because muon production makes up such a small fraction of surface production.

On the other hand, for the fluvial clasts with measured ratios that are higher than expected for surface exposure, one possible explananation is that the clasts were never exposed at the surface, but were exposed in the subsurface and delivered directly to the channel by landsliding. The reason the production ratio is higher in the subsurface is that muon production makes up a significant fraction of total production in the subsurface. However, the fact that we have no idea what depth the samples came from creates a much greater uncertainty in what we expect the source production ratio to be than any uncertainty in the muon production rates themselves. So in this situation we are very uncertain as to what the source production ratio is, but this is not because of

the uncertainty in muon production rates.

The important thing is that in neither situation do we really care about the uncertainty in muon production rates. It's either (i) insignificant, or (ii) significant, but much less important than other bigger problems.

Lines 15-17: It should probably be added that the predicted production ratios depend on altitude and feldspar composition and are therefore different. Which sample-specific characteristics are included in the "calculated surface production rates" that you use to normalize the nuclide concentration: scaling, but also feldspar composition, right? I guess the calculations are those in equation 1?

Again, we agree that this section of the text needs to be clarified. As this is a production-rate-normalized diagram, the simple exposure and steady erosion lines are computed for an idealized sample with surface production rates of both nuclides equal to one. The sample-specific production rate calculations are used to normalize the measured nuclide concentrations so that they can be plotted on the diagram. This operation is routine in the commonly used Al-26/Be-10 two-nuclide diagram, but this diagram is rarely used for Cl-36, so we agree that more explanation would be useful. We have added more explanation.

Page 11, line 17-Page 12, line 2: What would happen if the samples were not steadily eroded? On page 16, lines 18-19, you seem to imply that if the sample didn't originate from a steadily eroding surface, the burial age could not be determined – probably due to the varying production rate ratio? Is this issue unique to the 36Cl-10Be pair?

We did not mean to imply this. "Steady erosion" should read "surface erosion" and we have so corrected the text.

Page 12, lines 9-11: this seems like a circular argument: you use the comparison of the observed to calculated ratio to check whether or not the sample is at steady state erosion (lines 3-5), and here you use the same comparison to conclude that the applied

reference production rates are accurate. Please clarify.

Once again, we agree that this was not clear in the submitted text. The correct line of reasoning is: (i) the geological context of the sample implies that it has experienced an extended period of surface erosion; (ii) thus, the sample data must lie on the steady erosion line in a correctly constructed two-nuclide diagram; (iii) as we observe that the data do lie on the steady erosion line, the diagram must be correctly constructed, i.e. our production ratio calculation must be correct. We revised the text to better communicate this.

Fig. 7, caption: Regarding the sentence that starts with "Note that the steady erosion. . .", please specify "high erosion rates". According to the diagram, the sim- ple exposure line lies below the steady erosion line from the left-most erosion rate (âĹij15 m/Ma) on, so over the whole spectrum of considered erosion rates.

We corrected this by increasing the x-axis range so that the crossover of the simple exposure and steady erosion lines is clearly evident.

Page 16, lines 3-5: Please clarify how you determined 0.2 Ma burial at a depth of 1000 g cm-2. Is this just a scenario that could explain the measured nuclide concentration assuming 100% production during burial? .

Yes, this is a representative possible scenario that we use to highlight the fact that even a very small amount of exposure during exhumation could account for a lot of the measured nuclide concentrations. Of course many other similar scenarios are possible. We have clarified this in the text.

Page 17, line 5: Please clarify where the burial age "near 0.5 Ma" comes from.

This is an approximate estimate that can easily be obtained from the two-nuclide diagrams in Fig. 6. We have clarified this in the text.

Interactive comment on Geochronology Discuss., https://doi.org/10.5194/gchron-2019-2, 2019.