Editor Notes and Revisions

From Dr. Greg Balco

First, reviewers took particular exception to the use of an isochron method in a way that might be proper (if in fact all the samples from different locations have the same exposure history) or might be improper (if they don't). While it is true that the paper as written clearly states that the samples are from different locations and might not have the same exposure history, I agree with the reviewers that this is an improper application of the isochron method -- it might be right in this case by accident, but in fact the sample set doesn't strictly satisfy the assumptions needed to apply an isochron method. Thus, I strongly suggest removing discussion of the isochron approach from the paper. This would also simplify the paper somewhat, which would be helpful.

We agree with this assessment and have removed it from the paper. Also, you will notice in the tracked changes file that some of the citations are highlighted in yellow. I had to edit these without tracked changes turned on because Zotero wouldn't work with it on. You will see in the new submission the citations are corrected.

Second, while, again, it is true that the paper as written does contain all the information about which samples were subjected to which analyses, reviewers had numerous comments that indicated that they were unclear as to when/whether observations made on one sample could/should be applied to another sample. Please try to be more clear about this in your revision, possibly by stepping through your reasoning more explicitly - '...if this observation for sample A also holds for sample B, then...'

We have added in and changed multiple sentences in the document so that the reasoning is clearer. You will also notice that we conducted another step-heating on VM-10 so that we could have both the stratigraphically youngest and oldest samples represented in the step-heating experiments. With this added information and combining both the C/F and step heating data, we determined that the four Volcano Mountain lava flows erupted approximately coevally, at 10.5 ± 1.7 ka. Because of this and from the feedback received, we have created a new version of our step-heat figure that is easier to follow.

Third, in your revision try to be clear about the generality, or lack thereof, of the applicability of the method. Do you think that step-heating is likely to separate helium components only in this unusual lithology, or in others as well? Is there any evidence from the literature as to whether this should or shouldn't work in any particular situation? Note that I am not asking for a lot of speculation about where various helium components are physically sited, etc., etc., but mainly just for a concise and realistic assessment of if/when it might be worth pursuing this in other samples/lithologies.

We have adjusted our discussion section to include the elements you are referring to.

With regard to the more specific review comments, these reviews included a large number of technical comments that, as you say, can mostly be addressed by fairly simple clarifications of the text. There are also a lot of comments relating to the history of various analytical methods, who did what first, and which papers should most properly be cited for various technical points. Please try to correct these issues as the reviewers suggest, but please also keep in mind that this is not a review paper and it is not necessary to summarize the entire history of trying to deconvolve various helium components for purposes of exposure-dating. It would probably be appropriate to give a short description of the various methods proposed and refer the reader to review papers for the details.

We have added in the proper citations and more sentences to give credit properly.

One technical comment I will address specifically is the suggestion that the results may reflect mixing of two mantle/inherited components rather than one mantle component and one cosmogenic component. Although of course it is difficult to completely exclude this possibility without shielded samples of this lithology, I agree with your reasoning here that (i) there is not an obvious mechanism by which two separate helium inventories could be kept distinct during the likely thermal history of the rock, and (ii) because the samples are now at the surface, we know there must be a nonzero cosmogenic component.

Finally, I am appending here four technical comments from the fourth person who supplied me with comments after the online review period closed; please also take these into account in your revision, especially the remarks related to Table 4.

1. The paper implies throughout that it is possible to completely separate the cosmogenic and mantle helium components in olivine by crushing. Mantle helium dominates in the inclusions, but mantle and cosmogenic helium are mixed in the olivine itself. The text will be misleading to non-expert readers in a few places. A few examples: Line 56 “When powdering does not effectively remove the mantle component”. In my experience, powdering never fully removes the mantle component; Line 206: “the greater complication arises from the fact that mantle helium is not effectively removed by powdering to < 30” This is true, but is expected; Line 217 “

Survival of the mantle component when crushing this fine is not typical" This statement is probably incorrect. I know of no examples where crushing completely removes the mantle component, for xenoliths or basaltic crystals.

The original method of coupled crushing/heating never assumed that the two components could be separated, but that they can be "distinguished" by virtue of different residence sites and drastically different isotopic compositions. Much of the mantle helium is held in melt and fluid inclusions (easily released by crushing, which does not release much cosmogenic helium) and most of the cosmogenic helium resides in the solid mineral matrix. I cannot think of any examples where they are fully separated in heating measurements, so the text should be edited to reflect this. There are examples where cosmogenic helium dominates due to long exposure. If I am wrong here, then the text should include references to support the assertions.

We adjusted this sentence and clarified this in the paper. We agree that separated isn't the best wording.

2. One unique aspect of this sample suite is that the xenolith olivines have very high initial mantle helium concentrations and the lava flows have fairly short exposure ages of ~ 10Ka. The samples are apparently all xenolith olivines. Most of the basaltic cosmogenic helium data in the literature comes from olivines that grew in a basaltic melt, rather than xenoliths, and have much lower helium contents typically < 10 ncc ^{4}He STP/gram. Therefore, this study may be a special case in the application of cosmogenic helium, leading to higher detection limits to the abundance of mantle helium. This should be pointed out somewhere in the text.

I have added this into the text, thank you for the suggestion.

3. Table 4 is misleading and should be modified, along with the text discussing it. The table gives cosmogenic 3He calculations for VM-09, 10, and 11. However, the fusion measurements for all three of these samples are indistinguishable from the crushing measurements and therefore the cosmogenic 3He is actually below detection, so tabulating cosmogenic 3He contents here is misleading, since it is below detection. It would be better to give the "upper limits" for those samples based on some estimate of detection limits. One simple way to calculate this would be to estimate lowest possible 3He/4He that would be distinguishable from the crushing data (e.g., 3He/4He of two or three standard deviations above crushing) and use that to estimate detection limits for cosmogenic 3He. (However, other factors like reproducibility and variable blanks may play a role too, so this is up to the authors.) If these detection limits are below the other three samples, is there some plausible geological explanation, such as erosion or soil or snow cover? Or perhaps these three samples were collected from a younger flow or

flows? Given the fact that the cosmogenic 3He could not be detected, it is not justifiable to include those three “zero-age” samples in combination of the other three that yielded more reliable measurements (the average of the six samples, line 253). It would be better to take the average of the three samples for which it was possible to detect 3Hec, which would lead to a different conclusion, i.e. that the crush/fusion results are significantly higher than the isochron method. At least discuss this possibility.

We entirely agree with this assessment of Table 4 and have changed the format. Most of the tables have been updated to be easier to follow and understand.

4. Figure 1 caption should include a reference for the map (Jackson and Stevens, 1992).

We have fixed this.

Overall, the result of all these reviews is a fairly extensive set of revision instructions. I hope what you will take from this is that the large number of reviews indicates that the paper is interesting and potentially valuable to readers, and therefore I hope that you will undertake these revisions.

-- greg