Review of Mueller et al., (submitted to Geochronology)

Disclaimer: I have only access to the original manuscript (I cannot see the updated version from the previous review). However, it should not be an issue regarding my major comments section but I am sorry in advance if any redundancy for the detailed comments section.

This manuscript is a scientific contribution describing the ^{3}He cosmogenic (^3He_c) extraction and determination from ^{3}He mantle-bearing olivine. The paper shows that ^3He_c can still be extracted from highly enriched ^{3}He mantle olivine using the step heating extraction method (Kurz 1986a). The authors are using olivine crystals sampled from 6 different lava flows from the Nelruna volcano (i.e. Volcano Mountain - Yukon). The authors analyzed the ^{3}He total signal with two different extraction methods (crush/fusion, and step heating) in order to separate the cosmogenic ^{3}He signal from the ^{3}He mantellic component. Their results show that only step heating method is able to separate precisely ^{3}He signal between the mantle and the cosmogenic components in these samples. Overall, I find the paper well written and easy to follow but some sections are lacking of detailed information (see major and detailed comments below). Moreover, after careful review of the paper, I have several concerns regarding the data interpretation, especially the crush/fusion and isochron dataset. I think there is some important consideration that the authors might want to look at, in order to get more detailed information on the mantellic source of the samples and/or the ^3He_c signal. I think more information could be retrieved from the crush/fusion data and could potentially reveal some signature from those tiny void inclusion, that deserve to be discussed (see major comments section below). In addition, considering the whole dataset (crush/fusion, isochron and step heating), I would consider that variable mantellic source signatures cannot be properly ruled out despite the fact that step-heating data seem convincing, and that ^3He_c component is potentially retrieved from those ^{3}He mantle rich bearing olivine. However, I do have some concern regarding the low temperature (800ºC) step heating signal. This temperature step is crucial for the authors to successfully demonstrate that they extracted the ^3He_c signal. Therefore, considering the importance of this He data (at T~ 800ºC), one could think that the $^3He/^4He$ signal (Ra ~12-13) could be potentially related to a more primordial mantle source preserved from their observed inclusion trails (< 10 µm) instead of the ^3He_c component. Indeed, the authors pointed out that a mantle component could be preserved in tiny (<10 um) void inclusion observed within the olivine crystal (see the electron backscatter image in Fig.4). If these tiny (<10 um) inclusions are preserving a deeper mantle source signature, the first temperature step would therefore preferentially extract signal from these inclusions (due to their very small dimension). Consequently, first temperature step signal would reflect the signature of the inclusions rather than the cosmogenic ^{3}He. I think it is important to fully clear out that concern as this temperature step is critical for the paper and therefore should be fully investigated. Please find my detailed comments on that issue in the major comments section below.

The figures and tables show clear information but some sections are not well enough documented. Below I have listed my comments, with major issues within major comments section while secondary comments are located in the detailed comments section. To summary, the paper is well construct but require additional more careful work on the data treatment. A more detailed discussion regarding the first temperature step from the step-heating experiment is needed and additional information can be retrieved from the crush/fusion and isochron dataset. Nevertheless, the paper is providing interesting information and I do recommend this manuscript for publication only after major re-work and attention to their dataset.

Major comments:

Crush/fusion experiment. The authors are providing two data points for the crushing experiment (sample VM-06 and VM-09) with a Ra at ~8.2, and 6 data points for fusion with an average Ra at ~8.7. However, for 3 fusion samples (VM-02, VM-06 and VM-08) the $^3He/^4He$ ratio (Ra) extracted is measured at 9.2 while the 3 other fusion samples (VM-09, VM-10 and VM-11) the $^3He/^4He$ ratio (Ra) extracted is measured at 8.2, similar to the crush value. The authors also re-analyzed only two crushed samples (VM-06 and VM-09) with a one-step outgassing (fusion) and the results show either a crush-like Ra at ~8 (VM-09) or a higher Ra at ~9.6 (VM-06). Consequently, proper crush/fusion methodology can only be conducted on sample VM-06 and VM-09 as they are the only two samples with pre-crushed data. However, the authors seem to assume that crush Ra value from samples VM-06 and VM-09 can be applying to the 6 different lava flows dataset (line 165). I would mention that only two crush values used to assume the mantellic source of 6 different lava flow is somehow under representative to my opinion. I would have preferred a proper crush/fusion dataset for all considered olivine presented in this study in order to reduce any potential variability of the mantellic source. If considering that ^{3}He extracted from fusion reflect the matrix component and thus release the cosmogenic ^{3}He (with the assumption that ^{3}He radiogenic is negligible), I would therefore expect that sample VM-06 (Ra ~9.6) either preserved more efficiently its ^3He_c or has an older age to build up a ^{3}He cosmogenic signal. However, the authors sample description mentions that sample VM-09 (Ra ~8) is taken from the oldest lava flow analyzed in this study, and therefore I would have expected higher Ra value from sample VM-09 than VM-06 if ^3He_c signal was released preferentially during fusion. Additionally, surface vegetation coverage could hinder the production of ^{3}He cosmogenic. However, the authors mentioned that sample VM-08, taken on the same lava flow as VM-09, was the most covered sample with an 8cm thick moss (line 100) but still gives a Ra value of 9.4, significantly higher than Ra signal on VM-09 (Ra ~8). Consequently, both the sample location and the vegetation cover don't seem to be a good explanation for the observed Ra values measured in those samples. If we assume that the two crushed Ra values from sample VM-06 and VM-09 can be extended to the mantle component signature for all the samples, the discrepancy observed between samples VM-09, VM-10 and VM-11 (Ra ~ 8.2) and samples VM-02, VM-06 and VM-08 (Ra ~ 9.2) remains difficult to explain. The authors seem to not discuss those Ra values (lines 211-212) but I think there is some important information that deserved some attention. Moreover, this might have some implication for the step heating experiment results.

A possible alternative hypothesis to explain the observed dataset could be that the olivine samples are preserving a less degassed (i.e. deeper) reservoir signature than MORB (Ra > 8) from tiny inclusions within the olivine crystal. Those tiny inclusions (< 10 µm), observed by the authors (lines 215-219) could bear an OIB-like mantle signature (Ra > 8) or a more variable mantellic composition (larger Ra range), and therefore the variability between measured samples could be explained with a more or less amount of those tiny inclusions within the olivine matrix. Consequently, this could reflect a different mantellic source composition between the different samples rather than ^{3}He$_c$ component. This would better explain the Ra value variation in the fusion samples. On a first order check, we can verify if the isochron conditions are verified following the simple case of ^{4}He purely magmatic (no radiogenic ^{4}He, as assumed in the paper). The isochron equation is given Blard and Pik (Chemi. Geol. 2008) as: $(^3\mathrm{He}/^4\mathrm{He})_{fusion} = {}^3\mathrm{He}_c \times 1/^4\mathrm{He}_{fusion} + (^3\mathrm{He}/^4\mathrm{He})_{magmatic}$. If the validity of the equation is verified, therefore the isochron should display a linear regression. I quickly plotted below the data from the paper using the above equation (Fig. 1):

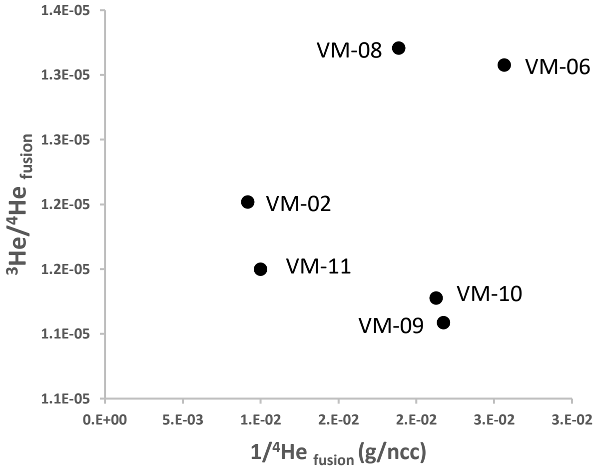


Fig. 1

The plot above does not show any convincing linear regression to my opinion, meaning that the assumptions are not met for the validity of the isochron. This could reflect either a variable (^{3}He/^{4}He)$_{magmatic}$ ratios in the different samples, an inhomogeneous ^{3}He$_c$ concentration or a similar ^{4}He magmatic concentration among the olivine crystals. If the samples have a variable mantle component (i.e. variable (^{3}He/^{4}He)$_{magmatic}$ ratios) therefore the isochron plot can be interpreted as a mix between MORB like mantle and a deeper un-degassed signal (OIB-like) contained in the tiny (<10 µm) void inclusion. That could lead to the observed ^{3}He/^{4}He signal in their fusion samples as the concentration between the different samples are dependent of the crystal size (and not controlled by ^{3}He$_c$). Indeed, if we observe the different masses of the olivine crystals, we can see that larger (or bigger) olivine crystal systematically lead to higher Ra value. For example, sample VM-02 (m = 0.25g), VM-06 (m = 0.5g) and VM-08 (0.32g) have an average Ra at ~9.2, while sample VM09 (m = 0.07g), VM-10 (m = 0.12g) and VM-11 (m = 0.1g) have an average Ra at ~8.2, in agreement with the assumption of a size/mass dependency of the Ra value due to the contribution of tiny inclusions (if we assume that the tiny inclusion concentration is more or less similar between samples). In addition, radiogenic component ^{4}He* is not mentioned in the paper but could also affect the ^{4}He signal measured from the olivine, as the last temperature step from the step heating experiment on sample VM-01 shows a Ra value of 7.9, lower than the value given by the crush dataset of Ra ~8.2. This potential ^{4}He* contribution could impact the step-heating temperature as Kurz 1986a showed that a very small fraction (<1%) of ^{4}He* on the first heating step can lead to a lower Ra signal. Consequently, the determination of the ^{3}He$_c$ signal could be likely underestimated due to the low resolution of the step heating experiments (3 temperature steps with only one at low T). This could lead to underestimate ages for the associated lava flows. This is not discussed by the authors while it can be important for the ^{3}He/^{4}He ratio.

Overall, the crush/fusion methods can only be properly applied on two samples, and those two samples (VM-06 and VM-09) show Ra values that are difficult to explain with a purely ^{3}He$_c$ signal at the moment. In addition, considering all the data with the assumption that (^{3}He/^{4}He)$_{crush}$ ratio from samples VM-06 and VM-09 is representative for all the samples, the Ra values are still difficult to explain with a purely ^{3}He cosmogenic signal. Therefore, a variable (^{3}He/^{4}He)$_{magmatic}$ ratio could better explain the variability of the Ra values. This variability in the mantellic signature could originate from a deeper mantellic source within the tiny inclusion (<10 µm) that are not extracted by the crush experiments due to their small size but are preferentially extracted from the temperature steps due to their very small diffusion domain. Therefore, the Ra variability observed in the fusion samples could simply reflect the variability of the tiny inclusion concentration between the different olivine. This agrees with the observed mass and Ra values where higher Ra are systematically observed with higher masses (i.e. higher numbers of tiny inclusions). As a consequence, the first temperature step on the step-heating experiments could therefore not reflect the $^{3}He_{c}$ signal, but rather reveal this OIB-like mantellic component from the tiny inclusions.

Step heating experiment. The major findings of the paper are provided by the 3 temperature step heating experiments (T_1 = 800ºC, T_2 = 1000ºC and T_3 = 1400ºC), and especially the first temperature steps (T_1 = 800ºC). The authors analyzed samples VM-01 and VM-08 with this protocol and revealed a $^{3}He/^{4}He$ (Ra) at ~12.3 and 13.2 respectively. This ratio is significantly higher than the one recorded by the crushing (Ra ~8.2) or the fusion (Ra ~ 8.7) experiments on average. The author interpretation is that higher Ra value reflect ^{3}He cosmogenic enrichment and therefore this Ra is the precise determination of the $^{3}He_{c}$ component is those ^{3}He mantle-bearing olivine. The two other temperature steps provide Ra value near ~8 and contribute little (T_2) or none (T_3) to the ^{3}He cosmogenic signal. The authors interpretation is that Ra value around ~8 reflect a MORB signature associated to a mantle component while higher Ra values are induced by ^{3}He cosmogenic production. This is supported by the crushed samples (VM-06 and VM-09) that give a Ra value at ~8.2 and reflecting the trapped magmatic ^{3}He (i.e. fluid inclusion). First observation is that I wonder why they didn't apply this methodology to all of their samples as it seems the only way to determine $^{3}He_{c}$ from these high He mantellic-bearing olivine. I would have expected, following their result on crush/fusion experiment, that they would have also re-analyzed the other samples to properly determine the $^{3}He_{c}$ and thus their associated ages, especially knowing that the authors took several samples for redundancy purpose (line 87). Secondly, as mentioned in the above sections, a variable mantellic source cannot be ruled out to explain the observed data from the crush/fusion and isochron dataset. Therefore, a variable mantellic composition that affecting the $^{3}He/^{4}He$ signal can also be explained with the step-heating experiment as this deeper mantellic component is assumed to be preserved into small trails inclusion (<10 µm). We can therefore assume that this signal is revealed preferentially at low temperature due to the inclusions very small diffusion domain (<10 µm). A way to test this hypothesis would be to perform a more detailed step heating experiment, at low temperature, to investigate if signal from a lower temperature (T ~500ºC) deplete those tiny inclusions and allow ^{3}He cosmogenic signal to be released at slightly higher temperature (T~800ºC). Such detailed temperature step protocol for $^{3}He_{c}$ signal on olivine powder has been described in Kurz 1986a to separate $^{3}He/^{4}He$ signal from very small faction of ^{4}He radiogenic signal implanted on the phenocryst surface (<1%). The $^{3}He/^{4}He$ ratio on lower temperature (T~500ºC) tend to have a lower Ra than the successive step (T~800ºC), as the first temperature step is likely more affected by ^{4}He radiogenic component (see Kurz et al., 1986a). In this manuscript, the signal at T~800ºC could also be affected by such radiogenic component or/and potentially by the tiny high-Ra mantellic inclusions. In both case, lower temperature step should either display lower Ra value if $^{4}He^{*}$ is implemented on the surface (even a very low fraction <1%) or higher Ra if reflecting preferentially the tiny inclusion contribution. The second temperature step (at T~800ºC) should therefore reflect a better $^{3}He_{c}$ signal if any. However, as this study present only one low temperature step (T_1), I would recommend to be careful with the signal at T~800ºC as additional processes affecting the Ra value cannot be ruled out. Consequently, lava flow ages presented in this study can be either under or over estimated. Additional important information can also be linked to processes in the mantle source (if we assume a variable mantellic component) of the analyzed lava flows (with a component from deeper mantellic source affecting the volcanic region). A more global geological implication could result from the paper and provide more insight of the volcanic system below the Nelruna volcano and its volcanic province.

Isochron method. In addition to the crush/fusion dataset, the authors are using the isochron method to extract a $^{3}He_{c}$ value from the fusion dataset. At line 172, they specify: "we assume the flow to have the same exposure age" while they specify between lines 93 and 98 that the lava flows have stratigraphy different ages from the youngest flow (near the cinder cone) to the oldest flow (third south flow). Therefore, samples collected on the different lava flows are expected to have different exposure ages. The authors are aware of such issue, (lines 173-174), but still try to apply the isochron method. I think a better approach would have been to use different aliquots of the same olivine population (sampled from the same lava flow) and cast these data into a proper isochron as defined by Blard and Pik (Chemi. Geol. 2008). That could have provided more reliable information on the potential $^{3}He_{c}$ signal. Therefore, no isochron is technically showed in this paper and I would be caution on the term used here. Instead, the authors are using 6 different lava flows merged altogether. The initial assumption of identical $^{3}He_{c}$ concentration is then not valid and a linear regression is not expected from the isochron definition given by Blard and Pik (Chemi. Geol. 2008). On the other hand, the authors are providing a plot with ^{3}He (pcc/g) vs. ^{4}He (ncc/g) following the relationship: $^{3}He_{fusion} = {}^{3}He_{c} + {}^{4}He_{fusion}*({}^{3}He/{}^{4}He)_{crush}$ (*equation 5 in the paper*). First, I want to point out that the data in table 2 display some discrepancies and are not matching the one plotted in Fig. 2. The exact Ra calculated from the ^{3}He and ^{4}He data are slightly different. Ra values for samples VM-06, VM-08, and VM-10 are given at 9.6, 9.4 and 8.2, respectively in table 2 while calculated values for these samples (directly from ^{3}He and ^{4}He data in table 2) yield to Ra values of 9.4, 9.5 and 8.1, respectively. Therefore, when plotting ^{3}He and ^{4}He directly with the data from table 2, I obtain a slightly different plot of the Fig. 2. See below the original Fig. 2 (left plot) and the revised Fig. 2, noted here Fig. 2a (right plot) with its associated linear regression equation and an estimated error envelop (from a regular linear regression fit in Excel):

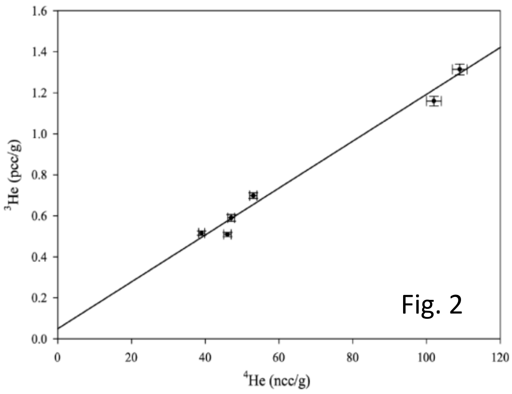


Fig. 2

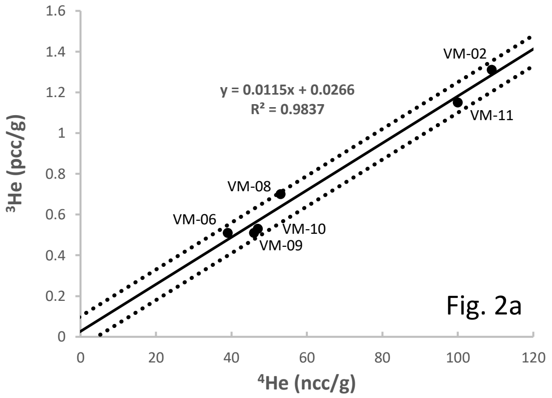


Fig. 2a

While I am not sure why some plotted data in Fig. 2 are not the one provided in table 2, I am assuming that ^{3}He and ^{4}He dataset in table 2 are the most reliable data source and I will consider then Fig. 2a as the best data representation. This slight discrepancy has, however, an important implication. Therefore, if we assume that this linear regression follows the equation 5 for Fig. 2a, the regression line at x = 0 should provide the ^{3}He cosmogenic value, calculated at 0.0266 pcc/g or 0.72 Matoms/g (see equation on the Fig. 2a). Note that this value is significantly lower than the one estimated by the authors (1.33 Matoms/g). I am not sure why I obtain such different value from the authors (despite the data discrepancy noted above). Please check if that estimation is correct. I also estimated the errors by taking an upper and lower envelop of the regression line to fit at best the dataset (see Fig. 2a). Then, the estimated error is 0.72 $^+/_-$ 2 Matoms/g. This is a very large error for such an apparent good linear fit and much larger than the one given by the authors (1.33 $^+/_-$ 0.68 Matoms/g). Therefore, some additional processes that affecting this dataset is likely to happen. An alternative approach is to consider that olivine crystals are actually representing two separate group sampling two different mantellic components (a MORB-like at Ra = 8 and an enriched Ra source). Therefore, samples VM-02, VM-06 and VM-08 (Ra ~9.2) representing the higher Ra source while samples VM-09, VM-10 and VM-11 (Ra ~8.2) representing a MORB-like source (see Fig. 2b below).

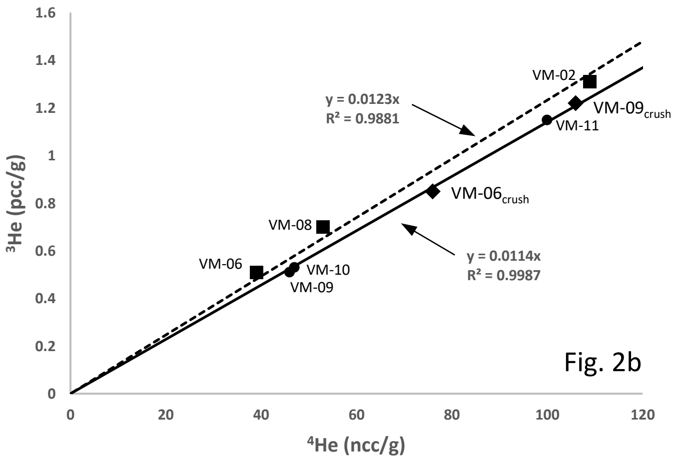
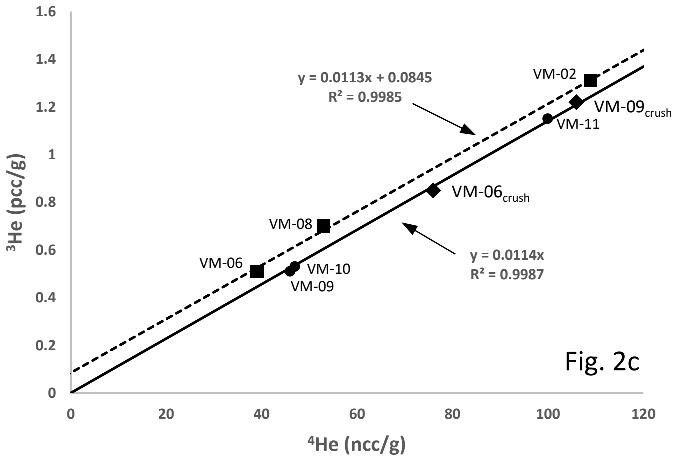


Fig. 2b

Data from the two crushed samples are also represented (solid diamond) but are not included in the regression calculations. If we also assume that no ^{3}He cosmogenic data is present in those sample, therefore the Y intersect is set to 0. The two linear regressions from this plot describe significantly better the observed data and allow to separate the two different mantle components, a MORB-like component (solid line) at ~8.2 (slope = 0.0114) for samples VM-09, VM-10 and VM-11 (solid circle) in agreement with the crush values (Ra ~8.2, solid diamond datapoint), and an OIB-like component (dashed line) at 8.9 Ra (slope = 0.0123) for samples VM-02, VM-06 and VM-08 (solid square) in agreement with Ra measured in those samples (Ra ~9.2) within errors. This agrees well with the hypothesis of a variable mantellic sources developed earlier (see major comments above). However, one could also consider that ^3He_c signal is responsible for the enriched Ra value observed in sample VM-02, VM-06 and VM-08. In such case, if we consider some ^3He_c in those samples, therefore the condition where the Y intersect is set to 0 is no valid anymore and the regression line for samples VM-02, VM-06 and VM-08 is shown in Fig. 2c below:



Consequently, important information could be retrieved from this Fig. 2c to support a $^{3}He_{c}$ component. First, linear regressions exhibit similar slopes giving a Ra at 8.16 for samples VM-02, VM-06 and VM-08 and Ra at 8.24 for samples VM-09, VM-10 and VM-11. However, while the slope value agrees with the crush value (Ra ~8.2), samples VM-09, VM-10 and VM-11 show higher Ra values (up to 9.5 Ra) which could then reveal the cosmogenic component. Therefore, the regression line at x = 0 could reflect an average $^{3}He_{c}$ value calculated at 0.0845 pcc/g or ~2.3 Matoms/g while regression line from sample VM-09, VM-10 and VM-11 gives no $^{3}He_{c}$ signal. This value could be a good estimate for average $^{3}He_{c}$ component for samples VM-02, VM-06 and VM-08. Secondly, $^{3}He_{c}$ value calculated with equation 5 (line 54) from the crush/fusion method for samples VM-06 give a value at ~2 Matoms/g. VM-02 and VM-08 do not have crush data but if we assume an average crush value, then $^{3}He_{c}$ is estimated at ~2.3 and ~2.8 Matoms/g, respectively, in fairly good agreement with VM-06 and the regression line values. Although this interpretation of the data cannot be ruled out, it has to be explained in the light of the sample location (i.e. expected lava flows exposure age), which still do not have a satisfactory explanation. The hypothesis of a variable mantellic source remains more likely to explain the data presented here, according to me. Consequently, the dataset provided by the authors is quite unclear to properly determine if ^{3}He cosmogenic is observed rather than a mix between two mantle sources. I would recommend to do a proper isochron (i.e. samples from same olive population), and calculate the pseudo-isochron with R-value determination (see Blard and Pik, 2008) to include the ^{4}He variability from different magmatic ^{4}He concentration (as well as radiogenic $^{4}He^{*}$), to better investigate the $^{3}He_{c}$ signal from these samples. Such simple ^{3}He vs. ^{4}He plot is not enough to capture the complexity of processes that can influence the He content in those olivine crystals.

Detailed comments:

The article seems to point out that they developed a new methodology to analyze ^{3}He cosmogenic signal by step heating the samples (see lines 68-69, and 292). This method, however, is not recent and has been used and developed by other authors before (Kurz 1986a). It would be more appropriate to refer at the step heating method as an already known approach (such as the crush/fusion or the isochron) but mentioning that you have adapted it with three temperature steps (800, 1000 and 1400ºC).

Line 20: "precise estimates of cosmogenic" is not really supported by the original manuscript as the step heating experiment (where the $^{3}He_{c}$ is the best measured) shows only one low temperature step, which already limit the precision of the $^{3}He_{c}$ determination. Indeed, very low (<1%) $^{4}He^{*}$ contribution (Kurz 1986a) or a variable mantle component cannot be investigating with such low-resolution step heating, and could likely affect the $^{3}He/^{4}He$ ratio.

Line 66: I disagree here. The isochron method was not used in the paper, otherwise aliquot from same olivine population would have been used. In addition, the equation for the isochron given by Blard and Pik (2008) is not used here. The authors simply plot ^{4}He vs. ^{3}He following equation 5 in the text which correspond to the classic crush/fusion method and called that an isochron. I would either remove isochron method statement, or if you want to assume that the 6 lava flows have same age, and can be used as one population, it should be clearly stated and the proper isochron equation should be used to verify the validity of the method. Please see my major comment above for more detailed discussion on the crush/fusion and isochron dataset.

Line 68: "we developed" should be replaced by "we used" or similar phrase.

Line 91-92: The authors specified that they have additional notes on the samples, such as vegetation cover or average sample depth, but failed to provide those data (or I didn't see them). I would have like to get them in a supplementary material, specially that some detailed sample information could have be beneficial.

Line 102-103: The authors specify in this section that 4 samples are disregarded and 8 samples are selected due to the mm-sized olivine. Could you please specify if no mm-size olivine were found in those 4 lava samples or if the olivine quality was insufficient for proper ^3He_c investigation? It is unclear from the description why those samples are disregarded. In addition, where is sample VM-07? It is supposed to be used in the study (see line 96 and line 103) but I cannot find this data in the text, the tables or the plots.

Table 1: This table could be more interesting if more information regarding the sample notes were included such as the vegetation cover or the topography shielding.

Line 111: The authors specify that the crushing protocol is derivated from Blard et al., 2008. There is no Blard et al., 2008 in the reference list. I found however, Blard and Pik (Chemi. Geol. 2008) and Blard and Farley (EPSL 2008). Please be careful when referencing literature (see comment at line 182 as well). In any case, both Blard and Pik (Chemi. Geol. 2008) or Blard and Farley (EPSL 2008) papers do not contain any indication (or I couldn't find any) for a crushing step at 2min followed by a step at 5min for proper ^3He_c extraction. Could you please provide information where this protocol has been taken? In addition, early crushing steps can potentially release ^3He_c from the matrix (see Blard and Pik (Chemi. Geol. 2008) and Blard and Farley (EPSL 2008) papers for more details). Do you have investigated such ^3He_c loss/contamination on the $^3He/^4He$ crush signal? This could lead to important impact on ^3He_c measurements if not well estimated. It should be at least mentioned here.

Line 121-122: Please add the re-extraction data to the dataset (in the main text or in supplementary). I noticed also that fusion is performed at ~1200ºC for 25min but step heating experiments show un-degassed samples at T~1400ºC for 30min step (last temperature step at table 3). I would recommend therefore to provide all the re-extract dataset to ensure that the fusion and step heating samples have been properly outgassed. At the moment, it seems difficult to fully outgas the samples with one temperature step at ~1200C for 25min. If re-extraction shows significant He, did you then add them to the total? Please provide additional information.

Line 124-125: The authors are using a blank correction for the He analysis with an empty furnace while using Sn foils to wrap their samples. I would have expected blanks to be run with empty Sn-foil packet instead, to better account the blank value. Same comment can be given for step heating experiment (see line 135). Given the blank level is given at 5% (without Sn-foil contribution), I suspect that blank could be underestimated if He outgassing for the Sn foil is not accounted for. In addition, 5% He blank contribution is not insignificant to my opinion.

Line 134-135: Please provide the re-extract data.

Line 157: What means early measurement here?

Line 166: Using equation 5, for sample VM-08, I calculate a ^3He_c at 2.8 Matoms/g instead of 2.58 Matoms provided by the authors. I am using an average $^3He/^4He_{crush}$ at $1.12x10^{-5}$ (from VM-06 and VM-09). I suspect that you are either using VM-06 or VM-09 crush value but without any justification. Therefore, for the sake of consistency, I would recommend to use VM-06 and VM-09 average crush value for all VM samples at the exception of VM-06 and VM-09 where ^3He_c can be properly determined. Otherwise please justify which crush values you are using for all VM analyzed samples.

Table 2: For clarity purpose, I would add a special label for the two samples that have coupled crush/fusion data. The other could be labelled as uncrushed olivine.

Fig. 2: Please give the linear regression value in the plot. This is important information and should be display clearly in the plot, not in the caption. Also, as mentioned in my major comments above, the data plotted here is not representing the data in table 2. Please check why this is not the same as this might lead to different ^3He_c given by this regression line based from equation 5.

Line 182: Thirumalai et al., (2011) and York et al., (2004) are not listed in the reference section. Please be sure all the references are included.

Fig. 3: The red solid line for ^3He_c is likely to be wrong. The calculated ^3He_c from equation 5 using VM-1 data at T~800ºC lead to ^3He_c calculated at 3.1 Matoms/g (which represent 92% of the total ^3He_c for VM-01), but in the figure, the red solid line is showing a value >5 Matoms/g. The black solid line seems ok, but could you please check if the plot has the proper values calculated for ^3He_c? In addition, the last temperature step (T~1400ºC) is very likely not fully outgassed. The signal is still showing high He content. If you have the re-extraction, please provide them to ensure that total outgassing of the sample is performed.

Lines 240-241: I think the argument for more detailed step heating is important here. Knowing that early temperature step can contain important information, especially for $^4He^*$ contribution that can lead to a lower Ra value at low T-step (see Kurz 186a), or if we suspect some ^{3}He/^{4}He lower mantle contribution from the tiny inclusions. I would have therefore, expected a better resolution for the step heating experiments. The SFT is capable of analyzing significantly lower values of ^{3}He, much lower than ~0.4 pcc/g (^{3}He concentration given at T~800ºC), especially knowing that blanks value is given at ~0.8 fcc, and therefore, in the worst-case scenario, acceptable values for ^{3}He could be potentially measured as low as 0.008 pcc (~10 times the blank). However, as a theoretical example, if a lower temperature step (T~500ºC) is performed on sample VM-01, and if ^{3}He signal is measured 100 times lower than the one at T~800ºC (i.e. ~0.07 Matoms), therefore the blank contribution given at 0.002 Matoms should "only" represent ~3% of the signal, which is quite acceptable. ^{4}He signal, on the other hand is more complicated and could lead to some limitation under the current analytical blank of the double wall furnace. The ^{4}He blank is given at 0.2 ncc which limit the measured signal at ~2 ncc of ^{4}He (~10 times the blank to ensure sufficient precision). It is noteworthy that signal lower than 10 times the blank can be measure but then larger error is expected and could limit the interpretation. Nevertheless, ^{4}He signal 10 times lower than the measured ones at T~800ºC (i.e. 4.3x10^4 Matoms) could still be measured for a hypothetical step at T~500ºC. Blank error could account for ~12% (5380 Matoms). I would like, however, to point out that the double-wall furnace (where step heating experiment is performed) is not baked but solely pumped out overnight (see lines 120-121), and ^{4}He blank given by the authors are quite high compared to some double wall furnace blanks given by other studies (Blard et al., 2015; Kurz, 1986; Williams et al., 2005, Yokochi et al., 2005, Zimmermann et al., 2012; 2018, Zimmermann and Marty, 2014), where blank values are given as low as ~300-600 Matoms. If we assume that blank values can be reduced significantly in the range of 0.02 ncc (~600 Matoms), then previous blank contribution of ~12% (for a hypothetical T~500ºC step) will drop at ~1.5%, and ^{4}He concentration 100 times lower could be even analyzed. In addition, peak jumping analyses can also be performed to measure ^{4}He on the CDD to keep good ^{4}He measurement precision for very low signals. Consequently, I think the authors could safely performed a more detailed step heating experiment with limited loss of the analytical precision given by the SFT capacity and/or a better baking/cleaning/analytical protocol for ^{4}He analysis.

Line 245-247: Why the authors are using the crush value here, while they have the mantle component determined with the highest temperature (T~1400ºC, where no cosmogenic contribution is estimated, see line 249)? I would rather use the high temperature ^{3}He/^{4}He ratio for the mantle composition instead of using a crush value from another lava flow (VM-06, VM-09 or average). My understanding is that no crush step is needed when step heating experiment is performed, as all the information are retrieved from the step heating (i.e., ^3He_c and $^3He/^4He_{mantle}$). Please justify the use of a crush value instead of the high-T value for mantle component.

Table 4 and Table 5: I found those two tables redundant. They show almost similar information, only isochron dataset is added to table 4. I would merge those two tables altogether in one clear table with the concentration and their associated calculated ages for all the methods used. It would be easier to have a table summarizing everything instead of two.

Reference:

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