Letter to the editor

Dear Marissa Tremblay,

We are pleased to have received such constructive reviews to our manuscript. We also appreciate your perspective regarding this work to represent a valuable contribution, and your compliments to our writing style and figures. We have made all the suggested minor suggested structural changes, as well as minor content additions, modifications, and clarifications.

In your report, you requested clarification in our response to reviewer 3 (Mark Kurz) regarding compositional-controlled ^{3}He production rates. We apologize for the confusion raised (likely due to an eagerness to reply in depth about possible future research field), and we clarify this here.

We do not have data about the compositional effects of pyroxenes to $^3He_{cos}$ production rates, nor we are saving any data for a future paper. In our study, we only analysed mixtures of clino- and orthopyroxenes. There is no conclusive evidence in the literature that these different minerals (nor olivines vs pyroxenes) have significant differences in their $^3He_{cos}$ concentration (see full explanation and citations in answer to reviewer 3). We still have (not analysed) sample leftovers, which could be potentially used for future research (although none of our sampled sites would be suitable for production rate calibrations, and we have no funding left for these analysis).

Regarding the rest of the comments and suggestions made by the reviewers, we feel that they have contributed to a better manuscript in terms of the content (e.g. provide a more thorough discussion about the potential effects of poorly constrained magmatic ratios in our results, have more consistency throughout the introduction, results, and discussion, etc.) and the fluency, improved by adjusting the manuscript structure to that suggested by reviewer 1 (Eric Portenga) and supported in your report.

We are pleased to provide with a point-by-point answer to reviewers (as well a to the editor's report and a list of modifications done in the new version of the manuscript). These can be accessed through the hyperlinks provided here:

Reviewer 1
Reviewer 2
Reviewer 3
Associate editor
List of significant modifications
Minor modifications

For improved clarity, see author comments marked by **AC:** and developed in *italic*

Sincerely

Pedro Doll and coauthors

Note: we apologize that the track changes version on our resubmission does not include figures, as we had problems with the document compilation. Hence, captions are somewhat funny in the track changes version. We appreciate the understanding.

Reviewer 1 (Eric Portenga)

Review of Doll et al., submitted to Geochronology.

In their submission, "Cosmogenic ^{3}He chronology of postglacial lava flows at Mt. Ruapehu, New Zealand," Doll et al. present a vast new dataset of cosmogenic ^{3}He ages of olivine and pyroxene minerals isolated from 23 lava flows circling the summit of New Zealand's Ruapehu volcano, which belong to 8 previously described members of at least 2 formations. The authors use ^{3}He exposure-age dating because it has greater precision on lava flow ages than $^{40}Ar/^{39}Ar$ methods, which were previously used to broadly constrain Ruapehu's lava eruption history since 20 ka. Six of the eight members erupted in the post-glacial late-Pleistocene and Holocene, so constraining Ruapehu's eruption history through these time periods is important to assess volcanic activity and hazards associated with eruptions.

The study is well organized and detailed and it was an enjoyable read. The authors come to several conclusions and many refinements to Ruapehu's eruption history and the lithological correlations of disparate lava flows as they are currently known. Overall, I find the Scientific Significance and Scientific Quality to be Excellent and the authors are commended for putting together such a comprehensive and detailed report! However, I believe the Presentation Quality needs some minor reorganization, particularly with regards to repetition of content and narrative presented for each lava flow or Member, which currently feels very disconnected across sections.

I find it really wonderful (maybe it was required, regardless) that the researchers consulted with local Iwi and that this is clearly established at the beginning of the Sample Collection Section. These details may not be necessary to the science presented, but they are necessary to demonstrate proper means of doing geological research in landscapes with Indigenous significance. It is an example we should all strive to follow.

The organization of the remainder of my review follows the list of aspects Referees are asked to consider:

AC: *We appreciate the constructive comments and the compliments, and the general manuscript reorganization suggestion (in particular, results and discussion section). This is an aspect where I (the main author) struggled with at the time of writing, and we agree with the suggestion that a more "lava flow led" presentation (sampled lava flows, previous work, results and short discussion for each site) would help with the narrative, so we changed the structure of the paper as suggested, similarly to how the referee proposed in Item 10).*

1. Does the paper address relevant scientific questions within the scope of GChron?

Yes. The paper focuses on a suite of ^{3}He ages that facilitate mean ages of 23 lava flows to be determined. The ages of these flows are inspected both through the lens of existing $^{40}Ar/^{39}Ar$ and paleomagnetic ages of the lava flows. Interpreting new geochronological constraints of lava flows in the context of existing ages determined by other means, for an active and prominent volcano in New Zealand makes this paper relevant for Geochronology. Given the deadly 2019 eruption of Whakaari, reassessments of stratovolcano eruption histories for New Zealand also seems timely.

AC: *We are pleased to read this.*

2. Does the paper present novel concepts, ideas, tools, or data?

Yes. ^{3}He is used because of the availability of olivine and pyroxene in the basaltic andesite to andesitic lava flows found at Ruapehu. ^{3}He ages also provide tighter constraints on lava flow ages than those presently available in the literature; existing ages identify many young flows (<20 kyr), but the authors clearly state and support the need for reanalysis with new dating methods because of the poor accuracy and precision of ^{40}Ar/^{39}Ar ages.
One question on this topic kept coming to mind: How novel is ^{3}He dating on olivines and pyroxenes for stratovolcanoes? The wording in paragraph 2 of the Introduction implies that ^{3}He dating of olivines and pyroxenes for stratovolcanoes is novel, or at least not frequently used, but support from the literature about the scarcity of ^{3}He data would be helpful for readers to understand better the novelty of this approach. In my own quick search of existing literature, it seems like ^{3}He dating has been used on lava flows before, but mainly basaltic lava flows from hot spot or extensional settings, in which case it seems this paper's application of ^{3}He on andesitic lava flows for a stratovolcano is novel.
AC: *As the referee pointed, ^{3}He dating has not been frequently used on andesitic stratovolcanoes. We emphasized this and added supporting literature in the introduction section, as well as strengthened our results implications in Section 5.4.*
Introduction (lines 64-68): "Thus, ^{3}Hecos can be used to complement chronological studies by providing greater detail on recent construction histories of volcanic edifices (e.g., Kurz et al., 1990; Foeken et al., 2009; Espanon et al., 2014; Parmelee et al., 2015). However, most of this research is focused on basaltic lava flows in extensional environments (e.g., Kurz et al., 1990; Licciardi et al., 2007; Foeken et al., 2009; Espanon et al., 2014; Marchetti et al., 2014; Medynski et al., 2015), and the application of ^{3}Hecos on stratovolcanoes (e.g., Parmelee et al., 2015) is still limited."

3. Are substantial conclusions reached?

Yes. Through their analysis, the Authors identify two periods of heightened volcanic activity at Ruapehu 17–12 ka and 9–7.5 ka. The accuracy of the ^{3}He dating methods for young lava flows also allowed the Authors to propose several reorganizations and associations of lava flows around Ruapehu between the Whakapapa and Mangawhero Formations. Overall, the Authors demonstrate, conclusively, that using ^{3}He exposure-age dating can greatly improve our knowledge of effusive eruption histories of stratovolcanoes.
Building on the novelty of ^{3}He dating for andesitic lava flows, I think the Authors could be stronger with their language about how effective this method can be for dating volcanoes elsewhere or at least constraining better the timing of young eruptions.
AC: *Comment addressed as per Item 2.*

4. Are the scientific methods and assumptions valid and clearly outlined?

Yes. The Authors do a great job in the Methods section describing the ^{3}He dating method and all aspects, assumptions, and data required to fully interpret their samples as lava flow exposure ages. The Authors also do a great job in outlining the different analyses that are done on samples from each site.
However, the paper presents new ages for 23 lava flows belonging to eight different Members of two (possibly three) different Formations. It is a lot for the reader to keep track of. With regards to Methods and Assumptions, there is a significant amount of field site/lava flow and sample selection details that ends up in the Results section:

- Tawhainui Flows: Lines 286-287, 290-291
- Mangatoetoenui Flows: Lines 300-301
- Taranaki Falls Flows: Line 316
- Saddle Cone Member: Lines 324-325, 327-328
- Rangataua Member: Lines 340-342
- Paretetaitonga Member: Line 350
- Turoa Member: Lines 356-357, 362-364
- Makotuku Member: Lines 375-377, 384-385
- Mangahuehu Member: Lines 387-391

AC: *Comment addressed as per Item 10.*

5. Are the results sufficient to support the interpretations and conclusions?

Yes. The Authors make it very clear what data are used to produce each sample's exposure age and how individual sample exposure ages are averaged to produce new exposure ages for each dated lava flow. The Authors have clearly considered the strength of each lava flow's ^{3}He exposure age based on the number of ages comprising the mean age, and the Authors present and support each lava flow's new exposure age as a mean age or a minimum age, which guides the Discussion of lava flows, consistency of new ^{3}He ages with existing $^{40}Ar/^{39}Ar$ and paleomagnetic ages, and proposed revisions to the eruption chronology of Ruapehu.

AC: *We are pleased to read this.*

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Yes. The Methods are very clearly outlined and detailed with all relevant equations and data provided for recalculation and reproducibility of new exposure ages presented in this study.

AC: *We are pleased to read this. However, we added additional crushed analyses that improved the robustness of our magmatic corrections, adding an extra subsection in the methodology section, as an answer to concerns raised by reviewers 2 and 3.*

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes. Again, because of the number of individual lava flows dated in this study, though, it was difficult to follow along with the narrative, at times, and remember which lava flow was which. To this end, some of the Background information about each lava flow sampled in this study did not appear until later in Results section, and it would have been helpful to have full descriptions of field sites, including previous work, earlier in the paper:

- Tawhainui Flows: Lines 284-286
- Mangatoetoenui Flows: Lines 296, 299-300, 304-305
- Taranaki Falls Flows: Lines 314-316
- Saddle Cone Member: Lines 319-322
- Pinnacle Ridge Member: Lines 332-333
- Rangataua Member: Lines 336-340
- Paretetaitonga Member: Lines 349-350

- Turoa Member: Lines 354-359
- Mangaehuehu Member: Lines 388-389

AC: *Comment addressed as per Item 10.*

8. Does the title clearly reflect the contents of the paper?

Yes.

9. Does the abstract provide a concise and complete summary?

Yes. Line 11 suggests 4 lava flows dated with $^{40}Ar/^{39}Ar$ methods were revisited, but according to Table 1, it seems there are 5 lava flows previously dated with $^{40}Ar/^{39}Ar$:

1. Mangatoetoenui Flows
2. Paretetaitonga Member
3. Turoa Member
4. Makotuku Member
5. Mangaehuehu Member

AC: *We understand that there might be a confusion between individual lava flows previously dated with $^{40}Ar/^{39}Ar$ (i.e. Delta Corner, Taranaki Falls, Lava Cascade and Tukino Slopes-b) and units (groups of flows, i.e. Formations, Members or submembers) that have one or more lavas dated with $^{40}Ar/^{39}Ar$ as shown in Table 1 (all units of Ruapehu except for the Saddle Cone, Rangataua and Pinnacle Ridge members). Direct comparison between ^{3}He and $^{40}Ar/^{39}Ar$ dates are only done for the individual flows with both types of ages available. This confusion may be caused by a lack of clarity in our manuscript, so we added "individual" before "lava flows" in Line 11.*

10. Is the overall presentation well structured and clear?

The paper as written seems to follow the standard Introduction > Methods > Results > Discussion > Conclusion format, but I am not convinced that this is the best way of structuring analysis, reanalysis, and chronological refinement of 23 lava flows. I personally struggled as I read through the paper to keep each flow organized in my head because background, field methods, results, and interpretation of new data from each flow were scattered throughout the paper. Upon finishing the paper and re-reading, I really wonder if a better format for the paper would be to lump the pertinent background, field methods, results, and interpretation of new data for each lava flow into its own mini-narrative set within a broader narrative of the need for higher-resolution age constraints on effusive eruptions at stratovolcanoes? The current Results section is basically organized in this way. In my personal opinion, I think the Authors might consider the following organization:

1. Introduction — Leave as is; it is great. Use this to set up the challenge of $^{40}Ar/^{39}Ar$ dating young lava flows and why (presumably for eruption hazards reasons) it is important to contrain the ages of young flows.
2. Geological Background — Keep it focused on Ruapehu and broad scale geological setting. Introduce the readers to the various lava flows and names for initial orientation and introduce previous geochronological work that dates Ruapehu lava flows, but keep it broad for now. Reiterate that other methods are available to date young lava flows.
3. Methods — Explain to the reader that specific sample collection for each flow involved its own considerations and focus broadly on what new methods will be applied to all samples collected (i.e. mineral separation, geochemical analyses,

measurement of He isotopes, calculation of cosmogenic ^{3}He, determination of exposure ages).

4. Specific Lava Flow Details — Here, I would go lava flow by lava flow and present all specific and relevant Background information (i.e. exiting chronology, assumptions of correlation between flows), specific field sampling site considerations, results, and interpretation/discussion of results. Constructing individual narratives for each lava flow would really aid the reader in following along with the importance and significance of each new exposure age. Much of what is presented in Sections 5.1 and 5.2 "Consistency with previous age constraints," and "Inconsistencies with previous classification of units," could be brought into individual narratives.
5. Discussion — Return to a broader Discussion about how ^{3}He exposure ages really complement existing ^{40}Ar/^{39}Ar ages and highlight the refinements proposed to unit age and classification. Sections 5.3 and 5.4 were great sections that highlight the importance of the datasets overall in better understanding the recent effusive eruption history at Ruapehu and broader applicability of this seemingly novel ^{3}He dating approach.
6. Conclusions

The Authors should feel free to agree or disagree with this suggestion, but I think tightening the narrative in this, or another way, would take this paper to the next level.
The only other comment about the structure of the paper is this: The first sentence of the Abstract highlights the importance of having a detailed knowledge of the timing of past eruptions for making accurate volcanic hazards assessments, but the Authors never return to this topic beyond the first paragraph of the Introduction. It leaves me wondering what new guidance can be made about effusive volcanic hazards, at least at Ruapehu, now that we have this new and wonderful ^{3}He dataset? Does the clustering of effusive eruptions between 17-12 ka and 9-7.5 ka mean that Ruapehu is in a quiescent state? Are these age clusters related to specific activity within Ruapehu or the broader tectonic setting of the Taupō Volcanic Zone or Tonga-Kermadec subduction zone? The Authors did such a good job of setting up the need for better chronological constraints on effusive eruptions to supplement existing argon-based chronologies, but the broader significance of new findings seen through the lens of volcanic hazards is missing.

AC: *We found this point to be very valuable. We changed the structure of results and discussion. We renamed section 4.3 (section 4.4 in the new version of the manuscript) to "Lava flows: background and new ^{3}He constraints", including a short comparison of results (i.e. whether they match or not previous age constraints). However, larger discussion points are still left for section 5.1.*
We also further developed on the implications of our results in section 5.3.

11. Is the language fluent and precise?

Generally, yes. Previous comments highlight the need for some reorganization to reduce redundancies in content, however.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes. Methods section is great.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
Possibly. See previous comments.

14. Are the number and quality of references appropriate?
Yes. Perhaps missing some literature on other applications of ^{3}He dating or importance of new data in context of hazards associated with past effusive activity of Ruapehu.
Lines 33-45: This section is a good introduction to TCNs, but I think it is lacking in its support from the large existing body of literature. For example, the assumptions of TCN dating should each be supported by references, as should the statement that ^{3}He diffuses through quartz and feldspars and volcanic groundmass.
AC: *In the introduction, we added references on literature from ^{3}He applications, as well as regarding TCN dating methodology and the importance of new data for hazard assessments. Lines 42-53: "The number of TCNs that fulfil these requirements and have well established methodologies developed for Earth science applications is relatively small (see Dunai, 2010), and the production rates and retention efficiency of TCNs vary across different minerals. 3He is a stable isotope with the highest production rate of all TCNs and a low detection limit in several geological settings (Blard, 2021), which makes it the ideal nuclide for dating young lava flows (Gosse and Phillips, 2001). This gas suffers diffusion loss in felsic minerals (e.g. quartz and feldspars, and in volcanic groundmass containing them, Lippolt and Weigel, 1988; Tremblay et al., 2014) at Earth's surface temperatures, and is therefore not normally used for silicic lithologies which are better studied using 10Be or 26Al (e.g., Klein et al., 1986; Nishiizumi et al., 1991; Smith et al., 2005). ^{3}He is more efficiently retained in olivines and pyroxenes (Kurz, 1986a; Gosse and Phillips, 2001; Shuster et al., 2004; Blard, 2021), so it is suitable for dating volcanic eruptions (e.g., Kurz et al., 1990; Foeken et al., 2009; Parmelee et al., 2015), reconstruct glacial histories (e.g., Cerling and Craig, 1994; Blard et al., 2007) and fault kinematics (e.g., Fenton et al., 2001) or estimate erosion rates (e.g., Ferrier et al., 2013; Puchol et al., 2017), considering that the studied rocks contain these minerals."*

15. Is the amount and quality of supplementary material appropriate?
Yes.

Minor Comments:
Line 48: First use of $^{3}He_{cos}$, but at this point the reader does not know there is any other kind, but they will in Section 3.6.1, so it should be defined. Authors could do this in Line 39, "Cosmogenic ^{3}He ($^{3}He_{cos}$) is a stable isotope…"
AC: *Addressed, now in line 61.*

Section 2.1: The authors might find Gabrielson et al. (2018) to be useful: Reflections from an Indigenous Community on Volcanic Event Management, Communications and Resilience (Advances in Volcanology, https://doi.org/10.1007/11157_2016_44)
AC: *Reference added*

Figure 1. I do not see any vents on Panel C despite a symbol being listed in the key.
AC: *Fixed*

Line 126: A reference to Figure 3 seems to be missing/out of order.
AC: *Addressed*

Section 3.6.1: I am not an expert in ^{3}He calculations. The authors have done an excellent job explaining each measurement, its significance, where it comes from, and whether it needs to be addressed or is negligible. Very straightforward and well done.
AC: *Thank you. This section was further developed as requested by reviewers 2 and 3.*

Lines 256-257: Put "ppm U" after measurement values and do the same for "ppm Th" and "ppm Sm." It will just make the sentence easier to process.
AC: *Addressed*

Lines 266-267: The sentence is oddly worded following "…in Table A3," and the Authors should revise for clarification.
AC: *Addressed*

Section 4.3: Already stated above, but some of these sections include a significant amount of field area background information, rather than new Results. Mini-narratives on each flow (as suggested above) can help with this.
AC: *See item 10.*

Table 2: The layout and placement of individual Lava Flow Member mean ages and uncertainties is confusing because the summary age appears to be in the $^3He_{cos}$ column instead of under Exposure Age. It's obvious that the n = value comes from the subsequent rows, so I don't think that is necessary. The table is already long, so perhaps putting the (INT 2 sig) uncertainties in a new row beneath the summary age could help with the alignment of the summary age?
AC: *Thank you for this comment, we modified as requested and changed the placing of the summary ages to below the data of each flow.*

Line 388: Define which ages "These ages…" refers to
AC: *Addressed*

Line 402: Might just say that the Taranaki Falls and the Lava Cascade Flows' ^{3}He ages are not just outside 2σ of Conway et al. (2016) but are older than ^{40}Ar/^{39}Ar ages. Also, it would help to remind readers that the Lava Cascade Flows are part of the Mangatoetoenui Flows of the Iwikau Member of the Whakapapa Formation since "Lava Cascade" is not shown in Table 1.
AC: *Addressed*

Lines 401-402: $^3He_{cos}$ for some reason has a "/" through it here, and only here.
AC: *Addressed, thank you for noticing.*

Line 404: Please define the term "rootless nature" of a lava flow. Volcano readers may be familiar but others may not. It's use a few time throughout the paper suggests it is important for age interpretation or unit correlation, so a definition would be helpful for all readers.

AC: *Addressed, now in second to last paragraph of section 4.4.1. "The Taranaki Falls flow (TFa) is a rootless (not continuous towards the vent it would have been erupted from) elongated lava flow ..."*

Lines 404-405: I am uncertain of the significance of the second half of this sentence (i.e. "as it precedes..."), both in terms of the order of events, rootless nature of the flow, and the geomorphology. Can the Authors please clarify?
AC: *We changed the wording to make this point clearer. In section 5.1: "Additionally, our eruption age would explain the rootless nature of the flow (Townsend et al., 2017), as it is older than the flank collapse event that affected the northern summit area of Ruapehu at ca 10.5 ka (Eaves et al., 2015) and so the upper section of the Taranaki Falls flow (Figure 6a)."*

Line 408: Parentheses needed around "Figure 5"
AC: *Addressed, thanks for noticing*

Lines 408-409, 415, 426-427: These are all statements about inconsistencies and seemingly should be discussed in the next section on Inconsistencies?
AC: *Indeed, but note that this section was directed to compare ages with age constraints, not with how new ages fit in the existing classification. We modified the title of the section (*5.1 Comparison with previous age constraints*) to better reflect the content developed within.*

Lines 453-457: Seems like these two bullet-points could be merged?
AC: *Addressed.*

Lines 459-460: Consider rewording, "Our ^{3}He based eruption ages allow two periods of enhanced effusive activity since the LGM to be indentified on Ruapehu, during which lava emplacement surrounding the summit of Ruapehu occurred nearly simultaneously."
AC: *Addressed*

Section 5.4: This whole section seems to justify the use of ^{3}He on stratovolcanoes. ^{3}He has been used on lava flows before, so is the significance here that it's never been used on stratovolcanoes? Whether ^{3}He has or has not been used on stratovolcanoes elsewhere should be expressed clearly in the Introduction so that the significance of this new dataset and the novelty of this approach at Ruapehu is clear to the reader. Overall, it seems like this section does not really interpret or discuss any of the new data or geospatial relationships between flows of similar age, so I'm not certain that this is necessary here. The first paragraph seems entirely like Background and the second paragraph seems like a Lit Review on Methods; I would consider what can be moved to Intro or Methods. The third paragraph seems relevant for the Discussion, but statements made are disconnected from the new data presented here. I think this could be strengthened by using new data-driven interpretations about the usefulness of $^{3}He_{cos}$ on stratovolcanoes. If $^{3}He_{cos}$ hasn't been used on stratovolcanoes before, this certainly is a great study to make the case for its broader use, but it doesn't seem the authors take full advantage of this opportunity.
AC: *We changed some parts of this section towards the introduction, and developed on how our results improved the chronology of Ruapehu, and that this example showcases how this method can be used in stratovolcanoes globally.*

Line 498: The two age clusters of 17–12 ka and 9-7.5 ka are mentioned here, in the Abstract, and in the first sentence of Section 5.3, but I still do not have a clear sense of the geological significance of these clusters beyond the fact that new ages identify two clusters of activity. If there is anything more the Authors want readers to take away from these two clusters, I think that needs to be more evident in the Discussion.
AC: *We added possible implications of these findings in Section 5.3.*
Citation: https://doi.org/10.5194/egusphere-2024-163-RC1

Reviewer 2 (David Marchetti)

I reviewed the pre-print manuscript "Cosmogenic 3He chronology of postglacial lava flows at Mt Ruapehu, New Zealand" by Pedro Doll and others. I generally find it to be in good shape, and would consider this needing only minor revisions. I applaud the authors for a putting so many ages into one paper! I'll answer the GChron specific reviewer rubric questions below and then offer broad comments first, shorter thoughts about particular points second, and then line by line comments and edits below that.

AC: *We appreciate the reviewer's constructive comments and the compliments.*

1. Does the paper address relevant scientific questions within the scope of GChron?

Yes, certainly does.

2. Does the paper present novel concepts, ideas, tools, or data?

I'll say Yes, but I would just say that the use of 'novel' in the question is a bit anachronistic. The manuscript has a lot of data (exposure ages). I applaud the authors for using all of it in a single paper.

AC: *We appreciate this. It was indeed a huge effort.*

3. Are substantial conclusions reached?

Yes. And they are well supported.

AC: *This is pleasant to read.*

4. Are the scientific methods and assumptions valid and clearly outlined?

For the most part, I'll mention one possible methodological problem below.

AC: *We were happy to address this concern in depth.*

5. Are the results sufficient to support the interpretations and conclusions?

Yes. I think the conclusions are well supported and uncertainties are realistic.

AC: *This is pleasant to read.*

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Yes. For the most part. See main comment.

AC: *We were happy to address this concern in depth.*

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

In most cases, yes, but given the wide background of work on the 3He method and myriad application papers, some readers may feel that a few instances are lacking more citation. For example, on the use of the crush or magmatic correction and how to best perform a crush.

AC: *We developed further in the methodology section.*

8. Does the title clearly reflect the contents of the paper?

Yes, dead on.

AC: *Thank you. We added the Māori name of New Zealand in the new version, to support the use of it, which has been promoted by New Zealand's government and society.*

9. Does the abstract provide a concise and complete summary?

Yes, fine abstract, however I have some edits given below to perhaps make it read better.

AC: *We appreciate the in-detail comments and notes.*

10. Is the overall presentation well structured and clear?

I think so, but it's a lot of data (which is a good thing) and keeping track of the various flows is difficult, not sure how to fix.

AC: *We addressed this by following a suggestion made by reviewer 1 (Item 10).*

11. Is the language fluent and precise?

In most places, line by line edits below to help clarify and smooth the reading.

AC: *We appreciate the detailed comments and notes.*

12, Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?
Yes.
AC: *We appreciate the in-detail comments and notes.*
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
No, not really. Added too in a few places perhaps.
AC: *We developed further the methodology section, in particular regarding magmatic ratio calculations.*
14. Are the number and quality of references appropriate?
Yes, for the most part.
AC: *Thank you.*
15. Is the amount and quality of supplementary material appropriate?
Yes. But I would like a little more info on the magmatic correction that I describe below.
AC: *We are happy to develop in depth.*

Overarching Issue

My main overarching scientific/methodological concern is with the magmatic correction used to generate the cosmogenic 3He values which are then used to determine the exposure ages. The authors did not analyze (crush) any of their samples for magmatic He ratios but rather used previously published 3He/4He ratio data from Patterson et al 1994 (GCA v 58). It should be noted that those data (Patterson et al., 1994) were from fused (heated) mineral separates and not from crushes either. I think the assumption is that since those 1994 samples were not collected for cosmogenic exposure dating (that paper says they are from flow interiors from road cuts, but the source for all samples isn't clear) that those samples will approximate the non-cosmogenic 3He/4He abundances, and for young flows with minimal nucleogenic 3He and radiogenic 4He, approximate the magmatic correction. Probably a good assumption.

The tight exposure age clusters for most flows as well as the low % of non-cosmogenic 3He, extremely low 4He total values (some have zero 4He, which is interesting), indicate that determining the 3Hecosmo amounts this way is fine. However, this all needs more explanation. For example, where did the $7.5 \pm 1.5 \times 10^{-6}$ 3He/4He magmatic ratio come from? Looking at Patterson et al 1994, I can get close to that number (7.58 or 7.6) from averaging the three Waimarino data (they are replicates of the same sample) measured in olivine, and then averaging the Waimarino and Ohakune data (Ohakune from pyroxene – just 1 sample). Ultimately the magmatic ratio used in this (2024) manuscript, which is applying 3He exposure age dating to almost exclusively pyroxenes (save for one olivine sample), is based on the 3He/4He ratio from 1 pyroxene sample averaged with olivines that are really gassy, measured from furnace heating 30+ years ago from rocks *likely* to not have experienced any cosmogenic exposure? Yes, more explanation please! Perhaps as much as was given to the nucleogenic 3He component which was found to be insignificant.

As I said at the beginning of the last paragraph – this is likely ok, and even really cranking on the magmatic values only changes the resulting 3Hec very slightly and well within the 1sigma uncertainty of the 3Hec values; but when 3Hec and exposure ages of one sample are different from others from the same flow it might be worth mentioning that the largely unconstrained 3He/4He magmatic ratio correction could be a factor. Sometimes there is a 'hot', high ratio inclusion that gets released during heating. A simple sensitivity test of the possible effect of having a gassy inclusion with a really high 3He/4He ratio would help. Would only take a few sentences to explain this all. Could just look up the range of 3He/4He magmatic ratios from subduction zone volcanism and extend beyond the stdev used in the 7.5 ± 1.5 E-6 ratio.

A He isotope data presentation suggestion; this could be me being pedantic, but I much prefer 3He/4He ratios to be given relative to air (R/Rair) as well as the true ratio.
Circling back around to the really low 4He total values. That is really interesting, they're all really low. Those are lower than similar age basalts in many cases? Might be worth mentioning this and suggesting that there is essentially no magmatic gas in almost every sample (except for MA samples – with older crystallization ages; but why not NR samples of the same exposure duration?). In almost every case those pyroxenes (perhaps the key, often less gassy than olivine, certainly so in the Patterson et al data and many other papers that measure both) didn't pick up but a tiny wiff of magmatic gas. Sorry to go on but the 3He/4He fusion ratios relative to air (Rair) are in the hundreds to thousands for samples with closure ages of like 10 ka. Just really cool.
AC: *We appreciate that reviewer 2 brought this point up, but at the time clearly stated that the assumptions made were probably correct but not properly explained.*
PH Blard performed in-vacuo crushed He analyses of three samples. We used this new data (provided as supplementary material) and the data from Patterson et al. (1994) to obtain a weighted average of a magmatic ratio. We explain this in a new section (Section 4.2) and why the (still) largely unconstrained magmatic ratio does not affect our results. We also added the sensitivity test the referee suggested. We modified the presentation of the magmatic ratio values to Ra values.

Secondary Points

- Figure 2 could use at least two more photos. I would actually like to see a more than 4 in the text if allowed by editors? If not allowed, then a bunch more in the appendices maybe? 3He dating is a great tool for determining the timing of young volcanism, showing readers what to sample from andesitic flows is important, and more examples are better.

AC: *We agree, and we extended this figure to include a total of 6 examples, not only from Aa surfaces but from the spatter deposit and the blocky flow sampled. Additionally, we provide a new supplementary file with photos showing a variety of surfaces sampled, samples which resulted in outliers and examples of surfaces that we considered not suitable for sampling.*

- You mention Figure 4 before first mention of Figure 3 – I think.

AC: *Fixed*

- I think Figure 3 is really valuable. It's great to see a complete understanding of the minerals used in the analyses. In that light I think Figure A1 should be moved out of the appendices and into the text. It won't take up too much room and helps show a deep understanding of the minerals and rocks.

AC: *We added Figure A1 as Figure 3a*

- At the first mention of the R correction factor I think the authors should briefly explain WHY the R correction factor is essential when determining 3He c values using the magmatic correction.

AC: *Addressed*

- For Figure 3. It might be interesting to determine the partition coefficients for these trace elements in the rock/mineral pairs. Could compare to https://kdd.earthref.org/KdD/

AC: *We now present these results at the end of Section 4.1. "These values indicate (maximum) partition coefficients (Kd) of 0.006–0.085 for U; 0.006–0.080 for Th and 0.15–0.74 for Sm in pyroxenes and 0.045 for U; 0.045 for Th and 0.11 for Sm in olivines. Obtained maximum Kd values in pyroxenes, in general, agree with values from the literature (Dostal et al., 1983; Luhr and Carmichael, 1980; Gallahan and Nielsen, 1992; Nicholls and Harris, 1980). Our (maximum) Kd of U and Th in olivines are similar to those reported by Dunn and Sen (1994) and Villemant (1988), while the Sm Kd in our olivines result in a value an order of magnitude larger than that of Dunn and Sen (1994), which can be explained by the impact of fluid inclusions with higher Sm contents within the olivine crystals."*

- Use of *ca* This is just me, but in geochronology papers I read ca to mean circa annum or 'about in time'. I know it can also just mean circa or "about" (and this partially stems from the abbreviation being either *c* or *ca*), but it's off putting for me to see it used relative to distances, like with 'ca 10 km', in the same way as dates. This is because when I see the latin 'a' in a geochronology paper, I read it as annum or year. As a reader I would prefer to see ca just reserved for dates, as a reviewer I understand that usage varies and it's probably ok as is. If you want to take my route here then the tilde ~ , or "approximately" works great for approximate distances, as used in the manuscript in a few places.

AC: *Addressed, we appreciated this comment*

- In Table 2, perhaps another digit in the latitude and longitude data. The elevations are down to the decimeter while the lat/long--which are usually much more precise that z--are coarser?

AC: *Added*

- Also in Table 2, I don't like the averaged eruption age being above the data it comes from. I know you may not want to use another line for the average age below the data, as you have a line above the data for the name of the flow, but I think it would be better.

AC: *Addressed. This, however, together with the alignment correction requested by reviewer 1, extends the length of the table from three to four pages.*

- Figure 5 could be improved a bit. It just looks a little odd. Perhaps just shorten it vertically? So many long lines require moving your head up and down to try and figure it out. Or perhaps better...move part b to a different spot and then extend horizontally and shorten vertically?

AC: *Addressed, we shortened it vertically.*

- Not sure what this journal wants but some journals want ka to only be used for a determined age, and kyr to only be used for a time duration between two events or ages.

AC: *Indeed. We modified a few abbreviations.*

- Table A2, would prefer 'minerals' to 'crystals'

AC: *Addressed*

- Table A3, the significant figures for the thicknesses seem unrealistic at a tenth of a millimeter? I know you were very precise and used calipers etc. but still, the surface of an aa flow top just can't realistically be measured to that precise of a thickness across the whole thing.

AC: *This is true, and we fixed this.*

- Plural possessive apostrophes in many places, like "minerals' surfaces". This may be grammatically and stylistically correct but its off putting to me a bit. Could just rewrite most of those sentences so that it's not needed.

AC: *We modified the wording to the best of our capabilities to make this easier for the reader.*

- Table A3 could have clearer explanations for the headings. And I'm not sure I follow the P3 to He nuc argument? The first part of TA3 has a 10^2 in the Pnuc column, the second part of the same table does not? This needs to be fixed and clarified.

AC: *We fixed a column placement issue and a column heading issue in Table A3.*

Line by line edits

3 - 'effusive' is awkward here, could rewrite
14 – maybe change 'dates' to chronology?
20 – maybe change to: "and social fabric and livelihoods of local communities"
43 – this line is a bit awkward, could rewrite for clarity.
67 – 'is formed by' confuses the relationship, the TVZ has andesitic stratovolcanoes in it, but did those stratovolcanoes form the TVZ? – The TVZ is an idea or grouping of stuff?
69 – replace 'which has experienced' with "with"
70 – I would list Tongariro before Ruapehu as you are describing N to S and Rup. before Tong. messes that up.
73 – delete 'have been'
149 – not a big deal but I prefer concentration before the acid or chemical name, so 5% HF
153 – were the final impurity removals done visually?
156 – would be good to mention how the samples were introduced into the ICP-OES and ICP MS. They were probably dissolved and then introduced?
168 - 'condensed' ….I'm not sure He would condense at that T but maybe under low P? Maybe "focused" would be better. And 'cryogenic' and 'cold' seem redundant next to each other.
AC: *We do not prefer this, so we specified the pressure under which the cryogenic trap worked. "After these two steps, He was condensed using a cryogenic trap at 12 K under ultra-low pressure (0.5–1 x 10^{-8} mbar), and then released at 75 K towards the mass spectrometer that measured, in static mode, ^{3}He and ^{4}He."*

173 – was the hot blank done prior to each analysis, or the days analyses? Could clarify.
AC: *Clarified. " HESJ gas standards (20.63 R/Ra; Matsuda et al., 2002, Ra: atmospheric ^{3}He/^{4}He ratio of 1.39, Lupton and Evans 2013) were measured daily with a reproducibility of 4.7% and ^{3}He and ^{4}He values were also routinely compared with CRONUS-P standards (Blard et al., 2015; Schaefer et al., 2016, reproducibility of 5.0%)."*

181 – delete 'emitted'
186 – 'was' should become "were"
222 – could LSD be represented as LSDn, is it the neutron flux monitor based routine? Because LSDn sure seems like a better 'framework' than a LSD one….
AC: *We acknowledge that the CREp calculator uses the Lifton et a. (2014) approach of "take into account specific cross sections for each particle, nuclide and target element" (Martin et al, 2017).*

However, LSDn is not formerly described in Martin et al (2017) nor mentioned in the CREp website, and the mention of a geomagnetic database with a different name than that of the CREp might raise confusion.

232 – 'fully described'…I seem to remember something about that age calculator having self-described terrible documentation….
AC: *We changed to "…calculator, described…"*

383 - need to fix the superscripts for Ar
329 - the 'rubbly nature of the flow' argument isn't very convincing
AC: *We modified the wording to: "However, the uneroded aspect of the flow's surface observed in the field during this study suggests that it could be younger than previously interpreted".*

402 - errant / after He
461 - i'd change 'affected' to "occurred in"
481 - i'd change 'comprise' to "include"
496 - i'd add "significantly" before affect
499 - perhaps delete 'We have demonstrated how' and just capitalize "Cosmogenic" to start the sentence.
594 - in the references you have J twice (don't think its JJ) and then Phillips twice?

AC: *We agreed with the rest of the comments of reviewer 2, and modified our manuscript accordingly.*

Citation: https://doi.org/10.5194/egusphere-2024-163-RC2

Reviewer 3 (Mark Kurz)

This is an interesting paper that presents important new cosmogenic 3He measurements from lava flows at Mt. Ruapehu, New Zealand. It will be of interest to isotope geochemists, volcanologists, and also the wider community interested in volcanic hazards, particularly because dating of young lava flows is so difficult. The number of measurements presented here is impressive, as is the number of samples per lava flow, so this is a prodigious effort. The new 3He age determinations are compared to previous age determinations, using other methods, which is rare in the literature and therefore quite useful.

Overall, this paper significantly improves the eruption chronology of this important volcano, nicely demonstrates the utility of cosmogenic 3He in dating lava flows, and should definitely be published. I do have some suggestions that I think would improve the manuscript if addressed:

AC: *We are very happy to read such positive reviews.*

- It is implied, throughout the paper, that the main geochronological tools for determining lava flow ages are Ar-Ar and K-Ar, as the "most conventional" methods. This ignores the important work that has been done by radiocarbon dating of charcoal beneath lava flows. This is not necessarily a common method, because it takes a lot of effort to find charcoal underneath lava flows, which is scarce in some environments. However, it is probably one of the best methods for lava flows in the age range discussed here. This method has been widely used in Hawaiian volcanoes, and was used in several of the papers cited (Trusdell, 1995; Sherrod et al., 2006). As it is written, one would get the impression that radiocarbon is not a viable method, which I think is misleading. I recommend that the authors at least mention radiocarbon dating, even briefly, in the text. Are there any lava flow radiocarbon dates from this region, aside from the sediments that are mentioned, and did the authors consider it? Is there some reason that charcoal is not plausible here, perhaps the high altitudes?

AC: *This is an excellent point that I (the main author) had missed. In the new version, we mentioned this technique and provided references, and indicated that radiocarbon is normally not applicable at high elevations or at periglacial areas, which leads to the following statements of the introduction regarding alternative methods.*

Introduction: "If available, radiocarbon dating of burned coal beneath lava flows can provide accurate eruption ages, and has been used widely in Hawai'i (e.g., Buchanan-Banks et al., 1989; Trusdell, 1995, see also Lockwood and Lipman, 1980), as well as in various volcanic regions (e.g., Moore and Rubin, 1991; Mishra et al., 2019; Sherrod et al., 2006). However, the use of radiocarbon is limited to areas with sufficient vegetation at the time of lava flow emplacement, so it is not applicable at high elevations or in periglacial environments. Alternative methods such as palaeomagnetism or cosmogenic nuclides exposure dating can support radiometric studies in non-vegetated areas and considerably reduce $^{40}Ar/^{39}Ar$ and K/Ar uncertainties for late Pleistocene and Holocene products (Sherrod et al., 2006; Parmelee et al., 2015; Wright et al., 2015; Greve et al., 2016), and are therefore important to generating more accurate eruptive histories in a wider spectrum of volcanic environments."

-In the section on calculation of cosmogenic 3He (pages 9-10), the discussion could be clearer on one important point. The samples analyzed here all come from fairly high elevations (> 1500 meters), so cosmogenic 3He is dominant compared to magmatic helium, for this suite of samples (i.e. this age range at this elevation). Therefore, magmatic 3He/4He is a small contribution to uncertainties. For this reason, the authors did not crush the samples in vacuum as a means of estimating magmatic helium contributions, they simply used previous measurements of the magmatic 3He/4He. I think this is reasonable, in this case, but would not necessarily work at lower elevations, or for younger lava flows. I recommend that the authors state this more clearly, so that future researchers are not led astray. One way to illustrate this would be to just point out that the magmatic 3He/4He assumed is ~ 5.4 Ra, and the measured (total fusion) values obtained here are > 200 Ra. It would also help if the measured 3He/4He values were given in table 2, and perhaps give the range of 3He/4He mentioned in the text.

AC: *This comment builds up on the "overarching issue" raised by reviewer 2. We appreciate this point being brought up.*
PH Blard performed in-vacuo crushed He analyses of three samples. We used this new data (provided as supplementary material) and the data from Patterson et al. (1994) to obtain a weighted average of a magmatic ratio. We explain this in a new section (Section 4.2) and why the (still) largely unconstrained magmatic ratio does not affect our results in section 4.3, with a new sensitivity test. We added the measured $^{3}He/^{4}He$ values in Table 2 and the range in the text.

-In the methods section, the authors should state clearly what was used as running standard, and how reproducible they were, as well as values and reproducibility for secondary standards (HESJ and CRONUS-P).

AC: *We added this in section 3.5. "HESJ gas standards (20.63 R/Ra; Matsuda et al., 2002, Ra: atmospheric $^{3}He/^{4}He$ ratio of 1.39, Lupton and Evans 2013) were measured daily with a reproducibility of 4.7% and ^{3}He and ^{4}He values were also routinely compared with CRONUS-P standards (Blard et al., 2015; Schaefer et al., 2016, reproducibility of 5.0%)."*

Figure 1. Is the geological map and age assignments based on the new data presented here, or the previous work?

AC: *We clarified this within the figure's label panel.*

Table 1. This table should make clearer what ages have been revised with the new 3He ages. It is difficult to compare the new ages with the previous constraints, so the table is confusing as it stands. How about a separate column for 3He ages?

AC: *We added which samples were re-dated and our results in Table 1.*

Figure 2 and text on Page 7. This figure shows two examples of sampled sites. I suggest providing the sample numbers in the caption. Are these typical? It would be helpful if there was more discussion of the features that were sampled. This is mentioned in the text but only in a general sense. I suggest providing photos of the "representative" range of features that were sampled, in the supplemental information. In the cases where there is good agreement for one flow, were all the features sampled similar? Do sampling site characteristics contribute to uncertainties? What can be learned from the large number of samples collected?

AC: *We agree that further discussion regarding what can be learned from the large number of samples would be beneficial. We added an extra four photos in Figure 2, and supply further in the supplementary material (S1), together with a text describing the range of samples and some thoughts regarding learnt lessons.*

Page 8. How big are the topographic corrections?

AC: *Available in Table 2*

Line 170: replace "R/Ra = 20.63" with "3He/4He = 20.63 R/Ra". R/Ra is the unit, not the parameter measured.

AC: *Addressed*

Page 11 and Figure 3. The trace element measurements (Th, U, Sm) in the minerals were determined by ICP methods, which means that some amount of mineral separate was dissolved. This should be stated and more details provided (how much mineral and what was the procedure?). A tiny amount of host rock contamination within the mineral separates, either on the surfaces or as melt inclusions, would dramatically increase the measured concentrations. I therefore suspect that the measured mineral concentrations are actually maximum values. This should be discussed in the text.

AC: *This is a very good point, we added more detail on the methodology of the ICP prep. In section 4.1, we indicated that trace elements values measured in the minerals represent maximum due to the possibility of groundmass and/or melt inclusion contamination that may be not accounted for at the time of measurement.*

Methods: "Each analysed sample consisted of 1 g of powdered rock/minerals that were fused at 980 °C for 60 minutes in Pt crucibles together with ultra pure LiBO2 in a 1:3 ratio prior to glass dissolution and measurements. The complete procedure is described in detail in Carignan et al. (2001)."

Results: "However, element concentrations provided for minerals represent maximum values, as there is a possibility of groundmass and/or melt inclusion contamination that may be not accounted for at the time of measurement."

Figure 3. How do the pyroxene compositions presented here, and in the table, compare to compositions of previous samples used for 3He determinations and production rate determinations, in the literature? Is there any impact of composition on 3He production rates?

AC: *Our original answer in the discussion forum raised confusion with the associate editor, so we will try to clarify this here.*

This is an interesting comment, and very relevant for future ^{3}He research. However, we do not want to delve into this discussion in this (2024) manuscript, as we believe this discussion can (and should) be the centre of future research. We only measured mixtures of clino- and orthopyroxene (see paragraph in lines 287-290 of the submitted pdf), so we do not have any data regarding possible variabilities of ^{3}He production rates ruled by mineral composition.

Thad said, we now mention that our concentrates have higher contents of orthopyroxene that those of Blard (2006) and higher clinopyroxene contents than those of Eaves et al (2015). Additionally, we justify our decision to use a world wide mean in more depth and indicating why it is correct to do so in paragraph 3 of section 3.6.2

Section 3.6.2: " ^{3}He$_{cos}$ production rates have shown to be indistinguishable in clinopyroxenes and orthopyroxenes (Delunel et al., 2016), justifying our decision to use a worldwide mean production rate estimate for our exposure age determinations. Additionally, this production rate value is supported by a local calibration test using the radiocarbon-dated debris avalanche deposits of the Murimotu Formation, on the outer northwestern slopes of Ruapehu (Eaves et al., 2015). Despite some studies suggested that olivines concentrate slightly larger amounts of ^{3}He$_{cos}$ compared to pyroxenes (Ackert et al., 2003; Fenton et al., 2009), the difference was almost statistically insignificant, and in a more recent study, Fenton and Niedermann (2014, as well as previous data from Blard et al., 2006) provided results implying that olivine and pyroxenes have similar amounts of ^{3}He$_{cos}$."

Results (section 4.1): "Comparing the obtained average compositions with previous [3]Hecos studies, our pyroxenes show higher contents of orthopyroxene than those analysed by Blard et al. (2006) and higher clinopyroxene contents than samples of Eaves et al. (2015).

Figure 4. I found this figure to be quite hard to read, and recommend enlarging the fonts or the individual figures to make them easier to read.
AC: *Addressed, we enlarged the font sizes of Figure 4.*
Citation: https://doi.org/10.5194/egusphere-2024-163-RC3

Associate editor report (Marissa Tremblay)

Dear Pedro Doll and coauthors,

Your manuscript received 3 detailed and constructive reviews. I agree with the 3 reviews that this paper represents a significant and valuable contribution. The writing is clear and the figures are very well made. The response to reviews was generally appropriate. Given that you will need to incorporate new constraints on the magmatic helium component from new crushing measurements, and given that there will be a degree of restructuring of the manuscript (see below), I am recommending this manuscript be revised subject further review by me and possibly one or more of the referees. In addition to the manuscript revisions you described in the response to reviews, below I address your specific queries directed to the editor. I also request that you to provide some clarification on your comments about pyroxene compositional effects in response to reviewer Kurz.

AC: *We agree with the editor's decision.*

Major comments:

Manuscript organization: I do agree with reviewer Portenga that it would be helpful to describe the specific geologic context, any existing chronology, and new 3He chronology on a flow-by-flow basis in one place. I would keep sections 4.1 and 4.2 as distinct subsections of the Results, and work in many details from sections 5.1 and 5.2 into subsection 4.3. Consider renaming this subsection something like, "Surface exposure ages and comparison with existing eruption age estimates."

AC: *We appreciate the editor's point of view, and this modification was done as requested. Note that sections 5.1 and 5.2 remain, to address in detail the cases we considered needed appropriate explanation.*

Reply to reviewer Kurz about pyroxene composition: I struggle with the notion of "we have interesting data about pyroxene compositional effects but prefer to save this for a future paper," as this implies that the authors have evidence that pyroxene compositional effects on production rates matter, and therefore influence the results presented in this manuscript. The response written by the authors is quite vague, and so it's difficult for me to understand what's going on. Can the authors provide some rationale that these effects are not influencing their results in a significant way, while also not revealing new data they hope to use in a future study?

AC: *We apologize for the confusion we might have created. Note, however, that we did not state that we have interesting data about the compositional effects, nor that we are saving any data for a future paper. As explained in our answer to reviewer 3, we only analysed mixtures of clino- and orthopyroxenes. There is no conclusive evidence in the literature that these different minerals have significant differences in their* $^3He_{cos}$ *concentration (see full explanation in answer to reviewer 3).*

We still have (not analysed) sample leftovers, which could be potentially used for future research, although none of our sampled sites would be suitable for production rate calibrations.

We also justify the selection of a worldwide mean (not composition-dependent) production rate value in the 3rd *paragraph of section 3.6.2.*

Minor comments:

Figures: I support the inclusion of at least 2 more field photos to be included as additional panels in Figure 2, as well as for Figure A1 to be moved into the main text as part of Figure 3.
AC: *We appreciate this, we consider this will be beneficial for the reader. Addressed.*

Topographic corrections: Since the authors list these correction factors in a table, it is not necessary to add this to the text.

Ma vs. Myr: The journal does differentiate. See 'Abbreviations' under the English guidelines and house standards section of the Geochronology Submission instructions page.
AC: *We appreciate the clarification and apologise for not being thorough in the first version, now addressed.*

Tables: If it is a struggle to condense tables into a format that fits on 1 page, consider moving all or parts of the table to the supporting information. Additionally, please do not reduce the precision of the sample location information to make the table fit on 1 page. Unless there is a good reason to reduce the number of digits (e.g., legal or site preservation reasons why location specificity should be shared), you should report the coordinates with sufficient digits that someone else could revisit the sites you sampled.
AC: *We kept Table 1 condensed in 1 page, with the changes requested by reviewer 3. Table 2 changed from 3 pages to 3 ½ pages long with the modifications requested by the reviewers.*

Sincerely,

Marissa Tremblay
March 12, 2024

<u>Significant modifications in the manuscript</u>

Title: we added "Aotearoa" (Māori name for New Zealand) before "New Zealand" to acknowledge the bicultural nature of New Zealand.

Structure:

- Structure: we changed the structure of results and discussion, following the recommendations of reviewer 1. We renamed section 4.3 (section 4.4 in the new version of the manuscript) to "Lava flows: background and new ^3He constraints", including a short comparison of results (i.e. whether they match or not previous age constraints). However, larger discussion points are still left for section 5.1 (renamed to: "Comparison with previous age constraints").

Introduction

- Introduction: we added radiocarbon as a previously used dating technique and additional references to ^3He applications; emphasized on the novelty of the application of ^3He in stratovolcanoes; we moved part of section 5.4 to the introduction (as suggested by reviewer 1).

Methods:

- We added more examples of sampled surfaces in figure 2, showing more "typical" samples and other sampled surfaces (i.e. PR spatter deposit, WP blocky flow). We added a supplementary file with more photographs and short discussions regarding how adequate different surfaces are for surface exposure dating (now supplementary file S1).
- We further developed on the methods used for geochemical analyses, and added the procedure used to obtain in-vacuo released ^3He for the magmatic correction (new data).
- In section 3.6.1, we explained why the use of the R factor is essential in our study.
- We renamed section 3.6.2 to "Determination of exposure and <u>eruption</u> ages".

Results

- We moved included Figure A1 as Figure 3a.
- We explained that, due to the technique used, measured radioactive element concentrations in minerals (and the calculated partition coefficients) represent maximum values (section 4.1).
- We calculated partition coefficients for the measured radioactive elements and compared them with the bibliography (section 4.1).
- We added crushed He measurements (new data for local magmatic He ratio calculation) and expanded the justification behind the (modified) obtained magmatic $^3He/^4He$ ratio with a new subsection (4.2). We added two supplementary files with new crushed He data and the calculations used to obtain the final magmatic $^3He/^4He$ ratio (now supplementary files S2.1 and S2.2).
- We explained why it may be needed to better constrain magmatic $^3He/^4He$ ratios in other studies.
- We changed the unit in which we present $^3He/^4He$ ratios to Ra.
- We added the measured $^3He/^4He$ ratios in Table 2, and typical ranges in the text.
- We performed a sensibility test of the effect of possible variable magmatic ratios in our results (within section 4.3).
- We adapted the final cosmogenic ^3He values and the derived exposure/eruption ages to the new magmatic $^3He/^4He$ ratio.
- We changed the summary eruption age towards the bottom of each flow in Table 2, and added an extra row to improve the alignment between the individual exposure ages and derived eruption ages.

Discussion:

- We emphasized on the novelty of the application of 3He in stratovolcanoes in section 5.4.

- We extended section 5.4 using data-driven interpretations.
- We developed on hazard implications of our results in subsection 5.3.

Minor modifications

- We made Figure 5 sorter to make it easier to read.
- In section 3.5, we indicated the standard reproducibility throughout our study.
- We clarified that the units shown in Figure 1 are according to previous studies in the label panel.
- We added which samples were re-dated and our results in Table 1.
- We increased the font size in Figure 4.
- We added one (1) digit to the latitude and longitude columns in Table 2 and Table A3
- We added the word "individual" before "lava flows" in line 11 to avoid potential confusion (i.e. as raised in item 9 by reviewer 1)
- In the introduction, we added references on literature from ^{3}He applications.
- We brought forward the explanation of the abbreviation $^{3}He_{cos}$ within the introduction.
- We added the reference of Gabrielsen et al. (2018) when mentioning the spiritual significance of Ruapehu in section 2.1.
- We added the missing vent locations in Figure 1c.
- We fixed the issue of mentioning Figure 4 before Figure 3 by eliminating the (unnecessary) mention of Figure 4 in the Methods section.
- We modified the wording in the geochemical results section for improved clarity and reading fluidity.
- We modified the wording in paragraph 1 of section 5.1 to clarify which ages are susceptible to user's decisions.
- We specified that our new eruption ages of the Lava Cascade and Taranaki Falls flow are older than radiometric ages for these flows by Conway et al. (2016).
- We fixed the typo of having a stoke in "$^{3}He_{cos}$" in paragraph 2 of section 5.1.
- We defined what we mean with a "rootless flow" in the second to last paragraph of section 4.4.1.
- We changed the terminology regarding Reviewer 1 comment (Lines 404-405: I am uncertain of the significance of the second half of this sentence (i.e. "as it precedes…"), both in terms of the order of events, rootless nature of the flow, and the geomorphology. Can the Authors please clarify?) in the second paragraph of section 5.1.
- We added the missing parentheses to "Figure 5" in the first paragraph of section 5.1.
- We merged the last two bullet points of subsection 5.2.
- We changed the wording on the first paragraph of section 5.3.
- We slightly modified Figure 6, making translucid those polygons representing lava flows with no chronological data (and classified in units based on geochemistry and geomorphology).
- We fixed the misuse of *ca*, restricting it to approximate ages. We also fixed the misuse of ka to represent periods of time instead of ages.
- We modified the word "crystals" to "minerals" in the Table A2.
- We reduced the significant figure by one in the average thickness in Table A3.
- We fixed a column placement issue and a column heading issue in Table A3.
- We modified our wording across the whole text to eliminate apostrophes after possessive plurals.
- We specified the pressure under which He was condensed, in the methods section.
- We specified that the blank analyses were done each day, not prior to each analysis.
- We modified the wording around the reason why we sampled the GR flow.
- We fixed typos in the references list.