VICTORIA UNIVERSITY OF WELLINGTON TE HERENGA WAKA

**SCHOOL OF GEOGRAPHY ENVIRONMENT AND EARTH SCIENCE**
TE KURA TĀTAI ARE WHENUA
**VICTORIA UNIVERSITY OF WELLINGTON,** PO Box 600, Wellington 6140, New Zealand
**Phone** + 64 4 463 5337 **Email** geo-enquiries@vuw.ac.nz **Web** wgtn.ac.nz/sgees

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Dear Assistant Professor Jensen,

Thank you for your detailed comments on our response to reviewers for our manuscript submitted for open review to *Geochronology*. Your depth of comment and support are much appreciated.

We have resubmitted a version of the manuscript with tracked changed to allow the revisions to be more apparent. We have addressed all the reviewer comments in this revised version of the manuscript. We have taken particular note of Abbott et al., 2020 "Community Best Practises" manuscript as advised by Reviewer 3, and have made some additional changes based on this guidance.

Primarily, as was requested by all three reviewers, we have spent time re-reducing all the data, including re-standardisation and further outlier removal, to produce what we now believe to be a very robust, accurate dataset. We have also added in extra text to improve the clarity and transparency of the data collection, and reduction, and have updated all the figures and tables to reflect this additional work on the data revisions. We hope you agree the article now suitable for publication as a foundation reference data set for New Zealand tephra studies.

We also note some additional comments from the Editor (shown in Green) which we address below:
Dr. Portnyagin points out the issues using VGA568 as calibration for Cl (too low to be used) and Na issues with ATHO-G in the same run that Dr. Kuehn also discusses. You mention that steps you will take to clarify why there are differences and adjust as necessary, however, you do not address the specific issue with Na and Cl. These should be responded to directly – I do not suggest re-analyses, but there has to be more clarity behind the quality of some of the analyses where standards suggest potential issues. This is also the case with the potential interference on Sc due to the oxide interferences. ThO/Th 1.3-1.8% is quite high, regardless of other reports and is a potential explanation despite the lack of other apparent oxide interferences. I just want to emphasize here that in an analytical dataset this large there is an inevitability that not every run was perfect, but when problems crop up that don't disqualify using the data but complicate it from a particular day, it is best to note all the reasons those problems may have arisen and state them clearly. I believe this is largely what both Dr. Portnyagin and Dr. Kuehn would like to see in the revisions. More acknowledgement, clarity and clear reporting of potential problems
We have added an additional subsection to the results section (Sect. 3.1 Data Quality; line 316- 402) in which we discuss the quality control on the data, and the variability seen in the analyses. This new text allows for more transparency in the dataset. We have also added a number of extra figures in the Supplementary Material (SM Tables and Figs 6) which we have used to show more clearly the quality of the data. This work aims to address the comments by the reviewers on the data quality and the transparency of our data collection and analytical accuracy and precision. As advised we hope we have acknowledged the key issues brought up by the reviewers.

As you have noted, the screening of the data is an issue raised by all the reviewers. I suggest (as seems to be your intent) to take the path suggested by Dr. Kuehn, which is to remove outliers from the main population

but still report them (just noted as outliers). This overcomes some of the interpretive challenges and allow you to make specific notes for the outliers as well (i.e., potential phenocryst contamination vs. outlier glass composition etc.).
However, Dr. Portnyagin does make some helpful suggestions as to how to parse the trace element data in particular, and in my opinion, does not force an overly heavy discrimination of the data as you argue happens on occasion. There are natural trends and variation that can be expected but then there are some that are much more likely contamination.
We have extensively screened the data and removed the outliers – which are now reported at the end of the data tables in the Supplementary Material.

The anonymous reviewer does have some challenging comments, several of which I see as beyond the scope of what your paper is attempting to achieve (as you note). However, I do think it would be helpful to make sure to explicitly address some of their points about geographic/stratigraphic coverage. You give a strong response to their comment, but it is not clear how you will address it in the paper itself. In particular I would be curious about their point that some of the localities they mention cannot be dismissed because of your reporting of layers from the Wanganui Basin - which is apparently relatively poorly understood. They appear to anticipate your response in their comment and you do not quite address it.
We have added some extra details in Section 2.1. (now entitled “Sample selection, collation, and collection"; line 181-198) to address the anonymous reviewers concerns about the geographic coverage of the data.

There seems to be a potential misinterpretation about the comments on the juvenile clasts. Juvenile ‘clasts’ in the way that the anonymous reviewer is discussing them refers specifically to the magmatic ejecta (i.e., just the tephra). So when they discuss the proximal deposit lapilli and pumice they are simply discussing the size fraction of the proximal tephra. In this context a pumice/lapilli clast is no different than a smaller glass shard – just bigger (and potentially more heterogeneous). And just like distal glass, they require numerous analyses to gain a true sense of the geochemical heterogeneity in an eruption. In that context do not quite understand your statement that an analysis on lapilli is essentially equal to a whole rock analysis– an analysis on a single lapilli is no different and an analysis on a distal glass shard. What I think the reviewer is getting at is that in your proximal deposit samples, some of the heterogeneity maybe due to the analysis of non-tephra glass, which I think is a fair interpretation. Reading the methods, you chose a sieved size fraction – a relatively fine one – and there was no componentry in the choice of material to analyse in the proximal samples. In this case there certainly is a possibility that this size fraction could contain shattered glassy material from a dome or obsidian - when broken they can look like (generally blocky) glass shards.

Regardless, in most of these cases I think simply a clear acknowledgement of some of the challenges in interpreting the analyses and organizing them as noted by Dr. Kuehn would be sufficient.
We have added extra clarity into the methods sections for EPMA analysis (Sect. 2.3) and LA-ICP-MS analysis (Sect 2.4). In addition, as reported above, we have added an additional paragraph to the results section (Sect 2.1) in which we discuss the quality control on the data, and the variability seen in the analyses. This allows for more transparency in the dataset. We have also added a few extra figures in the Supplementary Material (SM Tables and Figs 6).

Some minor comments:

For Dr. Kuehns comment on line 192 – this is an important question, and you agree with it but don’t offer how you are going to address it in the paper (with a statement that some of these are uncertain?)

See text added lines 210 to 212

Additionally, for some other comments (e.g., Line 245-255) you mention they are "noted" – does that mean his suggestion will be utilized or simply you acknowledge the option of doing it this way but will stick with the original method?

At this stage we have just noted these as we saw these as "suggestions" rather than requirements: lines 245-255 - to use VG-A99 and ATHOG as normalisation standards would preclude their use as secondary standards and hence would likely require a further standard to be used as a secondary standard; line 315 – we intend on having a follow up manuscript that looks into the statistical options for this data, hence why only simple PCA and ESC have been used at this stage.

Below we also detail the key changes made to the Figures and Tables:

Figure edits

Below we detail all the edits made to the figures in the updated version of this manuscript:

- Figure 1 no changes made
- Figure 2 edited with outliers removed for points 8 and 9 of NIST 610, note made in the figure caption
- Figure 3 updated after outlier removal and further data reduction
- Figure 4 updated after outlier removal and further data reduction; rounding improvement could not be made, as requested by R3, but this doesn't impact the data just the image aesthetics.
- Figure 5 updated after outlier removal and further data reduction
- Figure 6 updated after outlier removal and further data reduction
- Figure 7 updated after outlier removal and further data reduction
- Figure 8 updated after outlier removal and further data reduction; also now plotted as sample/primitive mantle (Sun and McDonough 1995) rather than sample/chondrite as requested.
- Figure 9 PCA analysis has been re-run after performing centre log ratio on the data set to account for the closure effect on the data therefore this is a new plot for this data.
- Figure 10 PCA analysis has been re-run after performing centre log ratio on the data set to account for the closure effect on the data therefore this is a new plot for this data.
- Figure 11 updated after outlier removal and further data reduction
- Figure 12 updated after outlier removal and further data reduction
- Figure 13 updated after outlier removal and further data reduction
- Figure 14 ESC rerun after outlier removal, and new figures made to reflect this change
- Figure 15 updated after outlier removal and further data reduction

Table edits

The following table edits have been made, and the table references have been updated in the text:

-Table 1 locations converted to Lat, Long in decimal degrees; Smithsonian GVP numbers added; date of analysis for EPMA and LA-ICP-MS added.

- Table 2 moved to SM Table 1.1b to be read alongside the EPMA set up information, additional data added to show crystals on which each element was run.

- Table 2 (new; original Table 3) updated after removal of outliers this information is also added in the text lines 260 to 261 and line 303.

- Table 3 new; original Table 4 – updated in the text. Additional details added for the Taupō eruption which there is new data for from the work of Barker et al., 2015

-Table 4 new; original Table 5 – updated references in the text, no changes made to this Table.

- SM Table 1 updated after Abbott et al., 2020. This now shows full details for EPMA (SM Table 1.1a and 1.1b) and LA-ICP-MS (SM Table 1.2) set up as prescribed by "Best Practise guidelines" and requested by all Reviewers. High oxide levels now discussed in text lines 383 to 393 with reference to SM Table 6, Figures 6.2.3, 6.2.4, and 6.2.5 which show plots of isotope comparisons where multiple isotopes of the same element were run, and between Eu and Ba. The isotope comparison plots (6.2.3 and 6.2.4) show R2 values ≥ 95% suggesting no impact from oxides. In addition, the plot of Eu vs Ba (6.2.5) shows now correlation also showing little to no impact from oxide production.

- SM Table 2 updated with outliers removed ($H_2O_D \geq 8$ wt%); removal of double isotopes; statement of clarity about data presented detailed in the text lines 260 to 261

- SM Table 3 values for A99 added, and details added to give more information on the figures show, outliers removed from results and added to list at bottom of the tables, and references to standard values given. SM Table 6 added to give more justification as to the reason for the use of a different standard value than the Jarosewich et al., 1979 paper. Details for this have been added in the text lines 362 to 382.

- SM Table 4 updated with outliers removed, 2*std changed to "2*stdev" and Offset now reported in (ppm) rather than (%).

-SM Table 6 (new) Figure 6.2.1 and 6.2.2 show plots of the data for elements Ti and Mn, which were run on both the LA-ICP-MS and the EPMA.

- Two versions of data included, individual tables for manuscript publication, and full tables submitted as supplementary files.

We look forward to hearing from you with regards to the improvements to the paper.

Many Thanks,
Dr Jenni L Hopkins (for the Authors)

Jenni.hopkins@vuw.ac.nz