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Interactive comment on "Development of a multi-method chronology spanning the Last Glacial Interval from Orakei maar lake, Auckland, New Zealand" *by* Leonie Peti et al.

Anonymous Referee #2

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Review for Peti et al.

Development of a multi-method chronology spanning the Last Glacial Interval from Orakei maar lake, Auckland, New Zealand

In review at Geochronology Discussions

Peti and colleagues present a multi-proxy age model for an exceptional sedimentary sequence spanning the last glacial cycle from the Auckland Volcanic Field. To develop the ago model, they integrate radiocarbon, tephra stratigraphy, luminescence dating, paleomagnetism, and cosmogenic Be. To treat their data objectively and to quantify uncertainty, they employ Dynamic Time Warping (DTW) and Bayesian Age Depth

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Discussion paper

modeling methods. Overall, the paper is well written, and the data are clearly presented. I was interested in reading more about the archive and the author's approach and perspectives on building their multi-proxy age model. Studies like this are essential for all of us that work on sedimentary sequences and the chronology will likely form the backbone for many future studies that will work on the Orakei (and other regional) maar lake. I feel this study is certainly suitable for publication in Geochronology with some revision. I am not an expert in the luminescence dating methods, and while they seem properly documented and presented in a way I can follow, hopefully another reviewer can evaluate them in more detail.

General Comments

Radiocarbon:

I feel that, while not perfect data (e.g., age reversals, unknown reservoir effects), the treatment of the radiocarbon data is fair and the authors are honest about their uncertainties. I would recommend the authors update the calibration to the SHCal20, now that it is available, and present (and make available) both the SHCal13 and SHCal20 based age models (so that other authors can make direct comparisons to either). Otherwise, the next study that presents Orakei maar lake data on age will need to re-do the age model and this age model will be dated.

Tephra Stratigraphy:

Obviously, the author's identification of the "unidentified" basaltic tephra layer T66 is central to the older part of this age model and the only real constraint beyond the RPI correlation (as the uncertainties in the luminescence data prevent those data from providing strong constraint at the temporal resolution of the final age-depth model). The authors propose that this a newly recognized tephra for the AVF, AVFaa, as it cannot be correlated to previously identified tephra layers. They use the Ar/Ar constraints from their proposed eruptive center, Mt. Albert, to assign an age to this layer. I think this assumption is reasonable, and while it is better explained in the appendix, I think it

deserves a little more attention in the main text (and perhaps the abstract) because of how important this interpretation/assumption it is to the final age model. This should maybe include the data needed to identify the tephra in the main text, as the authors do for their other tephra in Figure 3. The way the treatment of T66 is presented in the results section 4.1 makes it seem like the age of this tephra layer is well known, the eruptive history of Mt. Albert is well known, and the tephra identification has no ambiguity. This new AVFaa tephra may also be important for future studies. See below, but I also am curious if there is an RPI DTW solution that independently supports this age assignment.

I am assuming that AVF1 was not used in the age model because it has two possible ages ∼106 vs ∼83 ka. It seems like the author's age model, while not using the tephra as a constrain, is more consistent with the older of these two ages. I think it would be worthwhile to add a paragraph in the main text to discuss the AVF1 tephra, how the previously published age constraints were derived and how the new age model compares. Does the new age agree with either of the older ages? Why or why not do you think that is the case? Does it provide an addition independent support for the RPI based correlation?

Paleomagnetism:

I liked the authors use and application of DTW in their correlation of the RPI data. We all know that wiggle stratigraphic correlations can be non-unique, so while not always perfect, at least DTW is objective. However, to get a perfect DTW solution requires perfect data (which is never the case and cannot be expected in paleomagnetism). Thus, the result of the DTW solution when using a general DTW algorithm (like the one used in this study) for geologic data is often a stair-step pattern, implying sediment delivery in pulses separated by periods of no deposition. However, we often assume that sedimentary records like these accumulate gradually over time. The authors in a way deal with this by randomly sampling tuning points from the DTW solution and setting hard start/end tie points. However, this is problem that Hay et al, which the authors cite, also

address through their development of a DTW algorithm. In this algorithm, users can work with imperfect data by varying assumptions relevant to geologic data (such as how variable sediment accumulations are) to explore various possible DTW solutions that can be evaluated against independent constraints and/or expert knowledge.

Do the authors think it would be worth trying the Hay et al. DTW approach to explore other possible DTW solutions that may be more reasonable for imperfect geologic data? Why or why not? Can you treat the AVFaa tephra age independent of the RPI DTW solution and find a solution that independently supports the age the authors assign to the AVFaa tephra?

Sedimentation Rates:

It makes me nervous when I see a major change in sedimentation rates at a depth where the main chronometer for the age model changes. In the case of this study the authors find a switch from lower to higher sedimentation rates at around the same depth that the age model changes from being primarily constrained by RPI correlation to radiocarbon. I think this observation should be included in the main text. Why should I, the reader, be convinced that this accumulation rate change is the real signal and not an artifact of a non-unique or problematic RPI correlation? It doesn't appear to exactly line up with the facies unit changes or the lithologic log, but maybe there are other data that show a sedimentological change around the same time?

Data Availability:

Thanks for posting your data to Pangea. I would also recommend including the actual age-depth relationship with uncertainty as an independent contribution.

Specific Comments:

Line 263: Hay et al. aligned chemostratigraphic data, not paleomagnetic data. Their algorithm was modified to work with paleomagnetic vector data by Hagen at al. But, the Hay et al. algorithm would be the appropriate choice for RPI correlations.

Lines 495-515: There is information in this section that seems like it would fit better in the methods section, particularly the choice of DTW algorithm.

Figure 2: Would it be helpful to indicate the stratigraphic position/labels of the tephra layers?

Figure 6: It is difficult to read the small text in this figure. Please make the text larger.

Figure 7: It might help the clarity of the figure to decrease the symbol size so that it is easier to see how the age control points compare to each other.

Figures B1-B2, B4, C1-C6: All of these figures would benefit from increasing the font size of the smaller fonts to make them more legible.

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