Geochronology Discuss., author comment AC2
https://doi.org/10.5194/gchron-2021-19-AC2, 2021

Reply on RC2

Douglas P. Steen et al.

Author comment on "Paleomagnetic secular variation for a 21,000-year sediment sequence from Cascade Lake, north-central Brooks Range, Arctic Alaska" by Douglas P. Steen et al., Geochronology Discuss., https://doi.org/10.5194/gchron-2021-19-AC2, 2021

We appreciate the thorough and detailed review by Anonymous Referee #2. Our responses and proposed changes to the text are outlined below. Author comments are in bold below corresponding referee comments.

A general problem is that the two manuscripts frequently cross-reference each other and it is quite difficult to understand how either one can stand alone. Some raw data are put into both manuscripts (e.g. the 14C dates and tables) which gives future writers an unnecessary choice about which source to cite. Davies et al. state that details of the radiometric age-model construction are in Steen et al., but there are more details in the former than the latter.

We see the duplication of the datasets as minimal, and the inclusion of radiometric dates as important for both papers. The removal of these data from either paper would hinder its ability to stand alone. To avoid overt duplication, however, the 14C and 210 Pb data will be shifted into a supplement for the Davies et al. companion paper.

The editor could consider asking the authors to combine the papers into one so that problems that arise from twin-submissions are negated.

Prior to submitting the papers, and now following this review, the authors of both papers have considered how the papers could be combined into one. We have made a concerted effort but cannot find a path that enables us to present all the relevant data. We submitted these two manuscripts to Geochronology because we aim to convey the important details about the individual dating methods to this specialized audience. We believe that attempting to do this with a single paper would be unwieldy for readers: both manuscripts date sediments from this high-latitude lake using two completely unrelated methods and reporting this in a meaningful fashion requires two separate papers that can properly present the methods and results for these independent approaches.

There is also reference to a Masters thesis by Steen (2016) that is not easily accessible and Davies et al. refer to a submitted manuscript by Jensen et al., which might be crucial because it concerns the age of a cryptotephra.

Steen's (2016) thesis is available as open access through ProQuest: https://www.proquest.com/docview/1808501293. We will add the full URL link to the reference cited list. The submitted Jensen et al. manuscript is an invited review and, following its scheduled timeline and assuming its eventual acceptance, it should be publicly available by the time this manuscript may be published. However, this information is not crucial, as explained in Davies' response to reviewers of the companion paper.

In addition to a number of specific comments listed further on, I have a couple of general doubts about the manuscript by Steen et al. First, there are rather limited data, which are not replicated. Palaeomagnetic studies of lacustrine and marine sites should rely on three of more cores from each site so that there is replication of results, which can be stacked, and also the possibility to reject outliers. In this study two cores (CASC-4A & CASC-2D) were taken and measured for paleomagnetic properties. The authors state that CASC-4A has a more complete record of unit L3 and that CASC-2D data from unit L3 are rejected. Then the authors take higher resolution data from unit L2 in CASC-2D and splice them onto CASC-4A by matching the inclination records. No details about this match are provided. This exercise assumes the data from CASC-4A are good and forces the CASC-2D data from L2 to fit. It is better to use an independent parameter or proxy (magnetic susceptibility, for example) to match the records and then stack the paleomagnetic data and consider uncertainties.

The procedure used to create the composite sequence, as explained by the reviewer, is described on lines 261-271. We will add a reference to Figure 2.7 in Steen (2016), which illustrates the splicing. We understand the advantage of replication and stacking to generate a composite record. We also believe that splicing the lower part of one core, taken from where sedimentation rates are higher, onto the base of the other core is a valid approach. This strategy follows, for example, the recent development of a premier global marine oxygen-isotope record (Westerhold et al., 2020). Rather than stacking records, which is known to smooth variability, the record was spliced together from sites where resolution was highest for each interval. In addition, while further replication is always better, we believe that the available data are sufficient for the purpose of this study: helping to constrain the chronology of the sedimentary sequence. We also note the relative paucity, and therefore the value, of this type of record, which reflects the logistical challenges in this remote region.

The second major reservation concerns the tie-points (control points) in Figure 9. These tie-points give the impression that there are high-frequency matches between the composite Cascade Lake inclination record, the field-model predictions and the Burial Lake record. But, the long-term trends seem very different, particularly for the Holocene. Specifically, there are tie-points within the interval 4-8 ka. Models predict high inclination but the data (the composite record) show low inclination. The authors state in the introduction that major directional (PSV) features predicted from models can be used for age control, but they subsequently ignore the differences in the long-term major directional features. Why? An associated problem concerns the tie-points between the model predictions and the Burial Lake record that are older than 5 ka. These are very large differences in age (2-3 ka) rather than the Burial Lake ages being "somewhat older". If the Burial Lake 14C-based chronology is valid (as the authors argue) one has to deduce that the geomagnetic model predictions are poor, and thus should not be used to provide correlation tie-points).

We will improve the objectivity and quantification of correlations between PSV records by using: (1) an established tie-point identification algorithm, such as QAnalySeries, to detect points of correlation between records, and (2) Pearson correlation coefficients to evaluate the strength of alternative tie-point

correlations. This procedure has been used in similar studies, including recently (Li et al., 2021).

Additionally, we will assign different levels of certainty to difference subsections of the final age model, depending on the number of chronological methods (PSV, 14C, and cryptotephra) used to construct each section. This will explicitly show where the available data are in good agreement versus where they are more limited. While we accept that there are uncertainties to some of the PSV interpretations, especially in the lower part of the sequence where they are not validated by other methods, we also see value in presenting these data.

In general, the manuscript contains a lot of detail that could be removed through combining the two papers into one. There are aspects about the temporal and spatial development of the geomagnetic field that would be better suited for submission (and review) by a specialised geophysics journal.

Prior to submitting the papers, and now following this review, the authors of both papers have considered how the papers could be combined into one. We have made a concerted effort but cannot find a path that enables us to present all the relevant data. Both manuscripts present first attempts to date sediments from this high-latitude lake using two completely unrelated methods and reporting this in a meaningful fashion requires two separate papers that can focus on properly presenting the methods and results for these independent approaches. We acknowledge that the information included in Section 2.2 on the geomagnetic setting of the field area is not necessarily relevant to the discussion and main conclusions of this study, which focus on improving the chronology of the sedimentary sequence using paleomagnetic data. We therefore intend to omit this section to avoid diluting the main points of this study with extraneous information.

Specific comments

The start of the introduction could be more general and focus on paleomagnetic secular variation and its advantages and limitations as a relative dating method. The authors use the term wiggle-matching, which is often used by the radiocarbon community to objectively (statistically) match established changes D14C, but which here is really visual (and quite subjective) matching of trends in the PSV data and model predictions.

We will omit the term "wiggle matching" to avoid confusion with the procedure used in 14C dating.

The geomagnetic field models are not perfectly constrained, anywhere. Section 2.2 "Geomagnetic setting" contains details about the origin of the geomagnetic field and its manifestation on Earth's surface that are unnecessary in this study's context. The discussion and conclusions do not refer back to these details, so I suggest that they are omitted.

We are comfortable omitting this section to avoid diluting the main points of this study with extraneous information.

Section 3.1 The individual sections of the cores were not relatively aligned to each other or absolutely aligned to an azimuth, which seems like an experimental error if the purpose of the study was PSV. I appreciate that t is difficult to obtain whole cores that are oriented to an azimuth, but it is relatively easy to keep sections oriented when the core is cut into sections. Why was this orientation not done?

The core sections were not aligned relative to each other or absolutely aligned to an azimuth because the cores were not initially collected with the goal of obtaining paleomagnetic data.

Section 3.2 What is the approximate half-width of the signal that the 35 mm Bartington loop measures? That distance is equally important as the measuring increment (1 cm).

The spatial resolution of the Bartington MS2C is 20 mm. We will add this detail.

The authors mention that a couple of segments were not measured. The reason should be stated in the methods section. I think that the segments were measured, but the results were bad due to saturation of the SQUIDs by highly magnetic layers.

U-channels were not collected from Lithologic Unit 1 (L1) because it is a stony diamicton and would therefore not provide a reliable record of PSV. The interval from 387 – 251 cm blf in core CASC-2D was not measured for SIRM due to saturation of the SQUID electronics, as suggested by the Referee's comment. Please see lines 160 – 166 of the Preprint for additional information on this topic.

Section 3.3 These methods (and also the results) are duplicated in the twin paper by Davies et al.

We see the duplication of the datasets as minimal, and the inclusion of radiometric dates as important for both papers. The removal of these data from either paper would hinder its ability to stand alone. To avoid overt duplication, however, the 14C and 201Pb data will be shifted into a supplement for the Davies et al. paper.

Section 4.1 The authors state that the sediments recovered are undeformed by the coring procedure. This statement contrasts with an earlier statement made in section 3.2 about the samples being taken from as close as possible from the centre of the core, unless "appeared less disturbed by minor coring deformation". Please be consistent, and how do you know that the sediments are undeformed and/or deformed?

We are grateful to the reviewer for pointing out this contradiction. Indeed, the statement in Section 3.2 is not correct. It actually refers to the cores from Shainin Lake, which was included in Steen's (2016) thesis. In fact, the cores collected from Cascade Lake do not display visual evidence of deformation. The statement regarding samples being "taken as close as possible to the center axis of the core, unless either side of a core section appeared less disturbed by minor coring deformation" will be removed. We will also include clarification that sediment layers are undeformed in the Cascade Lake cores.

Please avoid the use of terms like "are significantly lower" where there is no known significance.

Terms like "are significantly lower" will be removed, as suggested.

The authors mention a "hint at the authigenic creation of greigite" but there is no proof. It would be better to state that the cause of the highly magnetic layers is unknown so that there is no speculation.

We agree and will add a sentence to acknowledge that this hypothesis cannot be substantiated without further analysis.

The average inclinations are close to the GAD model prediction for the site latitude, which the authors use to argue that the PSV record is good, but earlier on the authors state that the geomagnetic field might be different at high latitudes due to the tangent cylinder. The logic seems a bit circular. All palaeomagnetic data (that are ideally oriented to an azimuth) test the GAD model.

We intend to omit this section to avoid diluting the main points of this study with extraneous information on geomagnetic field dynamics and the tangent cylinder.

The data shown in Figure 5 were obtained based on analyses of the raw palaeomagnetic data, with examples shown in Figure 6, so it might be better to place current Figure 6 before current Figure 5.

We appreciate the suggestion, but it is our opinion that the order of Figures 5 and 6 is good in the current configuration, and that there would be little benefit to the reader if the order were switched.

Figure 2 is very cramped. The reader is unable to obtain any useful information from the different coloured lines that show all the ARM and IRM demagnetization data. I recommend simplifying the figure.

We understand the suggestion that Figure 2 could be simplified, however data for demagnetization of the IRM and ARM highlight some unique magnetic properties of the sedimentary sequence. For example, it shows the anomalous saturation of the SQUID electronics during IRM demagnetization of CASC-2D between ~ 250 and 300 cm blf, while these issues were not encountered in other core sections.

Figure 3 shows that the hysteresis loops are not closed at 1T, which means that a slope correction does not only correct for paramagnetic (and diamagnetic) contributions. The correction will include an unknown part of the unsaturated anti-ferromagnetic component, which seems relatively high in this case.

We will add a sentence to acknowledge that hysteresis values used in the Day diagram are estimates because of the issue raised by the reviewer.

Section 4.4 The maximum angular deviation (MAD) is really a measure of how well the ChRM can be defined, rather than a measure of magnetic stability. It is influenced by the stability of the equipment used to demagnetize and measure remanence and the signal-to-noise ratio. Low MADs are not a guarantee that the data reflect the ancient geomagnetic field direction.

We understand that low MAD values do not guarantee a high-quality paleomagnetic record that preserves ancient geomagnetic field direction, however, low MAD values certainly have been accepted as a prerequisite for such records (Stoner and St-Onge, 2007). In our experience using this equipment, such low MAD values are rare, pointing to the extremely well-resolved magnetization of Cascade Lake sediment. This inference is supported by inclination values that vary around GAD predictions and show variations consistent with known PSV.

Section 4.5 The relevance of the attempt to reconstruct a relative paleointensity (RPI) record is perhaps out of context with the aims of the journal (geochronology) and particularly the twin submission by Davies et al.

While the relative paleointensity (RPI) record is perhaps not directly related to

the main goal of this paper, it is common for similar paleomagnetic data studies to include estimates of RPI when possible. It is also reasonable to assume that these data could be useful for future regional studies of RPI or the construction of geomagnetic field models.

Section 4.7 As previously mentioned, the radiometric age model is presented in more detail by Davies et al. To avoid duplicating raw data I suggest either combining to the two manuscripts, or allowing Davies et al. to present the age model in detail, which Steen et al. test using the paleomagnetic data in a subsequent manuscript. The data in Table 1 show the 210Pb activity for the upper 3.6 cm, which is not useful for the PSV data set (no PSV data are from the short core)

We see the duplication of the datasets as minimal, and the inclusion of radiometric dates as important for both papers. The removal of these data from either paper would hinder its ability to stand alone. To avoid overt duplication, however, the 14C and 201Pb data will be shifted into a supplement for the Davies et al. companion paper. The 210Pb data are important as an independent constraint on sedimentation rate.

Section 5.2 I have made a general comment about this section in my opening paragraph. There are several problems with correlations, mainly associated with the (dis)similarity of the different curves. I do not understand why the authors consider that the Burial Lake radiometric age-model is more reliable than the Cascade lake because the sedimentation rate is rather linear. What is the reason for this argument? The Burial Lake radiometric age-model is definitely better because the 14C dated material did not contain terrestrial organic matter.

We agree and state that the terrestrial organic matter used for radiocarbon dating at Burial Lake is the main reason why the Burial Lake radiometric age model is preferred. We will omit the statement about the Burial Lake age model being "linear" as justification for its reliability.

There is a reference to a Masters thesis by Steen (2016) and an alternative PSV age model, which has been rejected by this study. I leave it up to the editor(s) to decide if this reference is suitable.

Section 5.3 This section contains quite a lot of speculation about the reason for possibly too old 14C dates. The authors need to consider that a paleomagnetic lock-in depth (delay) might also apply to the Burial Lake record, but the comparisons with the predictions of field models suggest that the offset would be quite large, possible unreasonable, in terms of depth (time).

Dorfman (2015) estimated that Burial Lake PSV features were ~ 200 years older than similar features in western North American records (Hagstrum and Champion, 2002), and that this difference could be attributed to post-depositional lock-in, concluding that Burial Lake could place an effective "maximum age" on geomagnetic features in this region.

Section 5.4 Much of this section is not relevant to the journal (Geochronology) because it concerns the development of the geomagnetic field (using paleomagnetism) and would be better suited to a submission and review by a specialised geophysical journal. The comparisons with regional records (and a global VADM) in Figure 11 seem unnecessary in the context of the aims of the twin submissions. Figure 11A has no subjective tie-points (unlike Figure 9) and I do not see much similarity between the different records. If these records were plotted against each other (using age as the control) I doubt that one would find a significant correlation. Have you tried to statistically check the similarity in this way and how adjustment of the age-models might improve a correlation coefficient?

We believe that there is value in comparing our Cascade Lake data with other regional records (Fig. 11). These comparisons are part of our comprehensive and balanced presentation of evidence of a geomagnetic signal. Based on this and other review comments, we intend to improve our tie-point correlation procedures.

References cited in Authors' replies

Hagstrum, J.T., Champion, D.E., 2002. A Holocene paleosecular variation record from 14C□dated volcanic rocks in western North America. Journal of Geophysical Research: Solid Earth (1978–2012) 107, EPM–8.

Jensen, B. J. L., Davies, L. J., Nolan, C., Pyne-O'Donnell, S. D. F., Monteath, A. J., Ponomareva, V. V., Portnyagin, M. V., Cook, E., Plunkett, G., Booth, R. K., Hughes, P. D. M., Bursik, M., Luo, Y., Cwynar, L. C. and Pearson, D. G.: in revision. A latest Pleistocene and Holocene composite tephrostratigraphic framework for 765 paleoenvironmental records for northeastern North America, Quat. Sci. Rev., n.d.

Li, C.G., Zheng, Y., Wang, M., Sun, Z., Jin, C., and Hou, J., 2021. Refined dating using palaeomagnetic secular variations on a lake sediment core from Guozha Co, northwestern Tibetan Plateau. *Quaternary Geochronology*, *62*, 101146.

Steen, D.P., 2016. Late Quaternary paleomagnetism and environmental magnetism at Cascade and Shainin Lakes, north-central Brooks Range, Alaska, MS Thesis, Northern Arizona University. https://www.proquest.com/docview/1808501293

Stoner, J.S., and St-Onge, G., 2007, Chapter Three: Magnetic Stratigraphy in Paleoceanography: Reversals, Excursions, Paleointensity, and Secular Variation, Developments in Marine Geology, 1, 99-138, doi:10.1016/S1572-5480(07)01008- 1.

Westerhold, T., Marwan, N., Drury, A. J., Liebrand, D., Agnini, C., Anagnostou, E., ... & Zachos, J.C., 2020. An astronomically dated record of Earth's climate and its predictability over the last 66 million years. Science, 369(6509), 1383-1387.