



Comment on gchron-2021-19

Anonymous Referee #2

Referee 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-RC2>, 2021

The manuscript by Steen et al. is part of a "twin-submission" that collectively build an age-depth model for a sediment sequence in Cascade Lake. Steen et al. focus on a paleomagnetic secular variation (PSV) record that extends back to 21 ka, which is a strength of the paper. The companion paper by Davies et al. (Gchron-2021-18) focuses on late Holocene cryptotephra in Cascade Lake and the construction of an age model based on radiometric methods (^{210}Pb and ^{14}C) and correlated ages of the cryptotephra. Davies et al.'s dating results are used to support Steen et al.'s conclusion that there are significant differences compared to (i) the radiometric-based age model and (ii) one that is based on relative paleomagnetic secular variation (PSV) correlations to the predictions of time-varying geomagnetic field models and the trends in an independently dated PSV record from Burial Lake (only 200 km away). The two manuscripts come to the same conclusion that several radiocarbon ages are too old, which is well known for lake sediment samples that contain more or less terrestrial derived organic matter that is older than the age of deposition. 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 ^{14}C 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. The editor could consider asking the authors to combine the papers into one so that problems that arise from twin-submissions are negated.

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.

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 or 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 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).

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.

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 $\Delta^{14}C$, but which here is really visual (and quite subjective) matching of trends in the PSV data and model predictions.

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.

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 it 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?

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 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.

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

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?

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

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.

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.

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.

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.

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.

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.

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.

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 ^{210}Pb activity for the upper 3.6 cm, which is not useful for the PSV data set (no PSV data are from the short core)

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 ^{14}C dated material did not contain terrestrial organic matter.

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 ^{14}C 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, possibly unreasonable, in terms of depth (time).

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?