

Geochronology Discuss., referee comment RC2
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Comment on gchron-2021-15

Anonymous Referee #2

Referee comment on "A Bayesian approach to integrating radiometric dating and varve measurements in intermittently indistinct sediment" by Stephanie Harmonie Arcusa et al., Geochronology Discuss., <https://doi.org/10.5194/gchron-2021-15-RC2>, 2021

The manuscript by Arcusa et al. investigates non-glacial laminated clastic sediments from high-mountain (3874 m asl) Columbine Lake in Colorado (USA). The focus of this study is on using varve counting in combination with sophisticated statistical data treatment for establishing a sound chronology – a process supported by radiometric dating (^{14}C , ^{137}Cs , ^{210}Pb). Based on this chronological approach, a high-resolution record of sedimentation rates is elaborated, which is intended to be used for future studies of this lake record via calculation of flux rates. XRF, HSI and magnetic susceptibility scanning results in combination with discrete samples for dry bulk density, water content, LOI550 and biogenic silica are additional data on display.

The manuscript reads well but there are issues that relate to data presentation and interpretation, which reduce the quality of this manuscript substantially. The paper addresses relevant scientific questions within the scope of "Geochronology" and presents novel ideas and tools. However, no substantial conclusions are reached, the scientific methods and assumptions are not valid and clearly outlined, results are not sufficient to support the interpretations and conclusions and the description of site, methods and results are not sufficiently complete and precise to allow their reproduction (traceability). In more detail: Sediments are poorly described, text and figures are not focused on the dating approach, the text is too long and with too many figures (10 figures in the text plus 11 figures as appendix). However, I consider this record as interesting and suitable for publication but not for "Geochronology" and not in its current state. I suggest to reject this manuscript and encourage the authors to resubmit a paper about environmental reconstruction based on the Columbine Lake record, which should make use of the high-resolution proxy data based on radiometric dating.

General comments

There are two main issues, where the authors fail providing scientifically sound data and interpretations. (1) The prior assumption that laminations are varves is not supported by the data and the authors themselves provide evidences that this record is laminated but not varved. (2) Some of the data seem to be erroneous or at least very unique and thus would require detailed explanations.

(1) I am collecting a number of evidences from the text that document that laminations maybe the result of seasonal depositional processes for individual cases but do not allow setting up a varve chronology. Clastic laminations can be considered as varves if at least one of the following can be considered as assured, all of which is not available for this study:

- Process understanding – sediment traps and monitoring
- Agreement with other independent dating methods – multiple dating
- Reproducibility – parallel cores from the same position in the lake provide comparable results
- Availability of sediment cores from different years – documentation of additional laminae for the younger core(s)

118f (numbers refer to line numbers): “Core COL17-1 is not laminated, possibly because it was collected at shallower depth, and was not considered further in this study.” As all three cores were recovered from the deepest part of the lake (25-27 m of water depth), this information points to a high intra-lake variability of depositional conditions. Such conditions are not favourable for establishing a varve chronology.

164ff: Thin sections have been prepared for the entire record but no microfacies investigation was carried out. It is evident, that counting laminations on images taken with 2x and 10x magnification from thin sections will not allow to develop reliable “varve” counts, especially if laminations are very thin as reported for this record.

235ff: “...the distribution of the varve thickness is similar in both cores throughout the sections with distinct varves (Appendix A Fig. A3). Furthermore, there is no evidence for systematic changes in the mode of deposition in these sections, as the indistinct sections occur throughout both cores, but not always at the same time and the sedimentary features were mostly the same above and below the indistinct sections.” This is not supported by the data presented. For instance, Fig. 4a documents that the distribution of the four varve types along depth is quite different between cores as well as lamination quality is different between cores. Also, Fig. 6 displays a large amount of heterogeneity between both cores studied by the three observers. Altogether, this points to difficulties in distinguishing seasonal laminae and in discriminating individual varves (years). Fig. 6 also disagrees with 364-365: “Varve quality was generally higher at the top of the two cores (code 1) ... (Fig. 6).” Higher varve quality is only shown for the top of core COL17-2 and only for observer 1. Although described as “original counts” in Fig. 7, the figure shows modelled and integrated varve counts instead. Moreover, there is disagreement between the results of all three observers. As two cores were analysed, original counts of both data

sets should be documented.

394ff: "The observer agreement is high for minimum thickness but low for maximum thickness (Appendix A Table A2). Observers disagreed on the number of indistinct sections, pointing to the subjectivity of varve delineations and confidence levels." If laminations are characterized by high variability in composition between couplets and additionally are frequently interrupted by indistinct laminations and/or homogenous sediment sections, the consequence should be to refrain from developing a varve chronology for such a record.

Independent verification of couplets as annual is not reproducible. "The varve count and uncertainty by all three observers show a high agreement with the ¹³⁷Cs peak, suggesting the couplets are annual. The whole sequence agrees generally well with the radiocarbon profile, particularly in the top 25 cm." (426ff) Despite of this text, no independent evidence is provided, whether the Cs peak is related to nuclear bomb testing or to the Chernobyl accident. Any of them is possible if only one peak is available as it is the case for this record. With regard to the Pb data, it is more realistic that the Cs peak relates to the Chernobyl accident in 1986, as Pb data provide ages of 1996 (CRS), 1998 (CFCS) and 1984 (Plum), with the latter being the most reliable according to the authors. Assuming this to be true, there is no evidence for the annual character of laminations. Also, there is no general agreement of couplet counts with the radiocarbon (Bacon) age-depth model, there is disagreement! This is documented by Fig. 10, which displays that calculated sedimentation rates disagree amongst the three observers and even more with regard to radiocarbon data. Moreover, this is supported by data provided with Tab. A2.

In the discussion, the authors provide a number of arguments that indicate why their "chronology" needed special statistical treatment to become comparable with the radiometric age-depth model. The following arguments speak for themselves:

508-509: "It is also likely that laminations are missing due to erosion."

512-514: "...the uncertainty surrounding the delineation of each varve is likely to be proportionately large because of the image quality and pixel resolution used in this study." Here the question arises, why microfacies analysis of thin sections was not carried out?

518-520: "About 78% of the sediment of COL17-2 and COL17-3 was identified as [varve quality] code 2, 3, and 4, all three designations indicating the observer was less than 80 % certain the thickness delineated was accurate."

526-527: "We find large uncertainty estimates even for the best quality varves in Columbine Lake."

550-551: "...information was filled in by the varve emulator which assumed that varves should be present at that depth." This adds another uncertainty to the record in the case rapid depositional events occurred.

649-650: "...the asynchronicity of the transition in the cores suggests site specific causes (e.g. processes that oppose varve formation),..."

709-711: "...sediment microstructures and the quality of the varve appearance are important sources of uncertainty in Columbine Lake: varves are thin, complex, and their formation mechanism appears to change through time."

All this should have prevented the authors from using these laminated sediments as the base for an incremental chronology. All varved records are laminated, but not all laminated records are varved!

(2) There are a few issues that appear erroneous:

Fig. 4a shows BSi data from 0-105 cm depth. Identical data are on display in Fig. A5 from 0-125 cm depth. Moreover, Fig. 4a shows varve type 1 for "non-varved" unit 5.

Fig. A5 shows organic content (8->20 %) and biogenic silica (0-12 %). As Columbine Lake is an alpine and oligotrophic lake, such relatively high values seem to be quite unusual and at least need further explanation. Furthermore, dry bulk density mainly varies around 0.15 g/cm³, which is very low for clastic sediments. Finally, a water content that varies around 0.9 % is unrealistically low. All this needs to be checked!

Specific comments (numbers refer to line numbers):

48: The sentence "Error sources are associated with (1) inter-site differences in varve counts..." needs to be corrected to "intra-site differences", inter-site differences make no sense here as the site is the lake. The same for 185 and 195.

88ff: Fig. 1a displays some evidences for a delta south of the inflow currently not in use. Furthermore, there is another lake basin in a distance of only ca. 150 m west of

Columbine Lake, which is probably acting as a sediment trap for coarser sediment fractions before they enter the studied lake. All this needs to be mentioned and discussed and might have implications for interpretations.

122-124: "(2 cm measurement diameter resolution)" – please reword, this is difficult to understand.

171ff: "Therefore, we used point counts and length measurements directly on individual grains in the slides. At least 100 grains were measured from the varve transects." Please explain this procedure with other words. As it is now, I am not understanding what has been done.

184ff: in the chapter "Description of the original varve model" the lamination is neither described nor confirmed as varved. Throughout the entire manuscript the presence of varves is regarded as prior but unproven information.

300: Characterization of unit 5 by the grain size clay is not supported by the data (as shown in Fig. 4).

301: Fig. A5 does not show data of unit 5; the same is probably true for Fig. A6. Furthermore, it remains unclear, which data is shown in the correlation matrix.

303-304: This sentence is true for Fe but not for P. Moreover, the drastic decrease of siliciclastic elements needs to be mentioned as well.

323f: The sentence "Some heavy metals (Zn, Ag) also increase to their maximum levels (Appendix A Fig. A4)." is only partly true and questions the interpretation (cf. 679), as Ag has similarly high values at the base of the record.

352: Why is the mineralogy provided only for type 2? However, this data is not used for any interpretation, it may as well be deleted.

364: Std. deviation provided in the text is 0.05 mm and distinctly different from the one provided in Tab. 1 (0.3 mm).

389: "Three observers independently measured the varves..." Here it is necessary to name

those who counted (not only in the chapter "Author contributions")! Are these three experienced sedimentologists or students? Additionally, it is not explained how the varves were counted.

400: Since DeGeer, marker layers are assigned macroscopically (in the case microfacies analysis is applied, this can be extended to microscopic marker layers) to distinct layers or changes in sediment composition to ease the counting of shorter sections of a profile (between individual marker layers) by different observers. I do not understand, why every observer sets up his or her individual set of marker layers in this study.

Before 500: the "Results" chapter very marginally describes and discusses sedimentological and geochemical data as they are shown with Figs. 4a, A4 and A5. Thus, the question arises, why this manuscript is expanded largely by including such data?

564: At the high altitude of the lake, "hydrolysis" (chemical weathering) is certainly of very little importance if at all.

568-569: Please explain the reasons for anoxia to develop in an oligotrophic alpine lake with clastic sediments like Columbine Lake?

644-645: The argument provided here and directly linked to anoxia "...and redox conditions are consistently indicated by the PCA analysis (Fig. 5b)." has to be treated cautiously, as PCA analysis explains less than 50 % of the variance and the suggested interpretation might be a misinterpretation.

Comments to figures and tables

Fig. 4: the positions of datapoints shown in 4b should be marked in 4a and the positions of datapoints shown in 4c in 4b. "Lithozone" is introduced here for a facies description. This is misleading in comparison to the term "units", which I would label as lithozones. Moreover, the lithology of Fig. 4a demonstrates that both cores are quite different. However, similarity is assumed throughout the manuscript. Here it would be nice to see how well the correlation really is by comparing MS data from both cores or using selected XRF data for this purpose.

Fig. 8: c) the x-axis needs to be extended to ca. AD 1750, to show where the data meet

with the x-axis. e) the Plum model needs to be explained. It cannot be understood as it is. d)+f) show calibrated 14-C ages in the topmost ca. 10 cm, data not listed in Tab. 3.

Tab. 1: minimum varve thickness is provided as 40-50 μm . This is just one silt grain and would be uncommonly thin for clastic varves. The unmentioned lake basin in the catchment could be an explanation for this phenomenon. However, this number is just a mere statistical value based on treated lamination measurements...

Tab. 2: Please provide data (values and percentages) of how many "varves" were actually counted and measured and how many modelled.

Tab. 3: is the data of IonPlus 3528 a pMC age? If so, please mention this!

Tab. A1: This table is difficult to understand. Perhaps the depth for each marker layer should be provided?

Fig. A2: Please, add a scale.

Fig. A3: This graphic is difficult to understand – there are three colors but only two explained in the legend.

Fig. A4: What does it mean, if the "Ratio Mn/Fe is normalized to Ti counts"? Is this $(\text{Mn}/\text{Fe})/\text{Ti}$? Usually, it does not make sense to normalize a ratio. Why it is done here needs to be explained under methods.

Fig. A6: For the correlation matrix information is lacking about which data (i.e. depth interval) is included in calculations. I assume, this is only the topmost 125 cm.