Geochronology Discuss., author comment AC2
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Reply on RC2

Stephanie Harmonie Arcusa et al.

Author 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-AC2, 2021

We thank Reviewer 2 for their comments. We agree that the manuscript is generally too long with too much complexity added by the fact that we tried to tackle too many questions: (1) presenting a new Bayesian approach to constructing a varve chronology, (2) extracting as much information as possible from a sub-optimal varve record to inform said chronology, and (3) presenting a new paleoenvironmental record. We agree that separating these questions is a better approach to increase readability. A manuscript focusing solely on a paleoenvironmental reconstruction based on the Columbine Lake record is a future consideration.

However, we disagree with the implication that only sediments with near-perfect varve quality are worth investigating. Indeed, intermittently varved sediments are relatively common and contain extensive chronologic information that is lost if the varves are ignored. This is further strengthened when one extends the scope beyond lacustrine sediment to geological outcrops. The value of this manuscript's work is to provide an approach to extract information from sites with poorer varve quality while still recognizing that lower quality increases uncertainty, which the approach diligently attempts to characterize.

Reviewer 2 suggests that clastic laminations can be considered as varves if at least one of four listed requirements are met, and states that our study meets none. We disagree, and argue the study readily meets two of the requirements:

- *Agreement with other independent dating methods – multiple dating*

This requirement should have the highest priority out of the four listed, and we disagree with Reviewer 2 in the assessment of the data presented in our work. Reviewer 2 rejects the hypothesis that Columbine Lake couplets are annual by citing that it does not match with the Chernobyl cesium peak. A Cs peak related to Chernobyl is extremely rare in the western United States. We are unaware of any examples of such a peak found in lake sediment in the western United States. It was overwhelmingly more likely that the Cs peak is associated with the peak of atmosphere testing in 1963. This date agrees very well with the varve chronology and is the best evidence that laminations in Columbine Lake are annual. Additionally, varve chronology is in close agreement with radiocarbon dating. Our chronology thus meets one of the four requirements.

- *Reproducibility – parallel cores from the same position in the lake provide comparable results*

We collected four parallel cores from Columbine Lake, three out of four showing a high level of reproducibility in visual appearance and sedimentary structure (Figure A1). We agree that when viewed under the microscope, it would be ideal if the microscopic structure of the lamina matched as well, but some amount of mismatch is expected given that the cores were taken from different locations, the sediment was challenging to embed, and the laminations were very small. Given the challenging situation, we argue that our results are in fact comparable across the parallel cores with limited uncertainty.

Given the strong evidence that the most recent laminations are annual, and that the cores document a common depositional signal, we strongly reject the implication that no chronological information is present in the laminations at Columbine Lake, even though the varve quality varies to a degree that would disqualify it from using traditional varve techniques. In fact, this is a major goal of the manuscript, to develop methodologies that extract geochronological information that could not be used with traditional approaches. In that sense, we agree with the reviewer that this sediment sequence would not traditionally qualify for development for a varve chronology. That's a major selling point of our new technique. This strong disagreement does not mean we reject all comments from Reviewer 2, many of which are very important to improve the quality of the manuscript.

We note one major point of misunderstanding, likely stemming from the novelty of our approach to building the chronology and the writing lacking clarity in its explanation of this new method. Our approach does not "count" varves in the traditional sense. It uses distance delineations of couplets that are then used by an algorithm to give a probability of the number of varves for a given depth. We see that the misunderstanding points to the need for revising the text and a revised manuscript will take greater care to introduce this new concept.

RC2 comments

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.

--> Columbine Lake sediment is indeed highly variable but three out of four of the collected cores contain laminations that can be reliably correlated across cores with the naked eye (Figure A1). Core COL17-1 was collected from a shallower depth on the slope of one of the deeper pockets, likely escaping the conditions favorable for varve preservation. It is not unusual for deep lakes to have an anoxic zone. For example, Whitlock et al. (2012) found that the limited surface area, conical bathymetry, and deep water (> 31 m) of Crevice Lake in Yellowstone National Park created anoxic conditions in the hypolimnion and preserved annually laminated sediment.

Whitlock, Cathy, et al. "Holocene seasonal variability inferred from multiple proxy records from Crevice Lake, Yellowstone National Park, USA." Palaeogeography, Palaeoclimatology, Palaeoecology, 331, 90-103.

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.

--> It is not clear what kind of extra microfacies investigation Reviewer 2 refers to or would have liked to see. Although micro-XRF analyses were not an option, point measurements of grain size under x50 magnification were made for several thin sections and shown in Figure 4. The laminations were readily measurable at 10x and 20x magnification.

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.

--> This seems like a point of confusion. Our text says, "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". The Reviewer's comment says, "For instance, Fig. 4a documents that the distribution of the four varve types along depth is quite different between cores". We agree that the cores differ on when the poorer quality can be found, with no evidence for any systematic change. The first statement refers to the overall distribution of thicknesses measured by counters between cores, not where the uncertainty is. Additionally, as much as we tried to objectify the quality assessment, quality codes will remain a qualitative analysis. Furthermore, some observers tend to "lump" varves, while others "split" varves, which even for high-quality sequences is an issue. Nevertheless, there is room for improvement in the description here, and we agree that we must revise the text to correct the other inconsistencies identified by Reviewer 2 regarding Figure 6.

--> Original "counts" for both cores are provided in the Zenodo package. These "counts" are plotted in Figure 6. However, we note and wish to clarify here a misunderstanding. We do not "count" the varves in the traditional sense where an observer counts 1 through n, we delineate them. The VarveR model takes these delineations and creates a count by combining both cores and transferring them to the depths of one of the cores, the master core, in this case, COL17-3, the dated core. So, the term "original count" in Figure 7 is correct for our purposes: the model "counted" the delineations, to create an "original count". Nevertheless, we see the confusion, and a revised manuscript will clarify the distinction.

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.

--> This comment brings us back to our fundamental disagreement with Reviewer 2. The premise of this manuscript is that varved sequences that include indistinct or more uncertain sections still contain extensive information to contribute to a chronology, and reject the premise that a near-perfect varve sequence is required to be useful.

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.

--> We reject the premise of this comment. Whereas a Cs peak related to Chernobyl is evident in European lake sediment (Appleby, 2008), we are not aware of a Chernobyl-related Cs peak reported in lake sediment in the western United States. For example, Colman et al. (2004) collected cores from Oregon and found no evidence. Zhang et al. (2015) collected cores from reservoirs in Oklahoma and similarly did not find a Chernobyl peak.

--> There is also limited evidence for the eastern United States. For example, Lima et al. (2005) found a small inventory preserved in Rhodes Island but Omelchenko et al. (2005) collected fifty-four sediment cores in Eastern Canada and did not find a Cs peak corresponding to Chernobyl. Munoz et al. (2019) collected cores from three provinces of the Mississippi River basin and found no peak corresponding to Chernobyl.

--> Consequently, we did not explain this unlikely possibility, but we can make this clear in the revised manuscript. Because it is overwhelmingly likely that the Cs peak is associated with 1963, we continue to argue that our varve chronology matches well with the radiometric data and serves as independent verification that the recent couplets are annual.

--> However, we agree there is a relatively poor agreement in the sedimentation rates between observers but considering the challenging sediment quality this may not be surprising and the integrated model attempts to quantify that uncertainty by merging all observer's data. It is also difficult to directly compare sedimentation rates derived from the varve chronology to the radiocarbon chronology because each chronology fundamentally estimates different scales of sedimentation, i.e., annual vs centennial timescales.

Appleby PG. Three decades of dating recent sediments by fallout radionuclides: a review. The Holocene. 2008;18(1):83-93. doi:10.1177/0959683607085598

Colman, S.M., Platt Bradbury, J., McGeehin, J.P. et al. Chronology of Sediment Deposition in Upper Klamath Lake, Oregon. Journal of Paleolimnology 31, 139–149 (2004). https://doi.org/10.1023/B:JOPL.0000019234.05899.ea

Lima, Ana Lúcia, et al. "High-resolution historical records from Pettaquamscutt River basin sediments: 1. 210Pb and varve chronologies validate record of 137Cs released by the Chernobyl accident." Geochimica et Cosmochimica Acta 69.7 (2005): 1803-1812.

Munoz, S.E., Giosan, L., Blusztajn, J., Rankin, C., Stinchcomb, G.E. Radiogenic fingerprinting reveals anthropogenic and buffering controls on sediment dynamics of the Mississippi River system. Geology, 47 (3), 271–274 (2019). https://doi.org/10.1130/G45194.1

Omelchenko A., Lockhart W.L., Wilkinson1 P. (2005) Depositional Characteristics of Lake Sediments in Canada as Determined by Pb-210 and Cs-137. In: Omelchenko A., Pivovarov A.A., Swindall W.J. (eds) Modern Tools and Methods of Water Treatment for Improving Living Standards. NATO Science Series (Series IV: Earth and Environmental Series), vol 48. Springer, Dordrecht. https://doi.org/10.1007/1-4020-3116-5_2

Zhang, X. C., et al. "Dating sediment in a fast sedimentation reservoir using cesium□137 and lead□210." Soil Science Society of America Journal, 79.3 (2015): 948-956.

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!

--> Again, we fundamentally disagree with the argument that 1) the laminations are not annual and 2) that the sequence has no value for chronology development.

(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!

--> We completely agree that all the figures need to be checked, we are appreciative that Reviewer 2 was able to identify areas where errors crept in as is expected of the review process. BSI was measured multiple times on the same core, so it is likely that a different dataset was plotted in Fig. 4 and Fig. A5. This will be checked thoroughly for the revised manuscript if the geochemical data is kept, which Reviewer 1 suggested that we remove entirely or provide a stronger rationale to keep. We suggest we will remove the geochemical data.

--> Unit 5 is indeed not varved in COL17-2 and the unit is almost entirely missing in COL17-3. Unit 5 correctly aligns with COL17-2, but the figure may be slightly off for COL17-3.

--> We appreciate the comment about the organic content, biogenic silica, dry bulk density, and water content. Those will all be checked for a revised manuscript, but we will not include geochemical data as per comments from Reviewer 1. It is unclear to us why this would be "unrealistically" low or "unusual" since it falls within the variance of other lake sediments we measured in the area. We will nonetheless do our due diligence and check again.

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.

--> This is noted and will be changed in the revised manuscript.

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.

--> This is noted and will be elaborated on in the revised manuscript where we do note this old inflow on line 565 in the current manuscript.

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

--> This was meant to refer to the diameter of the measuring device imprint. The revised manuscript will clarify.

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.

--> The revised manuscript will revise the explanation. Three transects were virtually drawn longitudinally across thin sections with characteristics of varve type 1-3. We measured the length of at least 100 grains along these transects.

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.

--> The manuscript follows the traditional IMRaD format. The manuscript describes the methods, but for the methods to include the Bayesian chronology development, the methods chapter must assume to a certain extent that the laminations have been shown to be annual, which the manuscript then does in the result. This is a common feature of varve chronology manuscripts. We note the misunderstanding with the title "Description of the original varve model" which contrary to the Reviewer's comment does not refer to Columbine Lake but to the VarveR algorithm (i.e., the model) presented by McKay (2019). In a revised manuscript we will make this distinction clearer.

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

--> Grain size for Unit 5 does not figure in Figure 4 as the dataset stops just before that unit. We do not have quantitative grain size data for that unit. Unit 5 is indeed clay-sized upon qualitative observation.

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.

--> We can clarify which data is used in the correlation matrix, but as per Reviewer 1 we will be removing this data from the manuscript.

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.

--> This is noted and will be changed in the revised manuscript.

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.

--> The values for Ag do hit maximum levels, which are like levels in unit 5. Our interpretation on line 679 states "Finally, mining became increasingly prevalent in the area from the 1800s (Blair and Bracksieck, 2011), although we did not find evidence for mining within the catchment. Mining indicators (e.g. Guyard et al., 2007) such as silver and zinc become abundant in unit 1, and the increase in heavy metals could have changed both lake productivity and signal a change in lithogenic input". We argue that nothing in this interpretation contradicts the observation that unit 5 also had elevated Ag.

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.

--> This will be removed, along with all geochemical data.

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

--> We can check that.

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.

--> The contributions of each author are described in the Author Contribution section, per journal policy. Beyond this, we prefer to anonymize the counters, however, we are willing to better characterize the training and expertise of the counters to provide context for the reader.

--> Regarding how the varves were "counted": on line 195 we state "In this study, thickness delineations were created as ArcMap shapefiles". We can elaborate on this some more as it appears from Reviewer 2's comments that our text is not making it clear enough that we are not "counting" varves.

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.

--> We did assign marker layers macroscopically (Figure 1) as we all as for each thin section. Each observer set up their own marker layers so the model could include an estimate of marker layer uncertainty as well, an important additional source of uncertainty.

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?

--> The revised manuscript will remove the geochemical data.

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

--> Hydrolysis does occur faster with higher temperatures, that is true. But hydrolysis means the breakdown of rock by acidic water. The entire catchment of Columbine Lake is full of naturally occurring heavy minerals which are leaching into the lake as evidenced by the low pH (5, lines 89 and 576) of the lake water. Clearly, water is becoming acidic faster than the temperature would suggest and we argue based on the decomposition of the rocks in the catchment that hydrolysis is likely an important factor.

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

--> Oligotrophic lakes can still be anoxic at the bottom if they are deep enough. In the case of Columbine Lake, it is most likely due to the small, deep pocket at the bottom of the lake that is sheltered from the wind. The weird geochemistry may also play a role. We also found that the sediment oxidized quickly once the cores were opened. It is not unusual for deep lakes to have an anoxic zone. For example, Whitlock et al. (2012) found that the limited surface area, conical bathymetry, and deep water (> 31 m) of Crevice Lake in Yellowstone National Park created anoxic conditions in the hypolimnion and preserved annually laminated sediment.

Whitlock, Cathy, et al. "Holocene seasonal variability inferred from multiple proxy records from Crevice Lake, Yellowstone National Park, USA." Palaeogeography, Palaeoclimatology, Palaeoecology, 331, 90-103.

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.

--> As per Reviewer 1's suggestion, we will be removing geochemical data from the revised manuscript.

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.

--> We can change the terms if it would be more accurate to do so. We can also show the MS and XFR data of both cores for comparison. This will be an important addition to demonstrate why we do indeed assume similarity (in addition to Figure A1 which shows images of that similarity).

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

--> We can extend the x-axis of (c). We can add more text to explain the Plum model. Panel d and f also include 210Pb ages in addition to 14C but we are using the same colors which is confusing. The revised version will make the distinction.

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…

--> Yes, the minimum thickness is very small, but the sediments are predominantly clay-sized. The minimum value is derived from the modeled results but is informed by the thickness measurements. The method does not allow for a minimum value that is smaller than any measured lamination.

--> We comment on the smaller basin in the catchment on line 92 "The lake is fed by a small pond and stream to the northwest and drained by Mill Creek to the northeast".

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

--> We did not "count" varves. Please see responses made earlier to address this point.

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

--> It is not clear what "pMC" refers to. If Reviewer 2 is asking about the date (-935 14C BP) we refer to the table's superscript (c) stating the sample returned a modern age and was discarded.

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

--> The table will be amended to enhance clarity. To explain, for example, marker layer 1 for observer 1 was found at a "depth" or "counts" of 699 in COL17-2 and 660 in COL17-3, indicating a difference of 39 "counts".

Fig. A2: Please, add a scale.

--> This is noted and will be changed in the revised manuscript.

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

--> The third color is the overlapped mixture of the other two.

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

--> As per Reviewer 1's suggestion, we will be removing geochemical data from the revised manuscript.

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.

--> This figure will no longer apply in a revised manuscript.