Responses to reviewers

Thank you to the editor and reviewers 1 and 3 for your constructive comments. We have made changes described below to accommodate your suggestions. We continue to disagree with reviewer 2 about the definition of "varves" and, more importantly, about the utility and validity of our study. Zolitschka et al. (2015) define a varve as any annually laminated sediment. We have tried to further clarify that our study does not claim that every lamination in the Columbine Lake sediments are varves, rather we argue that the clear laminations are most likely annual, due to agreement of the counts of well-laminated sediment near the surface and the ^{137}Cs date, and with the overall agreement with the ^{14}C dates. Although chronological uncertainty remains, we strongly believe Columbine Lake sediments have varves, even though they are intermittently indistinct. We show in the manuscript that these layers are near annual through multiple independent dating methods. Alternative explanations that the laminations are near-annual (but of unknown origin) or that we've happened across what may be the first lake in this part of the world to have a pronounced Chernobyl ^{137}Cs peak seems far less likely than our interpretation that Columbine Lake sediments have varves that are intermittently indistinct. The observation that these sediments do not meet the criteria for traditional varve counting is both clear in the text and the motivation for the manuscript. We have attempted to clarify in section 3.4, lines 147-156 try to address some of the issues brought up in the previous review regarding the definition of a varve. We have amended the text further to explain that the laminations in Columbine do not fit the definition of a varve couplet like those found in lakes with good quality sediment like one of the lakes mentioned, Skilak Lake. We reaffirm in the paragraph that one goal is to develop a method that can use sediment with indistinct laminations to build a chronology.

Reviewer #1

L82: missing subject at the end of the line?

The text was rephrased to say, "Laminated sediment, even when indistinct or intermittent, provides valuable information that can be used to improve chronologies and can provide new opportunities for regions that currently lack records (Ramisch et al., 2020)."

L158: first mention of R is earlier, so reference should be put in there – L125.

The reference was moved from line L158 to line L125.

L276: repetition?

Repeated word "contain" was removed.

L293: Reference after statement about true non-glacial clastic lamina.

A reference to Zolitschka et al (2015) was added to line 293.

L319: I do not follow the last sentence of this paragraph.

The sentence was trying to explain that the algorithm does not allow for values that are smaller than what was measured. The text was changed to make this clearer.

Table 1: While meaning is clear, the heading of column with variables is technically incorrect.

We have changed the heading to "summary statistic" and added a heading above COL17-2 and COL17-3 that says "core".

L442, Fig. 7 caption: No need for “Plum is a statistical…” it is already in the text.

Sentence was removed.

L453, Tab. 3 footnote: Is the last sentence missing something?

Yes, it was not clear. We have changed the sentence to say, “This date not used because it returned a modern age”.

L467: Please consider changing the name, as reader must keep in mind that one integrated model means something different than the other depending on the position in text. Names should me more straightforward and unique.

Good point, we changed it to “multiple observers integrated model” or MOIM throughout the text. We also had to change the legend in Figure 8 so we provide an updated figure.

L515: Is “contain” a correct word choice?

We changed “contain” to “have”.

L561-62: Last sentence should be rewritten.

We changed the last sentence to “We consider this a significant advantage of this approach, as it objectifies the subjective element of observer judgement, puts less emphasis on the observers, and tends to align discrepancies.”

L584: Safer to use “often” than “generally”.

Changed “generally” to “often”.

L599: With this sentence in the text one can expect short explanation and provided reasons why such analysis was not undertaken.

We deleted this as it was not relevant.

Tab. A2: same comment as for Tab. 3. Either way, consistent naming of headers.

We have changed the header to be consistent with Tab 1. We added a row above the core names and changed the header to “statistics”.

Fig. A2: Can you provide scale bars?

Thank you for requesting those. We had to remake the figure from scratch to get the scale bars to show up which is why the images are not the same as in the previous manuscript version.

Reviewer #3

Line 168 - "three" needs to be changed to "four", according to the list in the previous lines

Good catch, “three” was changed to “four”.

Line 179 - Either remove the comma after "probability" or remove both parentheses

Comma was removed.

Line 187 to 189 - I would suggest either removing the numbers in the brackets or saying directly that the laminations are assigned one of six different codes. As I understand it, in your current version of the manuscript you want to introduce the idea of prior probability estimates for over- and under-counting, but for example laminations with code 4 are also assigned a probability for over-counting (reference to line 192).

Yes, exactly. Thank you for providing clearer language that we can use. We have changed the text to now say on line 187-189 “Each lamination was assigned one of six different codes (Appendix A Fig. A2) with a corresponding distribution of over and under-counting prior probability estimate (Sect. 3.5). Codes 1, 2 and 3 are assigned by the clarity of the lamination’s appearance, with a code value of 1 being of higher clarity than a code value of 3.”

Line 210 to 213 - I understand the reason for assigning code 5 and 6 to specific indistinct core sections and then using an emulator. But how can the user influence the modeling process if the indistinct sections are due to a change in the sedimentation process? Would the emulator still try to match the missing section with a surrogate section?

Yes, based on the assumption that sediment processes are stable. If there are changes in the sediment process during those intervals, the emulator would be wrong. We changed the text to make this clearer. Lines 208-211 now say “This approach assumes that the sedimentation processes in these intervals is consistent with the well laminated sections and other laminated intervals can serve as surrogates for indistinct sections. We argue that this assumption is valid for Columbine Lake, as the distribution of the lamination thickness is similar in both cores throughout the sections with distinct laminations (Appendix A Fig. A3).”

Subsection 3.5 (line 215 to 225) - What happens to the laminations labeled 4 to 6 in the varve-only model? I think this needs further clarification on how these laminations are then incorporated in the modeling process.

Yes, good point. Basically codes 4-6 were treated the same in both scenarios. Only codes 1-3 changed. We changed the text on lines 218-221 to now say “In both scenarios, codes 1, 2, and 3 were given over- and under-counting priors, code 4 was given a 50% chance of over-counting and a 0% chance of under-counting, and codes 5 and 6 were simulated using the emulator as described above. Codes 4-6 were treated the same in both scenarios, and only codes 1-3 changed. In the first scenario, the priors for codes 1-3 were symmetrical and based on values found in the literature (Fig. 2a, e.g., Dräger et al., 2017).”

Line 256 - Since you have already introduced Plum, I would shorten the sentence to "[...] that use conventional CRS, CFCS, or Plum (Sec.3.2)."

We changed the text as suggested.

Line 427 - At the end of the line you wrote "model" twice.

One word was removed.

Reviewer #2

This review is about the revised version of the manuscript by Arcusa et al. investigating non-glacial laminated clastic sediments from high-mountain (3874 m asl) Columbine Lake in Colorado (USA). The study focusses on lamination counting in combination with statistical data treatment for the establishment of an age/depth model supported by radiometric dating. The obtained record of sedimentation rates is intended to be used for future environmental studies of this lacustrine archive.

The revision benefits from geochemical and paleoenvironmental data and interpretations being excluded. However, the conclusions drawn are still premature and highly disputable. Unfortunately, the authors did not consider major parts of my previous review, thus they are insufficiently taking care of criticism related to the chronology. I am not repeating these still valid arguments, but add some comments and suggestions that might help the authors to progress with this study.

In general, the dating of clastic sediment sequences is difficult. Not only because organic material for radiocarbon dating is rare or lacking, but also because depositional processes are manifold and especially in high mountain regions dependent on complex and highly variable lake internal (redeposition, sediment focussing) and catchment-

related sediment transfer mechanisms, which are closely linked to snowmelt, melting lake ice, precipitation and (currently not in this case) to glacier melt. Not considered by the authors is the existence of a delta, which indicates that quite some amount of sediment reached the lake, which disagrees with rather low sedimentation rates reconstructed. Moreover, depositional processes are not well understood. For instance, anoxia are suspected for the deep basin (l. 92) as necessary for varve preservation but not supported by any data. However, such conditions are very unrealistic for an oligotrophic high mountain lake with clastic sediments.

Moreover, there is still no proof of concept documenting that the observed layers are annually laminated. The authors argue that the Cs peak agrees with the "varve chronology". But this is weak evidence if at all, as the annual character of laminations is a mere hypothesis, indistinct laminations were interpolated, some cases of discontinuity exist as well as erosion. Thus, Pb dates are a more realistic approach to verify either Chernobyl or 1963 for the Cs peak. Following this approach, Chernobyl is the choice with an age range between CE 1984-1996 based on Pb dating. The argument that no other records with a Chernobyl signal have been reported for North America so far is not acceptable. This may be the first record and the reason may be related to the high elevation of this site?!

The authors argue that their "method does not ´count´ varves in the traditional sense of varve `counting`." But there is no further explanation what is the way they have chosen! As I understand figures and tables, they do count the laminations as done by others before. In line 150-51 it is stated that the term "varve" relates in this study to an algorithm-modelled or algorithm-simulated annually deposited lamination and not to the sediment record itself – thus being a theoretical description for the model output. It remains unclear, however, how the images of laminations are fed into the model and how the algorithm is then able to combine two or more of them to become a varve, i.e. integrating time into the record. Furthermore, this (scientifically inacceptable) discrimination between laminations and varves is not consistently applied. One out of many examples is the title of the paper: "…varve measurements in intermittently varved sediments". In both cases, "varve" is here related to the sediment and not to the model output! In line 212-13 it is suggested that "…the indistinct laminations are due to changes in preservation, not the sedimentation process." – an unsupported hypothesis. Also, the merging of the radiocarbon chronology with the lamination-based age/depth model to form the final output – the integrated chronology – is critical methodologically but also due to a problematic radiocarbon chronology. Explanation: Clastic sediments hardly contain organic matter. Thus, for radiocarbon dating the MICADAS is applied, which was developed to date very small sample sizes. But this easily causes errors, especially if the samples are extremely small (authors did not provide the weight of analysed material) and if samples are a mixture of different organic fractions. Moreover, contamination is a problem as well as reservoir effects (reworked organics from the catchment area and/or the littoral zone are frequent for clastic sediment records as in this case – see Tab. 3) may be present in addition leading to radiocarbon dates being too old and explaining the low sedimentation rates for a clastic depositional system with inflow. Finally, 78 % of the record has a poor varve quality code (according to their nomenclature: this should be a lamination quality!).

In conclusion, if the authors provide no evidence that counted laminations are annual, how can the algorithm reproduce an annually laminated depositional system (l. 253-54)? The "integrated radiometric-varve chronology" (l. 614) is a modification (modelling/simulation) of lamination counts to best fit the radiometric age/depth model. For such an age/depth model the term varve chronology is not applicable. At best it might be called a sedimentation rate-corrected radiocarbon chronology. The quoted sites of Lake Suigetsu and Skilak in the introduction are both dominantly varved sediment records and their annual character was determined convincingly. Lake Columbine with its large amount of non- or intermittently laminated sediment and without any characterisation of laminations being annual does not seem to be a good example to carry out the described modelling approach.