Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-87-RC2, 2020

Interactive comment on "Reconstructing past hydrology of eastern Canadian boreal catchments using clastic varved sediments and hydro-climatic modeling: 160 years of fluvial inflows" *by* Antoine Gagnon-Poiré et al.

Anonymous Referee #2

Received and published: 22 August 2020

General comments:

The manuscript by Gagnon-Poire and co-authors entitled 'Reconstructing past hydrology of eastern Canadian boreal catchments using clastic varved sediments and hydro-climatic modeling: 160 years of fluvial inflows' presents river discharge reconstructions from three short cores containing clastic varves reaching 160 years back in time. For the discharge reconstruction mainly two proxies have been applied (grain size and layer thickness). These data demonstrate the large potential for discharge reconstructions using annually laminated sediments. However, a few week points in the interpretation

need to be better clarified. In general, it is difficult to follow the large number of different statistical correlations between cores, proxies, proxy reconstruction and model results. A more concise approach with a focus on main correlations would make the manuscript easier to read. Furthermore, instead of levelling out the different signals in the three cores by a pooling approach, the causes for these differences should be better examined and documented. The implications of the difference between cores for selecting the most suitable core location for palaeoydrological reconstruction should be elaborated.

The cores have been taken from different parts of the delta surface and even the most distal core location is still 70 m above the deep basin. Sediment reworking processes on the delta should have an influence on the deposition and layer thickness as well as grain size. For example, a thinning of discharge layers from the proximal to the distal delta location (NAS-1 to NAS-2) should be expected, which, however, is not seen in the layer thickness plots shown in figure 6. A more detailed discussion of sedimentological processes on the delta surface should be added for clarification.

The 'anthropogenic impact' after dyke construction (in 1971 or 1972?) has been stressed several times (e.g. lines 444/445). However, it is not clear how exactly dyke construction impacted on the sedimentation. Was the main effect generated by the earth movements during dyke construction (if at all, how long did his effect last?) or by the reduction of the catchment? If dyke construction resulted in 'increased availability of sediments in the river system' as suggested (lines 588-589), why is that only seen in NAS-1 core? Why should there be more sediments in the system although the catchment size decreased? The different behavior of the cores NAS-1 and NAS-2 after 1972 need to be better elaborated. The argumentation that NAS-2 behaves like BEA-1 (lines 598 and following) is not convincing because the BEA-1 location is not affected by the Naskaupi River inflow, whereas NAS-1 and NAS-2 are located in the same direction towards the river inflow. Furthermore, in contrast to DLT, grain-size data do not show major difference between both cores after 1972. How is that explained?

Due to the core differences, post 1972 DLT data of NAS-1 were excluded from statistical analyses? Instead of excluding the data, correlation of NAS-1 and NAS-2 core data post 1972 with hydrological data should be compared. It would be interesting to see how the sedimentological differences affected the correlations with hydrological data.

The proxy data from different cores have been pooled to obtain a better statistical correlation with hydrological variables (lines 630-631). However, pooling masks the different sensitivity of the different core locations in recording natural hydrologicial variability. Moreover, it is not clear if the pooling includes all data from all cores or if some parts of the data are excluded. In line 614 it was pointed out that the post 1972 period has been excluded from one of the cores (NAS-1). If this part of the record is also not included in the pooling approach you put apple and pears in the basket and I wonder about the meaning of improved statistical correlation. Since the BEA-1 and NAS-1 (lines 599-604) are considered to record the 'natural hydro-climatic signal' one should expect a better representation of palaeohydrolgical changes in one of these cores rather than in pooled data from all cores.

The authors report variability on different time scales, i.e. long-term trends in mean annual discharge (line 687) and decadal-scale variability (e.g. lines 56-57) but they do not explicitly relate these. The appearance of variability at different time scales is an interesting finding that should be more emphasized and elaborated in the paper.

The statement about dyke effects on sediment transport and its 'implications for palaeohydrological reconstruction' (lines 703-705) and that dyking effects are 'clearly visible in the sedimentary record' (lines 743-744) are too much simplified. It has been shown that one coring sites has been affected by dyke construction but the two others not or only to a minor degree. This differentiation between core locations is an important point and knowledge about these differences and their causes is essential to select the most suitable coring locations for palaeohydrological reconstruction. In this respect, and here I repeat my previous comment, I do not consider the pooling as suitable approach even if it may improve statistical correlation. Often unspecific terminology is

used like, for example, 'thick and coarse', 'thicker' (examples in specific comments). This should be changed into quantified information.

Specific comments:

A number of 'distinctive marker layers' (labelled A-P, Figure 4, lines 381, 382) have been defined but it is not explained how distinctive these layers are and what makes them distinctive. In figure 4 they do not appear distinctly different neither in the core image nor in the XRF data.

In the chapter 'Regional setting' some information about vegetation cover should be added since that may influence catchment erosion and clastic sediment transport into the lake.

In chapter 4.7 it is not clear which sediment proxies have been compared with the rainfall-runoff modeling approach. Are these proxy data from individual cores (which?) or from pooled data? If it is pooled data, how did you account for differences in TVT between cores?

Line 162: It should be specified which efforts were made to retrieve undisturbed sediment surfaces. Taking short cores from such deep lakes without disturbance is a common problem to the community and it would be helpful to know how the authors tried to improve the coring in this respect.

Lines 185-186: Sampling intervals for Cs-dating are unclear. Was it attempted to sample individual varves or only sublayers? Sample intervals vary between 2 and 0.5 cm but according to figure 6 layer thickness was > 4cm? Please clarify.

Line 226: Specify 'coarse debris' and quantify grain sizes

Line 227: Explain the PSI. Is this a mean grain size for each lamination? What is 'lamination' in this respect? A varve or a sublayer (which?)?

Line 325: What is 'occasionally'? Provide the number or percentage of DL with sharp lower boundary.

Line 327: Explain ‘non-annual’ for these layers. All three described sub-layers (ESL, DL, AWL) are seasonal, i.e. non-annual. Also quantify ‘thin coarser’. What is the thickness (range or mean) and grain size of these layers? Finally, quantify ‘some cases’, i.e. how many of these layers did you count?

Lines 328-329: Provide information why Ca and Sr are relatively higher in DLs, i.e. which minerals in the DLs include these elements?

Line 344: ‘thick and coarse’ is unspecific. Provide information about thickness and grain size of this prominent layer. Are there distinct differences also in the elemental composition of this layer?

Lines 349/350/351: the ESL of pre-1972 CE is ‘thicker’. Provide quatified information instead of this unspecific information. It should be easy to calculate mean contribution of the ESL (in %) to the total varve thickness for the pre- and post-1972 intervals

Lines 350, 352: ‘post-1971’ or ‘post-1972’?

Lines 372/373: When exactly was the anthropogenic change in the catchment? Was it in the year before the 1972 marker layer or in 1972? If it was in the year before, why was there a 1 years delay in the sediment response?

Figure 6. Add the position of marker layers A-P in the figure.

Lines 414 and following: How is the P99D0 value influenced by the ratio DL/TVT?

Line 550: How often is ‘seldom’? In how many layers erosion traces have been observed.

Line 550/551: What kind of traces of erosion are these. Provide a description. I would expect differences between the proximal and distal cores. Please clarify.

Line 580: I disagree that river sediment input was ‘quantitatively and spatially con-

stant’ before 1971. There is distinct variability at different time scales in the data, e.g. between 1920 and 1960s.

Line 602-604: It is assumed that ‘natural hydro-climatic signal’ drives the sedimentation in BEA-1 (and NAS-2) without saying what this ‘natural hydro-climatic signal’ is. This statement should be easy to be proven or disproven by correlation with instrumental hydrological data.

Line 634: You will get at best a regional hydro-climatic signal but certainly no global.

Line 642: Quantify ‘slight variability’

Line 648: How do you explain ‘high thickness values’ (need to be quantified!) of ESLs and AWLs during the 1920s?

Lines 675-677: There is a detailed discussion on thresholds and flood amplitude reconstruction in Kaempf et al., 2014 (J. Quat. Sci.) that you may consider including in this part of the discussion.

Technical corrections:

Lines 328-329: ‘abundance of elements’. This is wrong because XRF scanner data are relative variations of element intensities but not quantified amounts

Line 547: instead of ‘underlying’ it should be ‘overlying’

Line 571 (figure caption): see comment above, XRF data does not give ‘abundances’. This are relative changes of element intensities

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