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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.

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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.

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Reply: We thank reviewer #2 for his positive comments on our manuscript.

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.

Reply: We have indeed tried to reconstruct streamflow using single core data and all possible core combinations. However, statistical analysis of these reconstructions shows poorer results (un-significant p values, negative average reduction of error (RE) and negative average coefficient of efficiency (CE) (values > 0 are needed to validate the twofold cross-validation technique). The pooled data from the 3 cores (mean DLT series and mean P99D0) are the combinations showing the best statistical results (calibration and validation).

We used pooled data from 3 cores in order to better capture the regional hydroclimatatic data, and also to somehow remove the noise that is inherent from the analysis of the tiny part of a single core in a very large lake. We do not believe that selecting a single "most suitable core" for paleohydrological reconstruction is the right strategy because a single core will be more sensitive to local disturbances and is probably less representative of the entire hydrogram.

One of the main goals of the paper is making the demonstration that Grand Lake sediments record a regional hydroclimate signal, not only to reconstruct the Naskaupi river hydrogram. We will clarify this in the revised version of the manuscript. Nevertheless, we agree it would be useful to include in the revised version of the manuscript a better explanation of the causes of the differences between the cores.

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.

Reply: Thank you for this suggestion, we will add a discussion about the sedimentological processes on the delta surface, although core NAS-2 is no longer on the delta itself. We will locate the NAS-1 coring site on the 3.5 kHz subbottom profile of the Naskaupi River delta on the Fig. 1C to help visualize the Naskaupi deltaic context and feed the discussion on sedimentological processes. Yet, there is a thinning of the detrital/discharge layers between NAS-1 and NAS-2, although quite small indeed. The mean DTL thickness of both cores will be added. It is clearly visible on Figure 4. In the context of this very large lake, the distance between the 2 cores is quite small, so we are not surprised to see such a small difference, especially considering that the laminations are still formed at the very end of the end (+/- 45 km away) and can be correlated with laminations from the proximal zone. The grain size is also finer in NAS-2 compared to NAS-1. The median grains size of both cores will be added.

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 1972need to be better elaborated. The argumentation that NAS-2 behaves like BEA-1 (lines598 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?

Reply: We are quite surprised by these comments. Section 5.2 answers most of these questions: for instance, the reviewer question, “Why should there be more sediments in the system although the catchment size decreased?”, was answered in lines 585-589: “The reduction of nearly half of the area of the Naskaupi River watershed reduced the water inflows and changed the base level of the downstream river system. The rapid base level fall must have triggered modifications of the fluvial dynamics such as channel incision, banks destabilization and upstream knickpoint migration, likely increasing the availability of sediments in the River system.”. Maybe the arguments were not enough clearly outlined, and we will make sure to improve the clarity of that section. What is certain is that the varve structure in both NAS-1 and NAS-2 cores changed after 1972, and we will emphasize that feature in the revised version of this section 5.2.

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.

Reply: As mentioned earlier, we tried to reconstruct streamflow using single core data and all possible core combinations. Maybe could we outline this in the supplementary data in order to keep the manuscript as simple as possible, focusing on the main arguments as suggested by the reviewer.

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 palaeohydrogical changes in one of these cores rather than in pooled data from all cores.

Reply: Well, our text in lines 599-604 explains that BEA-1 and NAS-2 (not NAS-1) are considered to record the ‘natural hydro-climatic signal’, i.e. without the influence of the dyke. So maybe there is some sort of misunderstanding here.

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.

Reply: Yes indeed, this is an interesting finding, but this theme will be exploited in an upcoming paper from the same site with a longer and even more interesting record. Unless the editor wants us to expand on this, we would like to hold that information for the time being.

The statement about dyke effects on sediment transport and its ‘implications for palaeo-hydrological 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

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

Reply: We agree to improve the text related to the explanation of the dyking effects, and augment our discussion about the differences in sedimentary processes occurring in the coring sites. We will make our terminology more specific, and change it in 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.

Reply: An explanation will be added.

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.

Reply: We will specify what is the vegetation of the High Boreal Forest ecoregion.

In chapter 4.7 it is not clear which sediment proxies have been compared with the rain fall-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?

Reply: We will specify that it is from pooled data, and we will provide the comparison for each core in a supplement in order to keep the MS simple.

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.

Reply: This will be specified.

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.

Reply: This section is confusing and will be clarified.

Line 226: Specify ‘coarse debris’ and quantify grain sizes

Reply: This will be done.

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?)?

Reply: This will be done.

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

Reply: This information will be added.

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?

Reply: This will be explained.

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

Reply: Allochthonous lithoclastic materials that composed the DLs are rich in Ca and Sr. These elements come mainly from eroded sediments of the Grenville geological province (i.e. plagioclase, granodiorite?) deposited in the Grand Lake’s watershed during glacio-marine/lacustrine phase and remobilized by spring floods. We did not

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perform EDS analysis.

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?

Reply: This section will be clarified.

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

Reply: This will be done.

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

Reply: This will be clarified.

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?

Reply: On 28 April 1971, by closing a system of dykes, the headwaters of Naskaupi River watershed were diverted into the Churchill River hydropower development. The base level fall must have triggered modifications of the fluvial dynamics such as channel incision, bank destabilization and upstream knickpoint migration during the rest of the year. We interpret that it was only during the following spring flood (1972) that the destabilized sediments (during the previous year) were the most remobilized and deposited on the Naskaupi delta. This section will be clarified.

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

Reply: This will be done.

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

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Reply: There is a significant positive correlation ($R2 = 0.38$ p-values = 0.01) between DL/TVT and P99D0. A lamination with a high LDL / TVT ratio is more likely to have high grain size values. However, this correlation shows that DLT and P99D0 remain independent variables and can both reveal different information (i.e. Q-mean and Q-max).

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

Reply: This will be clarified.

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.

Reply: This will be clarified.

Line 580: I disagree that river sediment input was 'quantitatively and spatially constant' before 1971. There is distinct variability at different time scales in the data, e.g.between 1920 and 1960s.

Reply: Reviewer is right, this statement is confusing, we will be more specific.

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.

Reply: This will be done.

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

Reply: Yes, reviewer is right, that will be changed.

Line 642: Quantify 'slight variability'

Reply: The variability will be quantified.

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

Reply: This will be quantified. Hypotheses will be provided.

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.

Reply: We are going to consider including this information.

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

Reply: OK

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

Reply: OK

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

Reply: OK

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