December 11, 2009

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Dear Drs. Pilon and Colosimo:

Subject: Response to IRG Review of Chapter 4 of the Draft Report "St. Clair River Sediment Regime

This is the Study Board's response to the comments received from the IRG on Chapter 4 of the St. Clair Draft Report. The Study Board's responses are captured within the peer review comments with blue-colour text.

> The Study is thankful for all the reviews from the peer review process. Unfortunately, this review came after repeated requests and too late for it to be integrated into the final report. The Study was pleased with the overall evaluation to be acceptable for this Chapter, but was surprised by the nature of some of comments. The Technical Working Group leads were helpful in formulating the responses to the review comments.

Much of Chapter 4 is based on reports that I did not review, including Morris (2008), Foster and Denny (2009), Best (2009) and Krishnappan (2009). Thus, my review of Chapter 4 is based on what is presented in the Chapter itself. I assume that the methods and analyses of the individual reports were reviewed by others previously.

> All constituent reports feeding into this chapter have gone through internal peer review for content and scientific correctness and interpretation. Three reports originating in the USGS offices by Dave Bennion (2009), Dave Foster and Jane Denny (2009) and Kevin Oberg and Jim Best (2009) have all undergone external peer review conducted by their agencies prior to their publication. Yet another report by Xiaofeng Liu and Gary Parker (2009) was peer reviewed through this Study process. All of these reports were available to the reviewer on the Study website.

1) P58, Sec 4.2.1. It is stated that the RTK post-processing was applied to the survey, which yielded sub-m accuracy. Why only sub-m? RTK systems generally yield cm accuracy, particularly if dual frequency receivers are utilized.

> Sub-meter is sufficient for the geological framework.

2) P59. Refer to Figure 4-2d when discussing gravel-tongue features.

Figure referencing has been improved throughout the chapter. The tongue-like features are better addressed on page 69 and refer to better and more appropriate graphics in Figure 4-9.

3) Throughout the chapter it is assumed that the reader is familiar with site locations on the river (e.g. Blue Water Bridge). Presumably, these are described and mapped in earlier chapters of the report.

Correct. In Chapter 2, Figure 2-4 locations are listed for the various features mentioned in the different chapters of the report.

4) P66.First paragraph. It is stated that the MINIMUM vertical error for the 2008 survey was 0.15 to 0.20 m. It is odd to state a minimum error. Some surveyed locations may have been exactly correct. Does this actually mean a minimum estimate for the mean error? Please clarify. In the subsequent paragraph it is stated that the maximum error in 2008 based on replicate surveying was 0.25 m. It would be useful to provide the distribution of errors, which could then be used to assess the previous statement of 0.15 to 0.20 m.

The term minimum vertical error is prefaced by "nominal" to indicate the level of error expected out of the 2008 bathymetry. The Study Board agrees with the reviewer some surveyed locations could have been correct. The information in this paragraph comes from several constituent reports and details are provided for some of reviewers observations.

5) P66, last paragraph section 4.4.1. The Bennion (2008) report also included direct comparison of elevation at surveyed points between survey years, not just comparison of interpolated maps from different survey years. This should be stated.

This is a summary chapter pulling out major elements of the findings. Details are in the constituent reports. This information is provided in Chapter 5, Section 5.3.3 (Page 96) where Bennion's point-to-point comparison is noted. Details and numbers are contained in Bennion's 2009 report. This is also noted on page 67.

6) P67. It was useful to learn that the location(s) of continuing bed erosion (0.5m to 1.0 m between 2002 and 2007) correspond to locations of exposed till.

The observation of the reviewer is noted.

7) p68 and Section 4.4 in general. I think this section needs to be rewritten. The paragraph structure is disjointed and information is delivered in a disorderly fashion. More importantly, the intent of this section appears to be to discount the observed change in bed elevation between 1971 and 2007. This is done by asserting that the 1971 survey must have been biased. This argument is made without even stating the magnitude of the observed change in bed elevation

(only figure 4-7 is provided, which shows only locations of erosion and deposition, without quantifying magnitudes). While it may be the conclusion/opinion of the report authors that the 1971 survey is unreliable, the argument is not presented well, and it has the appearance of a whitewash. This is unnecessary and could incite vigorous criticism. Instead, the Bennion (2008) report should be presented more thoroughly. To whit, the observed changes between 1971 and 2007 should be presented and quantified, using both raw data points (Bennion Figure 20) and interpolated maps (Bennion Figures 31-33). The lack of statistical significance of these changes should then be presented and explained, owing to uncertainty in the survey data (Note that I previously criticized the Bennion uncertainty model as excessively conservative. I do not know if the uncertainty model has been revised.) Only then should issues related to the 1971 survey and potential bias be raised, followed by discussion of the consequences for the Study (uncertainty of the results, etc).

> This section has been revised and some new findings (especially from Best *et. al.* (2009)) have been added. Note again that these are summary findings and the details referred to are readily found in the Bennion (2009) report. The intent here was to present the overall picture of what is known of bed elevation changes and how these relate to the present day morphology and sediment transport. There is no attempt to "whitewash" the issue and detailed comparisons are in Bennion (2009) report. The 1971 data remain paradoxical because surveys before (including 1970 navigational chart) and after 1971 show deep pools in areas of the upper river; the bed material has not changed (many charts of various dates, and sampling reports, identify the upper river as coarse-grained and rocky), the bed is generally non-erodible, and bed material flux extremely low (Liu and Parker , and Krishnappan reports), which makes it difficult to explain such large scale and rapid deposition (if deep pools were there before 1971) and erosion. The discussion then focuses on the issue of whether there has been recent (since 2002) bed erosion, on which the data are much more precise and secure and are the basis for understanding the present day morphology, dynamics and sediment transport.

8) As a general comment, figures should be cited to the original sources.

> All figures are derived from the constituent reports on sediment transport studies. As a rule, any graphic obtained from other sources is properly credited. There are a number of these in Chapters1, 2 and 6. Where graphics were generated as part of the contracts for the study, proper credit is given by naming the principle investigator of the Study and in the annex.

9) The river locations of Figures 4-12 to 4-14 should be specified. Also, local flow direction should be stated (the note "top to bottom" is insufficient, as the river does not flow exactly N-S at all locations along the reach).

> Locations are given in the new versions of the figures. Local flow direction is not relevant here – knowing the overall flow is sufficient.

10) Fig 4-15a. It would be helpful to use distinct colour schemes for elevations and morphological zones.

This distinction is clearer in the revised figures.

11) P74. The shear stress estimates employed the depth average Keulegan equation, which requires a ks estimate. It appears ks was assumed to be 0.05 m throughout the reach. However, the surface grain size in the reach is shown to vary from silty-clay to gravel/cobble (Fig 4-4). Clearly, ks is not constant in the reach. The authors could have varied ks based on location when estimating shear velocity. Alternatively, did the authors consider employing the log-law with a semilog-linear fit for vertical profiles of streamwise velocity? While this latter approach is sensitive to noise in the ADCP velocity data (thus averaging is required), it does not rely on a priori estimates of ks.

> The analysis reported in Best *et. al*. made the simplification of using a constant roughness height Ks = 0.05 m for the estimation of bottom shear stresses with the help of Keulegan (1938) equation and the mean flow velocity estimated with the ADCP measurements. Originally, researchers were going to obtain estimates of grain size distribution throughout the river but due to technical difficulties, this was not feasible. However, Liu and Parker also employed Keulegan's equation but with the roughness height Ks proportional to the D90. This sediment diameter was estimated from images of the bed obtained by Krishnappan(2006, 2007, 2008). This approach was only possible on the upper portion of the river, but a comparison of the values of bed shear stress obtained by Best *et. al.* assuming a constant roughness height turned out to be very similar to those computed by Liu and Parker as a function of grain size. The fact that the Keulegan relation involves the logarithm of the ratio between flow depth (H) and roughness height (Ks) implies that order of magnitude variations in roughness height would be needed to effect noticeable differences in estimates of bed shear stress.
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> The possibility of using the lower portion of the velocity profiles measured with the ADCP, suggested by the reviewer, to estimate values of shear velocity with the log-law was explored by Best *et. al*. (2009) but the data were relatively noisy to be able to estimate the slope necessary from a semi-log plot. Mier and Garcia (2009) did use the log-law and near bed LDV velocity measurements in a laboratory flume to assess the critical shear stress necessary to erode the glacial till from a sample obtained by Krishnappan (2008) in the St. Clair River.

12) Fig 4-18. State which flow rate this figure is based on. In Figure 9 of the Liu and Parker (2009) report it is stated to be the "medium flow" condition.

> It would be more appropriate to assign a value (5282 m^3/s) to the "medium" term used. However, Liu and Parker's report provides this information.

13) p75. The last sentence of Section 4.6.1 suggests that observations at higher flows may possibly generate sufficient shear to mobilize bed sediment. However, the subsequent section (4.6.2) shows that model results suggest flows within the range of observed flows are not competent. This should be clarified, and perhaps presaged at the end of section 4.6.1.

> To clarify – erosion is not predicted at any flow level.

14) p78, first paragraph. It is suggested that the median particle size (d50) is the maximum particle size that can be moved by a flow. This is incorrect. Rather, d50 is a good index value for the entrainability of a sediment mixture for a given flow (Wilcock 1992). In other words, you can use the Shileds curve with d50, and this should indicate when the bed will start to mobilize.

> It is not clear that there is disagreement here with what is said in the text. We are using the ideas (*e.g*. Wilcock) referred to in the comment slightly differently. The issue here is: what is the maximum particle size that can be moved by the observed shear stress? In a mixture of sizes, particles larger than D_{50} have a critical shear stress greater than that of D_{50}. Therefore, taking the Shields entrainment curve as representing the critical shears stress for the D50 particle size also implies that this is the maximum particle size likely to be moved at that shear stress. Entrainment of larger particles in the mixture would require a larger shear stress than this.

15) The chapter ends by acknowledging the apparent discrepancy between observed mobile dunes despite insufficient flow competence and lack of observed bedload transport. The suggested mechanism is ship wake. My immediate question is whether or not ship wake can generate coherent dune fields. In hindsight, it would have been useful for the Study to have evaluated the ship wake mechanism more specifically, as it remains an important unknown.

> There has been some follow-up work on this effect, which became apparent as an outcome of these studies. However, ship effects, while possibly driving a low intensity bed material flux in some areas, are not causing general lowering of the riverbed.

The Study Board believes that this independent peer review has greatly help improve the quality and scientific credibility of the final report.

Respectfully,

Ted Yuzyk	Dr. Eugene Stakhiv
Canadian Co-Chair	U.S. Co-Chair