R. Halliday Comments and Study Response

This report is presented as a draft report for public review. Although parts of the report read quite well and explanations of technical material are probably understandable by some non-technical readers, other parts of the report contain very dense writing and high-level of jargon. The report would benefit from an end-to-end edit aimed at simplifying the text. Note as well that the glossary defines many basic technical terms but doesn't define others. As one example, chapter four makes a couple of references to 'binning' – a term that is not defined in the glossary. For the purposes of a public report, binning could be replaced with 'grouping' a more understandable term.

> The final report has undergone a major rewrite with the help of a scientific editor and higher level scrutiny. The glossary was updated with new technical terms as required. For example, the term binning was added.

The draft report includes many figures arising from Excel plots. Because of arbitrary assignment of scales and other quirks of Excel plotting routines, the figures are often needlessly difficult to comprehend. The final report should include figures that are better than Excel plots. Some other figures appear to be place-holders; it is assumed the final report will contain improved versions.

> We acknowledge that graphics in the draft report were of a lesser quality. In the final report, all tables, figures, maps, charts and diagrams have been produced with a consistent layout and to a high quality for production purposes.

This report was prepared before the authors received peer-review comments on various contributing reports. The peer-review comments on contributing reports should be considered during the revision of the draft report.

> All peer review comments received were carefully considered while developing the final report. A three stage approach was used with the peer review and public comments. First the chapter sub-products comments were reviewed and commented upon. Public comments of a scientific basis were reviewed next. This was then followed by the chapter comments review. This ensured that all comments would be factored into the final report.

Specific Comments

Page 19, Table 2-1. If agreed-on figures are readily available it may help inform the general reader if retention times for each of the Great Lakes were included in the table. Presumably, these are in the order of 100 + years.

> Several methods of estimating residence times or retention/detention times are available in the literature that computes for quantity and water quality management, limnological

studies in the Great Lakes. The Study determined that it was not essential for addressing the current Study objectives and therefore did not present these calculations. If there is a need for this in the next phase, these will be estimated.

Page 20, Section 2.1.2. I assume IGLD (1985) is referenced to mean sea level at Pointe-au-Père rather than to an earth-centred datum such as WGS 84. I would like to assume that the GPS-based data acquisition described later in the draft report takes this into account, but this should be indicated. The horizontal datum, presumably NAD 83, for the study should also be specified.

The review is correct that the Study used IGLD (1985) and is referenced with respect to a zero at Pointe-au-Père. All horizontal referencing is to NAD 83. Any mapping, bathymetry that the Study used, for example, 1971 bathymetry of St. Clair River was converted to NAD 83 from NAD 27 using the Michigan State Plane Southern projection. This is referenced in the work for 2007 bathymetry report by Bennion.

Page 23. Lake St. Clair, second paragraph. Indicate if there any significant flow from Lake Huron via the Black River to the St. Clair River? Note depiction in Figure 2.2.

The Black River feeder canal was investigated by the Study and determined to be not significance in terms of the transfer of water around the City of Port Huron. No flow records exist for the Black River.

Page 35, Figure 2.6. It would be preferable if the figure read from left to right. In the note under the figure, a minimum time period of 33 years over which water level scenarios should be evaluated is mentioned. This could be based on the 32-year cycle plus one year. However the uncertainty in the 32-year cycle given in the text (page 34) is +/- 6 years. Treatment of apparent cycles in hydrologic records is fraught with problems. In this specific case, based on the information provided, a 40-year time period would be appropriate as the round number is greater than 32 + 6 and does not imply any unwarranted precision.

The figure was extracted from a paper by Baedke and Thompson (2000). The objective of using the graph was to look at the paleo-hydrology for a longer historical period. The graphics were used primarily to illustrate the historical water levels in the Great Lake have fluctuated greatly over time.

Page 46-47. Note that the paragraph at the bottom of page 46 is worded in terms of NBS while the ‘equation’ uses NTS. Although these terms have been defined previously, this dual usage needlessly complicates the discussion. One possibility would be to use NBS in the equation and show the inflow/outflow terms separately.

The equation graphic was replaced by a table that accounts for the contributing factors. The usage of the term NBS was also corrected and now it reads NTS to be consistent.

Page 69. Chapter 4 is particularly well written. The text does, however, refer to many features, *e.g.* Sarnia Water Works, Casino, that are not shown on any map in the report.

Figure 1-2 depicting the map of the St. Clair River study area was updated to show all the referenced geographical features.

Although there are implications elsewhere in the draft report that the river channel shoreline is protected against erosion, it would be useful to include an explicit description of the nature of the shorelines of the St. Clair and Detroit rivers, existing protective works, and any potential for erosion.

While the report does not make a general assessment of the impact of shore hardening in the St. Clair River, there are cases where local features that may have caused morphological changes are discussed. The impact of the peerless seawall south of the Blue Water Bridge was added in Chapter 4 addresses this issue.

Page 80. Paragraph three. Was there any overall bias between the two surveys?

General observations and assessment of bias between surveys in 1971 and 2007 were reported by Bennion and this information was used by Bruxer and Thompson while conducting uncertainty analysis. For example, qualitative observation of shallow bias of earlier data was one bias that was noted. What is referenced in paragraph three deals with two surveys with the same equipment and crew few days apart. No bias was detected in this case.

Page 85. Figures 4.9(b&c). Could show the locations of these two in 4.9(a). Also, 4.9(a) shows IGLD85 elevations while 4.9(b&c) do not.

These figures have now been updated.

Page 90 (first line) and 92 (middle). It would be preferable to not to refer to 'vertical velocity profiles' in this manner especially given the discussion elsewhere in the report of concerning vertical velocity components. Use velocity distribution in the vertical or similar wording. Also it would be more appropriate to refer to a Price <u>current</u> meter.

The wording was revised.

Page 92. Footnote. Change to read, "Pascal (Pa) is the SI metric measure …"

The text was revised.

Page 104. typo, "uppermost"

The text was revised.

Page 106. As for an earlier comment, simply refer to a Price current meter in paragraphs one and two.

The text was revised to use this terminology.

Page 106. Discussion of ADCP measurements. Note that the plot of measurements in Figure 5.1 (page 107) shows an apparent bias in the ADCP measurements versus conventional measurements. It would be useful to discuss any apparent bias in ADCP measurements in this section. It would also be worth noting later in this chapter, say on page 125, that the change in conveyance determined by the models is in the same order as the uncertainty in a single measurement of discharge. This may provide a context for the modeling challenge faced by this study.

> The intent of Figure 5.1 was to show the comparison of the time taken for discharge measurements by the two technologies. While the objective of the figure was not to compare or estimate the bias, the uncertainty bands about the measurement lines for the two techniques would greatly overlap. Furthermore, as seen from the variations in the water levels through the day, a significant unsteadiness was present in the flow that would impact on the conventional estimate.

Page 107. Last paragraph. It may be more correct to state the AVM technology is new to the St. Clair River. It's actually a more mature technology than the ADCP. See Halliday (1975) for example.

> The reviewer's expertise in this area is acknowledged and we have noted that point.

Page 109. Section 5.3.1. The eight gauges operated on the river are identified. It would be useful to describe the nature of these gauges. Do they all consist of float actuated instruments? How often do they record? Are the stilling wells all the same size? Given that the stream velocity can be as high as 2 m/s, what precautions have been taken in the configuration of the intake pipes to reduce drawdown due to velocity?

> While agreeing it is good information to include, the Study concentrated on the describing generally the information that was required to address the science questions. This relevant information is available from the operating agencies, NOAA and CHS and the investigators are aware the limitations.

Page 125. Last line in paragraph below figure. The numbers in parentheses are ft^3, not m^3.

> This correction was made.

Page 130. Last paragraph of 5.4.6, second last sentence. An 'annual discharge' should be a volume. Is 5500 m^3/s the mean discharge?

> The text was revised.

Page 132. Ice jams. The discussion concerning the possible effects of the 1984 ice jam on conveyance need to be reconciled with the discussion at the top of page 128 concerning the quantity of material that could be removed without changing conveyance. Assuming that the exact location of the ice jam is known, it should be given. Knowing the location, is there

anything unusual about the pre-and post-jam bathymetry. Note also the last sentence of this section indicates there were no velocity measurements while page 99 says velocities may have been up to 1 m's.

> The Study carried out two special projects during this past summer. The first was a qualitative assessment of the 1984 Ice Jam by Dr. Spyros Beltaos of the National Water Research Institute, Canada. The second study was a state-of-the-art numerical modelling of the 1984 Ice Jam using a 2-D approach by Dr. Hung Tao Shen of Clarkson University. Both assessments along with the earlier modelling provided an assessment on the possible effects of the 1984 ice jam. These assessments are integrated into the final report.

Given that this section is speculative, would the authors care to speculate on the potential effects, if any, of the record high flows in 1986-88 on changing conveyance?

> Based on the 1984 ice jam assessments it was determined that this event was not the primary cause of conveyance change. The report now points to a number of factors including the record high water levels as being contributing factors.

Page 134. Line 8. As indicated earlier, Figure 5.1 does not appear to support the assumption that the two methods produce similar results.

> It is noted here that the comprehensive testing of the two technologies (conventional and ADCP) provided very similar result and did not exhibit any bias in the Niagara River.

Page 135. Could note that the standard error is not dissimilar to the conveyance difference. In the first sentence below the figure there is a syntax problem.

> The text has been revised.

Page 140 – 5.6.1 line 13. Change to read, "lakes Michigan-Huron and Erie are <u>each</u> considered …"

> The text has been revised.

Page 149. Chapter 6 is covered by other reviewers.

> Noted.

Page 184. Confident. Although the draft report discusses the possible effects of the 1984 ice jam, it does not discuss the possible effects of extreme high or extreme low water on changing conveyance. It seems out of place to introduce these notions in this section of the report.

> The text was revised to address this observation as noted above.

Page 185. Hydroclimate. It is understandable that, given the wide range in values, one could be 'confident' in the hydroclimate component. There is no uncertainty band given for the figure and the wide range makes the 'confident' classification somewhat unsatisfying.

> The graphic that is referred has been removed. The text on the different levels of confidence remains and illustrates the qualitative nature of the assessment.

Page 188. Conclusions. The report is careful to identify science questions at the beginning of each chapter. As part of the Conclusions, or perhaps in a separate Summary, it would be useful to provide a short response to each science question. This response should also be part of the Executive Summary.

> Agee. The report has been significantly modified to capture this.

Page 192-93. Legacy Recommendations. The authors may wish to develop specific recommendations concerning improved understanding of the hydraulic regime during ice and weed conditions. For example, development of a contingency plan in the event of a significant ice jam. This could include monitoring plus post-jam detailed bathymetry.

> The Study, while acknowledging that the Great Lakes are one of the better-monitored systems in the world, identified areas where new stations will greatly enhance the accountability of the water budget. The first in this direction are the four hydrometric stations and two evaporation measurement instruments. The second phase will also be looking at adaptive management needs in the basin these monitoring requirements will be re-visited.