Authors' responses to Colin Rennie's review comments on the report: Holtschlag, D.J., and Hoard, C.J., 2009, Detection of conveyance changes in St. Clair River using historical water-level and flow data with inverse one-dimensional hydrodynamic modeling: U.S. Geological Survey Scientific Investigations Report 2009–5080, 39 p. (http://pubs.usgs.gov/sir/2009/5080/pdf/SIR20095080.pdf). Review comments are in black font and authors' responses are in blue font.

The authors have presented a thorough statistical analysis of inverse HEC-RAS modeling to evaluate conveyance changes in the St Clair River. The results indicate that reaches at the first channel bend downstream of Lake Huron have undergone increased conveyance since 1962, and that conveyance of reach DP-PE in this bend has increased with respect to all other reaches. This is where dredging and scour have changed the channel section since 1962 (Bennion 2007). Furthermore, conveyance in the river as a whole has also increased. The results appear to be sound, but I have the following comments.

P26 A single bathymetry was used for all years, thus changes in conveyance were parameterized using an "effective roughness". In other words, changes in both channel section and roughness were parameterized by changing Manning's n. This is clearly stated in the conclusions, but the authors should make this explicitly clear earlier in the report (e.g. p.26). This lumping of channel section and roughness is convenient because multiple model bathymetries need not be created. This is particularly important given that survey data are not available for every year between 1962 and 2007. However less information is derived from the modeling, because it is not known whether changes in section or roughness are causing the change in conveyance. Furthermore, unrealistic Manning's n values were sometimes predicted. The high effective roughness for reach DP-PE in the 1960s (n~0.4) is likely because the 2007 channel section is larger than it was in the 1960s. Particularly unreasonable effective roughness values were predicted for reaches which did not converge to unique values (Table 7, p51). Have the authors verified that their "effective roughness" approach yielded stable model results? In particular, were water surface profiles reasonably continuous between sections? Possibly, better model results could have been obtained if channel sections for a given year had been input, and roughness calibrated to match water levels during that year.

The authors define "effective roughness" in the Purpose and Scope section on page 4. Paragraph two of this section states:

> Channel-roughness characteristics, which cannot be measured directly in the field, are only one component affecting conveyance that also includes channel cross-sectional area and hydraulic radius. Historical measurements of bathymetry and shoreline conditions, however, are not sufficient to fully characterize changes in cross-sectional area or hydraulic radius during the study period. Therefore, any systematic changes in estimated conveyances are associated with the combined effects of changes in channel-roughness characteristics, cross-sectional area, and hydraulic radius. Although nominally attributed to channel roughness, the methods used in this study do not provide a basis for identifying which component or

> components affecting conveyance may be changing with time or location. In this report, the term 'effective channel roughness' refers to the combined effect of physical channel-roughness characteristics, changes in cross-sectional area, and wetted perimeter from those described based on a bathymetry survey in 2007. Effective channel roughness is expressed in units of Manning's "n."

The authors state in the second paragraph of the section entitled "Inverse 1-D Hydrodynamic Modeling of the St. Clair River (p. 10) that:

> Both water-level and flow data are needed to estimate unique values of effective channel roughness when water-level data are used as boundary conditions. When too few flow measurements are available, parameter estimates are highly correlated and depend on arbitrary, initial parameter values. This dependency is not a result of the inverse modeling approach, but is revealed in the output statistics. Highly correlated estimates of effective roughness parameters cannot reliably be used to infer reach conveyance.

The test for unique convergence was used to identify those reaches where there was insufficient water-level and flow measurement data to support estimation of effective channel roughness. So, any effective roughness values that were estimated, but that did not uniquely converge, were interpreted as uninterpretable with respect to conveyance change. The range of reasonable *effective* channel roughness values may differ from reasonable channel roughness values because *effective* channel roughness values were used as a measure of conveyance rather than surface roughness characteristics traditionally indicated by Manning's roughness factors.

Model results were stable in all respects for simulations in which parameter estimates converged uniquely. It is not clear what "better model results" might have been. Because the true annual channel cross-section geometries were not measured, it was not possible to input the information. If this information had been available, it would not have been possible to uniquely estimate surface-roughness characteristics because the water-level and flow data needed to support their estimation were not available. If the measurement data and annual channel geometries had been available, one could have estimated surface-roughness characteristics, although this was not the phenomena of interest. In particular, conveyance, as indicated by *effective* channel roughness, was the phenomena of interest and it was uniquely estimated when and where supporting measurement data were available.

P35 It appears that gauge datum values are in disagreement, given that negative water surface slopes were measured in the 2.4 km long PE-BR reach. The discrepancy may have arisen because the PE and BR gauges are operated by different agencies in different countries. The authors correctly point out that analysis of conveyance change is sensitive to errors in datum values for each gauge. Did the authors attempt to rectify gauge datum discrepancies? Furthermore, were gauge datum values stable over the study period?

As the authors indicate in the third paragraph of the Purpose and Scope (page 5):

This study was part of an integrated, multi-agency analysis of water levels and flows on the St. Clair River. In particular, the water-level and flow data were compiled and adjusted (…by other agencies). The data and models developed by other agencies were used without modification, except as indicated.

Rectifying gage datum discrepancies and determining the stabilities of gauge datums over the study period was conducted by agencies more directly involved in the collection and distribution of this data than the USGS. It is the authors' opinion that this study component was completed as fully as possible given the limitations of the source data. Any unresolved anomalies were identified in this report, as needed, although the resolution of any problems was beyond the scope and resources of this investigation.

P31 It appears that flow resistance was considered constant for a given year. Flow resistance is known to vary with flow stage, but this does not appear to have been considered directly in the modeling. Admittedly, the authors did use statistical analysis of the measured data to consider the influence of Q on conveyance ratios (Fig 8, p46). A more direct approach to assess conveyance changes while considering the influence of Q would have been to plot available Q data as a function of both S and year. Such a plot would demonstrate temporal changes in discharge for constant head (ie conveyance change), as well as changes in Q as a function of head during a given year. The latter should display a 0.5 power relation, and any deviation from a 0.5 power relation can be interpreted as the influence of Q on conveyance.

The possible seasonal variability of effective channel conveyance could not be estimated by use of existing data. In particular, because of the difficulties in mobilizing a measurement campaign, historical flow measurements tend to be clustered into a limited number of events per year. The clustering limits the temporal resolution of estimation. In St. Clair River, flow (Q) is a function of water level (WL) and fall (S?), but is also unsteady, so the suggested graph may have been noisy, but perhaps interesting.

p17 The overbank roughness values in the standard HEC-RAS model are very low (assuming the overbank areas are vegetated). Were the overbank areas inundated in any of the model runs? If so, overbank flows may not have been simulated well (overpredicted), in which case conveyance estimates for the main channel may have been underpredicted. However, this possible error would have been consistent for all years, thus it likely would not influence the study conclusions.

The study authors did not modify data or models developed by other agencies (see Purpose and Scope) unless there were sufficient data to support this modification. Water-levels on St. Clair River seldom exceed bankfull, so the opportunities to measure these events is quite limited. The authors are not aware of any measurements in the overbank areas. The authors agree with the reviewer that this possible source of variation would not likely influence the conclusions of the study.

p20 Longitudinal water profiles as a function of discharge are only parallel if flow is uniform for all discharges. Was this the case? Flow profiles are more likely to be nonuniform at low flows.

The authors claim (first paragraph in section entitled "Water-Level Fall between Gages") that "For various flow magnitudes, longitudinal water-surface profiles tend to be parallel. Thus, water-level differences between gaging stations (the fall in water levels) are more nearly constant and less sensitive to changes in flow magnitudes than water levels themselves. In this report, trends in water-level fall between gages were used as an indicator of trends (in conveyance)."

The authors acknowledge that flows are not strictly uniform on St. Clair River under any flow condition, nor or they strictly steady because of variable winds affecting water levels on lakes that form the boundaries of St. Clair River. They do not consider, however, that this small discrepancy undermines the results of the analysis.

P28 Was eight hours sufficient to eliminate temporal correlation in water level time series? Given the generally steady flow in the St. Clair River, this would be surprising.

In the section entitled "Nonlinear Regression" (p.11), the authors state that the potential for temporal correlation in the water-level residuals (rather than the water-levels themselves) was reduced by sampling at 8-hour intervals rather than hourly intervals. Clearly, the autocorrelation of water-level residuals (or water levels) would be less at eight-hour intervals than at one-hour intervals. Moreover, because simulated water levels explained about 98 percent of the variability of measured water levels, the autocorrelation of the residuals would be less than the autocorrelation of water levels themselves. Quantifying the autocorrelation characteristics in either water levels or water level residuals was made difficult by nonstationarity apparent in both series. In the water-level residual series, seasonal level changes in residuals were sometimes apparent. The level changes were thought to be associated with seasonal affects of vegetation retarding flow. If periods of level transitions were avoided, however, the autocorrelation function formed from 1000+ sequential values in a subseries *g*enerally showed low correlation (~0.1) at lag 1 (8-hour intervals) and no significant correlation at greater lags. This amount of correlation in the water-level residuals is not thought to substantially influence the conclusions of the study.

P29 The parameter SOSC is not defined.

In the section entitled "Parameter Convergence" (page 11), the text indicates that "the fractional decline in the SOSC, here S(b)…" is intended to convey that SOCS (the parameter) is the same as S(b) which is defined by equation (8). SOSC can differ from S(b) when prior estimates of the parameter vector b are prespecified. The authors apologize for any confusion that may have resulted.