Tolson Response Document to all review comments related to April 2009 report.

Bryan Tolson
Nov 7, 2009

Original review comments are numbered and copied and pasted below. My responses are in italics below each number. When I reference a page/location in my report, it is for the revised report (Nov 7, 2009).

Comments and Responses: Brian Barkdoll Subproduct Review

1. Please provide a table of contents.

   Done.

2. Please include an executive summary.

   Done.

3. Please provide a list of assumption of CGLRRM and their applicability to the current modeling effort.

   Unfortunately, this list would be too lengthy to include in the current document. Instead, the references to Clites and Lee (1998) and Quinn (1978) are original publications on CGLRRM model.

4. In lines 57-58 it is stated that "The Chicago diversion was taken to be a constant 91 m3/s over the simulation period." Justify this.

   No longer assumed constant. Time series of co-ordinated diversions for Chicago is now used in model.

5. In lines 82-84 it is stated that "Component NBS is net inflow to a lake and is calculated as NBS = over-lake precipitation + runoff into the lake – over-lake evaporation." Also in lines 90-91 it is stated that "Therefore, this report focuses as much as possible on using component rather than residual NBS values for driving the model." Since the analyses will depend on over-lake precipitation, runoff into the lake, and over-lake evaporation, explain where you get these data and how accurate they are. They seem difficult to quantify, especially over large lakes such as are being studied here. Discuss the possible effects on the accuracy of the modeling effort.

A reference to other parts of the study re NBS estimation is now provided on line 221 of report and reads as follows:

"See chapter 6.1.2 in IUGLS (2009) for a more complete description of residual and component NBS estimation methods."

The possible effects are now noted as a limitation in the Discussion (second bullet in Section 5) and reference is given to ongoing IUGLS studies specifically focused on NBS estimation.

6. Justify the coefficient 1600 in Eq (3) in line 212.

Eq. 3 is no longer part of the analysis and does not appear in revised report due to the update of component NBS.

7. Justify using 10% overestimation of NBS in line 221.

This section is no longer part of the analysis and does not appear in revised report due to the update of component NBS.

8. In Section 2.5.2, Scenario Analysis of Steady-State Lake Levels, please explain the rational for this analysis. It seems arbitrary and not realistic.

On line 520, this Section now has an intro pgh to do this that reads as follows:

"The purpose of this steady-state analysis was to evaluate the importance of conveyance independent of any temporal variability in all other CGLRRM model inputs. For example, from the analysis in Section 3.3.1, it is not clear if the impacts of conveyance change would continue to increase beyond 2005. The steady-state analysis is one way to ensure the model evaluates the total or long-term impact of a change in St. Clair River conveyance."

9. In Table 5 explain how the column labeled "Fall Difference" is calculated and what significance it has.

Calculation formula made explicit now in the table. See Table 5 at line 540.

10. In lines 586-591, "Metric M4 is defined as the number of years when the average annual MH-Erie lake fall difference is less than or equal to the historically low value of 2.051 m for the measured data in 1990 (see Figure 10). Metric M5 is defined as the number of years when the 3-yr moving average of the MH-Erie lake fall differences is less than or equal to the historically low value of 2.101 m for the measured data in 1991", how can anything be lower than its historically low value. Perhaps it would help to explain if M_4 and M_5 are alternate realizations, how they were calculated, etc.

The word "historically" was removed and the description improved and now reads as follows starting on line 729:
"Metric M4 is defined as the number of years when the average annual head difference is less than or equal to the low value of 2.051 m for the measured data in 1990 (see Figure 7). Metric M5 is defined as the number of years when the 3-yr moving average of the annual head differences is less than or equal to the low value of 2.101 m for the measured data in 1991 (see Figure 7). These values are deemed to be on the basis of the 1963-1998 period in Figure 7."

11. Summarize why the bootstrap method is preferred over Monte Carlo analysis.

On line 673 the text now reads as follows:
"As per the Hydroclimate Uncertainty peer review comments, bootstrap experiments (rather than simple Monte Carlo sampling) are conducted to generate the alternative NBS time series. Monte Carlo sampling is problematic here because it would require developing complex time series models that respect the temporal and spatial correlation structure among all inputs."

12. In addition to the Bootstrap Method, a sensitivity analysis should be performed to evaluate uncertainty.

It is not clear what exactly the reviewer thinks should be analyzed via sensitivity analysis (NBS inputs, Ice and Weed factors, lake-to-lake stage-fall equation coefficients etc.). Unfortunately, not enough time is available for a sensitivity analysis on every input to CGLRRM. Future investigations should evaluate uncertainty of CGLRRM parameters and inputs … however, this is a much larger scale effort than the scope in the current study. Furthermore, any sensitivity and particularly uncertainty study should be undertaken based on the findings of a variety of ongoing IUGLS hydroclimate projects now noted in the Discussion Chapter (Section 5).

13. In lines 668-69, justify the use of 1000input time series. How did you know it was enough?

New bootstrap analyses performed a convergence test to show 1000 was in fact sufficient for desired level of accuracy. See section 4.4 on page 27.

Grammer Corrections

All corrections implemented.

Comments and Responses: Colin Rennie Subproduct Review

Major comments

1) Line 228. The author has attempted to determine the best conveyance equation to use for the 1996-2005 period. It appears that conveyance changed during this period, because no one equation works for both the 1996-2000 and 2001-2005 periods.
Why not use equation 2 for the former period and equation 3 for the latter? This has important consequences for the entire report, because the report concludes that changes in climate have dominated over changes in conveyance as the cause of changes in water level fall between lakes Michigan-Huron and Erie (lines 489 for deterministic, 853-857 for stochastic). These conclusions are based on use of equation 2 as the best equation for the entire 1996-2005 period. However, conveyance changes in 2001-2005 are not captured with this equation, despite an attempt to fit the model (Sections 2.4 and 3.1), which reduces the possible model influence of conveyance changes on lake levels.

> *The old equation 3 was never meant to fit the observed data. Nonetheless, Eq. 3 is removed from the report as it was no longer necessary. The results and analysis in the Section in question have changed due to updated NBS data. However, the revised report explicitly discusses the prediction errors in the 2001-05 period. In particular, the pgh starting at line 345 now addresses this issue and argues that the problem in the model is more than a problem in just the conveyance equation. In summary, the new results and discussion in this pgh make it clear that the model & data divergence in the 2001-2005 period cannot be solely attributed to conveyance change or Equation misfit in this period.*

2) Similarly, it can be argued that these results are predetermined by the choice of conveyance equation used to represent the system. Perhaps the author should also consider use of equation 3 in alternative scenarios for Tables 9 and 10.

> *The old equation 3 was never meant to fit the observed data. Nonetheless, Eq. 3 is removed from the report as it was no longer necessary. The conveyance equation is an approximation and is unfortunately of fixed form in the CGLRRM model. However, the predetermined choice of the conveyance equation form has been agreed upon by the co-ordinating agencies for many years. The constants in the equation were developed via least squares regression of data not used in this report (see Section 3.1 (line 237) for revised presentation of conveyance equations). As such, the constant values were objectively determined relative to the available data.*

3) Ideally, it would be possible to use a different conveyance equation for different years within a simulation set (e.g. in the scenarios shown in Lines 791-798). It appears that this was not possible with the model?

> *See response to comment 1. I agree this would be ideal but the model is not setup for this type of simulation. Such an experiment would not be simple to conduct as*

model runs would have to be awkwardly chained together. But as discussed re comment #1, it is clear the 2001-2005 problems are due to other factors and as such calibrating new conveyance equation to overcome errors that are clearly due to other problems in the model was deemed unnecessary and not helpful if the true sources of the errors were not simultaneously addressed (whatever they may be) .

4) Line 387-390. The author notes that basin supply data are questionable for some periods. This apparent, unexplained, change in lake inputs may be the key to the entire question of change in lake levels. As the author notes, this requires greater exploration.

Agreed. This is why the entire set of report analyses were redone with updated NBS data as of Aug 2009. Note however, the fundamental findings remain unchanged.

5) Line 569. Why is M2s > M2 ? What is M2s (how is it calculated)?

Terminology chained such that s is dropped. Section revised. See pgh at line 721.

6) Line 722. The author has demonstrated that for a fixed conveyance relation (eq.2) random change in basin inputs only rarely will create lake water level conditions seen in 2001-2005. How does this demonstrate that climate variability controls lake level?

The analysis and statement in question were removed from the report.

7) Figure 11. A more rigorous, statistical identification of a change point in NBS would improve this argument.

Agreed and this is why the analysis is removed. Figure no longer appears in the report. New analysis simply isolates 1996-2005 on a much simpler basis and does not refer to two climate eras.

Minor comments

1) Please use hyphens throughout text for lake-to-lake stage-fall-discharge equation.
Done

2) Line 210. Please use "to fit the observed.."
Not applicable. Section removed from report.

3) Line 604. Section 3.3 should precede Section 3.2.
I disagree. Order unchanged.

4) Clean up a few sentences (e.g., lines 636, 682, 781)
Done

Comments on St Clair report relevant to Tolson report:

Loucks comment re pg 165:

> This discussion is entirely devoted to the types of false conclusions that can be drawn from small samples of autocorrelated time series. It would be better to use the context of extreme low supplies and levels to describe how climate has affected the apparent decline in MH to Erie head difference.

Pgh in question:

> "To assess climate and conveyance change in the context of a simulation study in comprehensive and objective manner required sampling these periods separately in a series of bootstrap experiments and incorporating the post-1977 conveyance change. The resulting simulations showed the average drop in head difference is estimated to be 59 percent less when the 1966-1986 climate era is sampled instead of the 1987-2005 climate era. Furthermore, only 1 of 1,000 alternative NBS sequences sampled from 1966-1986 period generated head difference behavior that could be considered equal or more extreme than the historical conditions observed from 1996-2005. Based on these results, it would appear that change in climate era is more than three times as influential as the post-1977 change in conveyance considering their impacts on the magnitude of the drop metrics."

Response:

> *The discussion is eliminated as the report no longer includes the analysis in question.*

Loucks comment re Figure 6-9:

> I assume this figure will be formatted for the report.
>
> *New figure reformatted on Tolson report and available for main report.*

Bonsal comments below:

11. Page 161, paragraph 2, line 3: What does 'in part' mean?

    Suggest simply deleting words 'in part'.

12. Page 162, second bullet near middle of the page: Why is the second period 1996-2005 when on the previous page it was identified as 1988-2006?

    Suggest adding the following underlined sentence to the pgh following the bullet so the entire pgh reads as:

    "Note that the equation for the 1996-2005 period was developed by Environment Canada to specifically represent the current conveyance regime and thus derived on

the basis of recent St.Clair River flow data from 1996-2006. As such, it is not clear the equation represents the conveyance regime before 1996 and therefore, modelling efforts are focused on 1996-2006 period."

13. Page 162, 3rd paragraph from the bottom (and elsewhere): What does 'reasonably' mean? This is not a very scientific term.

    Modelling results are often summarized using the word "reasonable" – usually due to the fact that objective, clear and concise scientific statements summarizing model predictive quality are not available. The word works in the context of the source report where the subjective and visual evidence to justify the word are reported. Perhaps saying something like

    "Tolson (2009) demonstrated that CGLRRM predictions were of reasonably good quality to justify using the model as a tool to investigate …"

Whitfield comments:

In general, Whitfield comments are helpful and they only happen because of the direct transfer of text in the Tolson report to the text of the main report. The sentences in question should be revised so they stand alone in the overall document.