Net Basin Supply Comparison Analysis by Dr. Frank H. Quinn – Peer Review Response

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Overview

The Hydroclimate Technical Work Group Chairs would like to thank Dr. Eric Loucks, Dr. Paul Whitfield, and Dr. Barrie Bonsal for their consolidated review of Dr. Frank Quinn's report (Quinn, 2009a) analyzing Great Lakes net basin supplies. In response to the peer review, Dr. Quinn undertook a substantial revision of his paper (Quinn, 2009b). How each of the review comments was addressed in the revised report is noted in red font following each of the reviewers' remarks in the attached Appendix – Peer Review Findings.

The Chairs would like to note that Dr. Quinn's report is a compendium of numerous reports he prepared and submitted individually. Synthesizing his diverse reports into one report for review may in part have contributed to some of the concerns noted by the reviewers. This approach was an outcome of the exploratory and forensic nature of Dr. Quinn's analysis as he uncovered and identified critical issues with the component and residual net basin supply series. His discoveries resulted in substantial revisions to both data series. Because of the timeline for the accelerated St. Clair River Report, the data revisions were not available until after publication of the draft report. Dr. Quinn has revised his report using the more recently corrected data in addition to the reviewers' comments. His latest findings will be incorporated in the final St. Clair River Report.

The Hydroclimate Technical Work Group Chairs and the author are grateful to the reviewers for their insightful remarks. Their comments have resulted in a revised report that has more clarity, is better defensible and is more comprehensive than the initial draft.

References

Quinn, F.H., 2009a. *Net Basin Supply Comparison Analysis* (dated April 17, 2009). Hydroclimate Technical Work Group Task 2.2, St. Clair River Task Team, International Upper Great Lakes Study,Tecumseh, MI.

Quinn, F.H., 2009b. *Net Basin Supply Comparison Analysis* (dated September 3, 2009). Hydroclimate Technical Work Group Task 2.2, St. Clair River Task Team, International Upper Great Lakes Study,Tecumseh, MI.

Appendix – Peer Review Findings

Manuscript: _Net Basin Supply Comparison Analysis__

Author(s): __ Frank H. Quinn

Name of Reviewers: __Eric Loucks, Paul Whitfield, Barrie Bonsai_

1. Are the objectives of the work clearly stated?
   - The stated purpose of the analysis is to compare the residual and component methods for computing Net Basin Supply (NBS), however, little background is given to set the context for why this is important and what are the advantages/disadvantages of each method. No information is provided as to how the methods will be evaluated and what criteria will be used to select a recommended approach.
     The Executive Summary and Introduction were expanded to address the relevance and importance of this analysis to the St. Clair River Study science questions. New sections on Purpose and Methodology were added to the report that includes additional background information on the advantages/disadvantages of each method of computing net basin supplies. Information was provided as to how the methods are evaluated and the criteria used to select the recommended approach in the body of the report.

2. Are the methods employed valid, appropriate and sufficient to address the questions, hypotheses or the problem?
   - The questions and hypotheses are not clearly defined, so it is impossible to assess whether the methods are appropriate. The methods used in the analyses are in some cases appropriate, but not in others (e.g., the use of parametric t-tests and f-tests without first testing for normality).
     The questions and hypotheses are addressed in the Purpose and Methodology sections added to the revised report. Dr. Quinn tests for normality and includes the results in a new Appendix 1, confirming normal distributions of the net basin supplies.

3. Are the observations, conclusions and recommendations supported by the material presented in the manuscript (e.g., data, model and analyses)?
   - The conclusions and recommendations are rather limited in scope, but are for the most part, consistent with the analysis. However, these conclusions could have been reached with a much simpler analysis.
   - In cases where the methods differ significantly, there is no recommendation as to which one is more accurate
   - The inherent flaws in each method should be presented in some fashion and then correlated with the analysis results
     The intent of Dr. Quinn's original studies was exploratory and forensic in nature. He was commissioned to review the net basin supply series using a variety of approaches to provide quality assurance of the data. Hence, there was not an expectation of far reaching scientific conclusions or recommendations. Nevertheless, his work identified serious computational problems in both data series and yielded key results that corroborated other Study findings using different modeling approaches. In his

revised report, he makes recommendations as to which net basin supply series (residual or component) is most appropriate for climate analysis and regulation plan formulation and addresses their relative accuracy and uncertainty. He logically presents the flaws in each method and substantiates them with the results of simple, but direct analyses.

4. Are the assumptions used valid and are the mathematics presented correct?
   - A few limitations of the analysis are stated but no underlying assumptions. Statistical tests (Students t, and F-test) are presented on pages 14, 15, 37, 40, 41, 50 and 59 which require population normality. This assumption is neither stated nor verified. Correlation coefficients are used without specifying the sample size or significance level. The analysis period is frequently broken up into sub-periods with no statement of the purpose or basis of this segmentation. The implication is that the methods of measuring or deriving the underlying data changed from time to time but this is not discussed.
     In his revised report, Dr. Quinn addresses the issue of normality and provides results in Appendix 1 of the report. He also includes information on sample size for the correlation coefficients and provides the rationale for selecting sub-periods of the data set for separate analysis. Although the reviewers comments were considered regarding specifying the correlation coefficients' significance level, we could not find methods for doing so in the statistical literature or in consultation with statistical experts associated with the Study.

5. Is the manuscript well organized, material precise and to the point, and clearly written using correct grammar and syntax?
   - Difficult to assess given the lack of clear purpose and methodology. Report presents and describes a large amount of data but leaves the reader wondering what it all means. For example, eleven graphs are provided on Pages 21-27 with little or no discussion of what they mean.
     Dr. Quinn's revised report has added sections on Purpose and Methodology to address the reviewers' comments. He has also greatly re-organized the report, including sections for each lake (Superior, Michigan-Huron, and Erie) that synthesize the findings of his individual reports. He has moved the detailed analysis and graphs to 3 appendices (Appendix 2-4). He includes recommendations and conclusions so the reader has a clear understanding of the report's findings.

6. Are all of the figures and tables useful, clear, and necessary?
   - All of the figures on pages 31 through 47 have severe formatting problems that make them difficult to read. Suggestions for improvements to the Figures are provided in Specific Comments below. Many tables are poorly labeled such as Table 1. It would be helpful to use line work highlight summary information (totals, means statistics) in tables.
     Dr. Quinn has revised the tables and figures as recommended, to the extent possible with the software available to him. He presently does not have access to manuscript preparation services or editorial support. Figures and tables to be included in the final St. Clair River Report are being prepared to

the report editor's specifications and standards. The tables and figures have been revised as noted in the Specific Comments.

7. What is the quality of the overall work?
   - With the flaws in the approach and analysis, we are not ready to accept the conclusions as offered without additional work. From what has been shown graphically, it is clear that there are important differences and there is a seasonal bias between the two methods of calculating Net Basin Supply. The analysis of change in storage is primitive and needs to be more highly resolved in time and space. The author should consider the potential impact of Atlantic Multidecadal Oscillation or the PDO in this analysis. In any case, there needs to be a more thorough analysis to better resolve the biases and differences that exist between the two methods of calculation.
     Dr. Quinn has undertaken a substantial revision of his report to address the reviewers' concerns. We acknowledge the reviewers' concerns regarding a more highly resolved analysis of the change in storage, but such an analysis is beyond the scope of work and funding originally presented to Dr. Quinn. To better resolve the change in storage in space and time would require extensive dynamic numerical modeling supported by much more highly resolved lake level data than is readily available.

     The reviewers' recommendation to address the Atlantic Multi-decadal Oscillation or the Pacific Decadal Oscillation in this report is also acknowledged and is being addressed under a separate task in the Hydroclimate Technical Work Group by Dr. Taha Ouarda. His preliminary report has been received, but the acceleration of the St. Clair Report timeline did not permit it to be integrated with Dr. Quinn's

Specific Comments:

1. Page 1 Paragraph 1. A more fulsome definition and explanation of Net Basin Supply is needed.
   The text was expanded to better define and explain Net Basin Supply.

2. Comment: after the one paragraph "executive summary" there should be inserted an introduction that addresses the comments above and gives the reader a guide to what will follow. It should state the objective and describe the problem. It would also be the place to put a 'map' that shows the place names that are used for the readers reference.
   Purpose and Methodology sections were added, and the Executive Summary enhanced to provide guides to the reader on what will follow.

3. paragraph 2 line 7. The expression 'good agreement' is used without a precise definition. I also found that the text is written from an insider's point of view. While most people know the Great Lakes the connecting channels are less well known. It the names of the rivers are to be used a figure would be a good addition.
   "Good agreement" was qualitatively defined in this report to mean where using either of the two net basins supply series would yield the same results given their inherent uncertainty. A figure of the Great Lakes system with the Connecting Channels names has been included in the report.

4. Paragraph 3 – high correlations are uninformative. While I would agree that they should use all the water level gauges, some accurate measures of movement of water between lakes would be much better. I found this entire paragraph to be rather weak.
   While the author recognizes that high correlations in some circumstances are uninformative, here the high correlation between the change in storage and the differences between the residual and component supplies suggests a causal relationship. Highly accurate measures of the movement of water between the lakes (Connecting Channel flows) are not available and are indeed one of the driving factors behind the St. Clair River Study.

5. Page 2 paragraph 3 "… is about as good as one could expect …" is not quantitative.
   This text was removed.

6. Page 5 "OGOKI" and "Ogoki" are equivalent? The term is used before being defined. Similarly, a definition and discussion of the effects of thermal expansion would be helpful.
   "OGOKI" and "Ogoki" are equivalent and will be corrected in the final report. The author provides a reference to Meredith (1975) that defines and discusses the effects of thermal expansion for the reader unfamiliar with this phenomenon.

7. Page 6. The mention of Sharepoint files is out of context and should be defined or deleted.
   A discussion of the data source and version date has been added in the Methods section.

8. Page 6. It seems odd to consider all the components as CMS when not one of these is measured in these units. Figures 1 – 4 would benefit from confidence limits and/or error bars. This is another place where an error analysis would be a useful addition. The

average monthly precipitation and evaporation as CMS needs to be more fully described, and the confidence limits shown on the plots. Figure 3 is captioned 'residual' but labeled as an average annual rate?
Expression of overlake precipitation, basin runoff, and lake evaporation are typically expressed as flow rates or as depths over the lake surface. They are expressed this way so that they can be plotted using the same y-axis units and are directly additive. The error analysis is being conducted by Dr. Carlo DeMarchi, and he provides plots of these figures with confidence limits/error bars. Because of the time constraints of the accelerated Study, Dr. DeMarchi conducted his work in parallel with that of Dr. Quinn's, and his results were not available for inclusion in Dr. Quinn's report. In Figure 3, the term 'residual' refers to the net basin supplies computed using the residual method, correctly expressed as an annual average rate.

9. It might be worth commenting on the impact of winter severity (that affects ice duration) on the variability of annual and monthly evaporation values reported for each of the lakes analyzed (i.e., Table 1 in each section).
The effect of winter severity as measured by extent of ice cover and seasonal duration is being addressed by Mr. Ray Assel, former principal investigator of the Great Lakes Environmental Research Laboratory and recognized ice expert. His work was also conducted in parallel to that of Dr. Quinn's and was not available to Dr. Quinn at that time.

10. Page 9 – change in storage is a volume not a flux.
While the change in storage is indeed a volume, it is customarily expressed as a flux in units of cms when calculated on a monthly basis. As noted earlier, they are expressed this way so that they can be considered in the same units as other components and are directly additive.

11. Page 9 Figure 5 and paragraph 1. I find it difficult to accept that this is a surprise. It should approximate the root mean square of the other components quite well – which it appears to do.
The comment is noted and the text changed to address this observation.

12. Text referring to Figure 6 should note the increase in variability after 1995+/-.
The comment is noted and the text changed to address this observation.

13. The y-axis scale on some of the Figures is too large to see the variation of the variable being shown. This includes Figures 7 & 19 (Lake Superior), Figures 7, 21, 22, & 24 (Lake Michigan-Huron) and Figures 7 & 14 to 17 (Lake Erie).
The suggestion is noted and the figures changed.

14. In Figure 8 on page 11, the residual minus component NBS has almost the exact variability as evaporation. This is corroborated by the high correlation coefficients in Table 2. Can you comment on why this is the case?
The suggestion is noted and the text changed to elaborate/speculate on why this is the case.

15. Page 11 – Figure 9 – where does the "-1600" come from? This needs some explanation. A simple test would be to plot P-1600 against R and look for a linear fit. The close fit

between 1955 and 1965 aside, the curves are quite different.
This figure and associated discussion was removed in the revised report based on the corrected data.

16. Page 12, paragraph 3: It is not entirely clear how the preceding analysis warranted the breaking of the time series into segments of 1948-1966, 1967-2000, and 2001-2006. Can this be elaborated? The same can be said for Lake Michigan-Huron on page 36 and Lake Erie on page 56.
    Analysis of the revised data resulted in different conclusions being drawn. The text was revised with the new findings.

17. Figures 11 – 13 would be better in chronological order. I would offer that these plots are very reminiscent of early climate change detection work where the monthly time step shows a difference but it is not significant because of the averaging properties and timing shifts. It is clear that some view of field theory needs to be considered as all three plots show month to month persistence in sign.
    Suggestion noted and the figures removed.

18. Page 14 Table 3 5 – units? There was not a very clear description of the testing, but almost every assumption of the t-test will be violated; without a description of the f test and without unit one can only guess at a variance ratio, but in any case I would expect that whatever version of the f test was used will not be appropriate as these data are not iid. The author should consider more robust methods, perhaps non-parametric ones, that do not make strong assumptions about distributions, normality, and independence.
    Units were added and the text changed.

19. Page 15 – paragraph 1. This description is insufficiently clear. It should be more specific about the assumptions and linked to improved Tables. Without some definition of what is in these tables the reader has to guessing. For example in Table 23 January my guess is that the average different between the two methods is 827 with a standard deviation of 694. If one considers the confidence interval is 1.96 * STD then the ave is not different from zero – but the t-test, assuming this is the probability of t would indicate otherwise.
    The suggestion is noted and the text changed.

20. Tables 6 and 7 should be replaced with plots of one against the other [x:y] – correlation is not an appropriate method. If correlation coefficients are presented, a complete correlation matrix indicating the cross-covariance of all variables should be provided.
    The tables were removed and the suggested figures added.

21. Page 16. The section on “impact of a standard month” needs more explanation.
    To assist the reader, this is addressed earlier in the revised report.

22. Page 19. Paragraph 1 – no reference for Meridith provided
    The reference has been added.

23. Last two sentences on page 19 seem to refute each other. If thermal expansion will be important in the future should something be done starting in 1965?
    The text was changed to clarify and address the reviewers’ comments.

24. Page 22- 25 The logic of this uncertainty/error analysis needs to be more clearly presented. The first paragraph on page 22 might be what was actually done, but that logic seems incorrect. The confidence limits around the mean for a normal distribution is 1.96 * Standard Deviation. This text seems to say exactly the opposite. There are more effective ways of considering the differences between these two series and these should be pursued. In many of these figures the series is consistently outside the uncertainty envelope, and one would expect this has some seasonal component based upon the earlier figures.
The uncertainty/error section was revised to address the reviewers' comments.

25. Figure 6 on page 34: The extreme low value (near 5000 cms) near the end of the time series may be worth mentioning
This figure was corrected based on the revised data.

26. Page 48, Figure 25: Should the caption read component NBS uncertainty (as opposed to residual)?
The uncertainty/error section was revised to address the reviewers' comments.

27. Page 48, 1st paragraph of the Conclusions, lines 4-7. The sentence starting 'The largest differences…' is unclear and should be reworded.
The suggestion was noted and the text changed.