

Manuscript: Net Basin Supply Comparison Analysis

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Name of Reviewers: Eric Loucks, Paul Whitfield, Barrie Bonsai

1. Are the objectives of the work clearly stated? **2**
 - 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.

2. Are the methods employed valid, appropriate and sufficient to address the questions, hypotheses or the problem? **3**
 - 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).

3. Are the observations, conclusions and recommendations supported by the material presented in the manuscript (e.g., data, model and analyses)? **3**
 - 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

4. Are the assumptions used valid and are the mathematics presented correct? **3**
 - 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 subperiods 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.

5. Is the manuscript well organized, material precise and to the point, and clearly written using correct grammar and syntax? **3**
 - 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.

6. Are all of the figures and tables useful, clear, and necessary? **4**
- 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
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.

Recommendation

C

Rating

50

Signature: Eric Loucks (US), Barrie Bonsai and Paul Whitfield (Canada)

Date: June 6, 2009

Specific Comments:

1. Page 1 Paragraph 1. A more fulsome definition and explanation of Net Basin Supply is needed.
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.
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. If the names of the rivers are to be used a figure would be a good addition.
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.
5. Page 2 paragraph 3 “... is about as good as one could expect ...” is not quantitative.
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.
7. Page 6. The mention of Sharepoint files is out of context and should be defined or deleted.
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 labelled as an average annual 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).
10. Page 9 – change in storage is a volume not a flux.
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.

12. Text referring to Figure 6 should note the increase in variability after 1995+/-.
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).
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?
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.
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.
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.
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.
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.
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.
21. Page 16. The section on “impact of a standard month” needs more explanation.

22. Page 19. Paragraph 1 – no reference for Meridith provided
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?
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 $1.96 * \text{Standard Deviation}$. 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.
25. Figure 6 on page 34: The extreme low value (near 5000 cms) near the end of the time series may be worth mentioning
26. Page 48, Figure 25: Should the caption read component NBS uncertainty (as opposed to residual)?
27. Page 48, 1st paragraph of the Conclusions, lines 4-7. The sentence starting ‘The largest differences...’ is unclear and should be reworded.