

General comments

This is my second review of this paper which describes the development of a modelling system to estimate net basin supply to a strategic Canada/USA water body: Lake Ontario. In the following Specific comments Section, I believe that the comments in italic were not considered since the authors did not provide a rebuttal.

In my first review there were three basic objectives:

- (i) “propose a methodology for calibrating the distributed GEM-Hydro platform developed by ECCC in order to improve streamflow simulations for Lake Ontario, which we expect would ultimately propagate into improved simulations of Lake Ontario Net Basin Supplies (or NBS, the sum of lake tributary runoff, overlake precipitation, and overlake evaporation: Brinkmann 1983);
- (ii) compare GEM-Hydro with two other distributed models (inter-comparison study) in order to identify avenues to further improve GEM-Hydro; and
- (iii) propose and evaluate a method for estimating runoff for the ungauged parts of the watershed.”

In this new version, I still find three stated objectives:

- (i) P.2, lines 16-17: « One of this paper's objectives is to present the first evaluation of the capabilities of the new SVS scheme for hydrological prediction in Canada
- (ii) P.4, line 5-7: « ...this study mainly aims at finding a methodology to implement the distributed GEM-Hydro model over the whole Lake Ontario watershed, including its ungauged parts, in an efficient manner. » - by the way, we do not find a methodology...we develop one – please consider replacing accordingly!
- (iii) P.4, lines 10-11: A second objective is to compare GEM-Hydro with two other distributed models (which is this study's contribution to GRIP-O) in order to identify avenues to further improve GEM-Hydro. »

It seems the new objective (ii) is a combination of the previous objectives (i) and (iii) minus evaluation of NBS with respect to those currently available. This answers one of my previous comments.

As it is the paper reads more like a technical report than a scientific paper. I would have preferred a scientific paper that provides more fundamental information between the computational time scales of the LSS and those of WATROUTE and the UH. *For example, discuss the relationship between the computational time scales and the dimension of the computational elements used in WATROUTE and the UH versus those used in the LSS.*

I encourage the authors to consider the comments introduced in my review as I feel the paper represents a good technological contribution to the hydrometeorological community.

Specific comments

- *P.1. consider modifying the end of the title as follows: ...on the Lake Ontario basin, Canada*
 - *Please be consistent and use only one nomenclature, replace « watershed » by « basin » throughout the manuscript. Do not alternatively use catchment, basin and drainage area as synonymous. I know they are, but in a scientific document, it more conventional to use only terminology and since basin is used more than once in the manuscript, stick to it.*
- *P.4, Section 1.5: please correct me if I am wrong, but WATFLOOD has no LSS, just a simple potential evapotranspiration equation, unless WATCLASS was used. So WATFLOOD is more along the line of GR4J with that respect.*
- *The following comment introduced in my first review should be addressed : I think there is room here to provide more fundamental information between the computational time scales of the LSS and those of WATROUTE and UH. Furthermore, discuss the relationship between the computational time scales and the dimension of the computational elements used in WATROUTE and the UH.*
- *P.5, line 34 and P.6, lines and 4, please be consistent use either « computation time » or « computational time », not both.*
- *P.8, equation (1): why presenting the PBIAS expression and not the NS...the latter being more complex than the former...*
- *P.11, lines 3-6: the equifinality problem still exists for the global calibration, please discuss? Despite global calibration is not be exempt of equifinality, the attention paid to the parameter ranges used (Table 3) allows to be confident in the physical relevance of the final parameter values.*
- *P. 12, line 20: Please Moira river should be written as Moira River throughout the manuscript, please be consistent*
- *P.12, line 34: the acronyms for Nash-Sutcliffe Coefficient using logarithmic values of streamflows are here NSE and NSE Ln, however, in some of the tables and Figures, the following acronyms Nash and Nash Ln are used, please use only one set of acronyms throughout the manuscript, not two.*
- *A similar comment applies for Pbias and PBIAS throughout the manuscript.*

- P.13, lines 1 and 20 and in some of the tables and figures: the acronym for Nash-Sutcliffe Coefficient using square root values of streamflows are here Nash $\sqrt{\quad}$ and NSE $\sqrt{\quad}$, please be consistent and use only one acronym throughout the manuscript, not two.
- P.17, lines 16-17: « However, as a limited number of subbasins were used for the inter-comparison due to computational time limitations, no general model ranking can be derived from this study. ».
 - *This means perhaps this paper is premature. Or as mentioned in the general comment section. Model intercomparison should be considered as supplemental information.*
- *I still do not get it, perhaps WATROUTE needs to be calibrated separately otherwise why calibrating with the UH? It is only valid to use WATROUTE if it can reproduce the UH at the chosen outlets used for the UH calibration. Unless there is a philosophical point I am not getting, which is perhaps possible, but doubtful. Please make a strong rebuttal to this statement.*

Figures

- There are two « Figure 3 », hence the second Figure 3 should be Figure 4, and Figures 4 and 5 should be Figures 5 and 6, respectively.
 - Replace sub-catchment by sub-basin, please be consistent.

Tables

- Tables 3, 4 and 5
 - The range for some parameter values defies the imagination, any explanations?

Answer to traditional questions

Is the paper free of errors in logic?

- Yes

Do the conclusions follow from the evidence?

- Yes.

Are alternative explanations explored as appropriate?

- Yes.

Are biases, limitations, and assumptions clearly stated, and uncertainty quantified?

- Yes.

Is methodology explained in sufficient detail so that the paper's scientific conclusions could be tested by others?

- I am not sure

Is previous work and current understanding cited and represented correctly?

- Yes.

Is information conveyed clearly enough to be understood by the typical reader?

- Yes and no – I still have some minor issue related to the local and global calibration strategy

Are all figures and tables necessary, appropriate, legible, and annotated (as appropriate)?

- Yes.