Hydrol. Earth Syst. Sci. Discuss.

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Title: A Hydrological Prediction System Based on the SVS Land-Surface Scheme: Implementation and Evaluation of the GEM-Hydro platform on the watershed of Lake Ontario

General comments

This is an interesting paper describing the development of a modelling system to estimate net basin supply to a strategic Canada/USA water body: Lake Ontario. From a technological point of view, the work is state of the art and clearly highlights recent efforts undertaken by Environment and Climate Change Canada to develop a robust hydrological modelling system. More specifically, the objectives of this paper are to:

(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.''

Objective (i)

As far as I can get, the paper never actually demonstrated what is highlighted in yellow. So unless the reader is an insider, there is no evidence that currently applied modelling systems do not provide satisfactorily the sought-after streamflow simulations. I understand with the interest of developing a state-of-the-art modelling system, but the paper does not provide a strong motivation. The authors need to convince the readers here.

Objective (ii)

After reading the paper a couple of times, I feel the paper has yet to actually and clearly identify the avenues to further improve GEM-Hydro. The authors mentioned that SVS would benefit from a soil heat balance equation based on one missed spring peakflow; that is farfetched as it could have resulted from multiple sources, so I believe the paper does not make a strong case here. Now, I am not sure the reader will get anything out of this objective and, at best, I think the model intercomparison should be considered as supplemental material. Furthermore, neither the abstract, nor the conclusion, provide any tangible answers to this objective.

The above comments might seem harsh, but they are factual. Do not get me wrong I think the paper reports relevant technological information to the hydrometeorological community. The paper shows promises and I believe the authors can streamline the content to the essentials, that is the demonstration of the advantage of substituting a unit hydrograph for WATROUTE during calibration can actually reduce the computational time required for model calibration and illustrate that GEM-Hydro can benefit from a local/global calibration strategy to provide '' good streamflow predictions ''. Should the authors decide to follow this suggestion, they should substantiate their rationale from a scientific point of view rather than a technological one. For example, they should provide more fundamental information between the computational time scales of the LSS and those of WATROUTE and UH. Furthermore, they should 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.

It is noteworthy, Section 1.4. is confusing. In fact, although I am somewhat familiar with the work of the first author on local and global calibration, but I needed to read the section more than once to comprehend; and even then, I am still not sure what was actually done. I do not quite follow the use of GR4J in the calibration of GEM-Hydro-UH (see my comment below related to the content of P.10, lines 15-17) because the paper does not provide the underlying hypothesis. I strongly recommend the authors to provide a diagram or sketch describing the steps taken to achieve the calibration strategy introduced in Section 1.4. At this point, I doubt that most readers can appreciate what the authors actually did.

I have made suggestions in the following list of specific comments below on how to fulfill what I perceive as shortcomings. I have also added a few editorial comments to improve the paper. As a side note, I found a bit difficult the exercise of going back and forth between the content of the introductory sections to remind myself what the acronyms meant. The authors should provide a list of acronyms to facilitate the reading of the manuscript.

I strongly encourage the authors to address these comments as I feel the paper could certainly be a good technological contribution to the hydrometeorological community.

Specific comments

- P. 2, line 10: the following sentence:
    - « Going beyond anomaly forecasts (which are bias corrected based on a model climatology) to obtain unbiased short-term streamflow forecasts is more challenging due to limitations of operational Land-Surface Schemes (LSS), which are generally geared towards improving weather forecasts, sometimes at the cost of not representing (or misrepresenting) surface and subsurface hydrological processes that are critical to hydrological simulation. »
        - …should be modified as follows:

- Going beyond anomaly forecasts, which are bias-corrected based on a modeled climatology to obtain unbiased short-term streamflow forecasts, is more challenging. This is due to limitations of operational Land-Surface Schemes (LSS), which are generally geared towards improving weather forecasts, sometimes at the cost of not representing (or misrepresenting) critical surface and subsurface hydrological processes.

- P.2, line 15: the following sentences:
  - Hydrological processes in land-surface models used for NWP are improving quickly (Balsamo et al., 2009; Masson et al., 2013; Alavi et al., 2016; Wagner et al., 2016), as soil water content and snow are recognized as important sources of their predictability that remain to be fully tapped into (Koster et al., 2004; Entekhabi et al., 2010). Environment and Climate Change Canada (ECCC), the Canadian department that provides operational weather and environmental forecasts, is in the process of implementing a major upgrade to the LSS used by its NWP model, the Global Environmental Multi-scale model (GEM)
    - …should be modified as follows:
      - Hydrological processes simulated by land-surface schemes (LSS) used for NWP are improving quickly (Balsamo et al., 2009; Masson et al., 2013; Alavi et al., 2016; Wagner et al., 2016), as soil water content and snow water equivalent are recognized as key state variables for streamflow forecasting (Koster et al., 2004; Entekhabi et al., 2010). Environment and Climate Change Canada (ECCC), which provides operational weather and environmental forecasts within its boundary, is in the process of implementing a major upgrade to the LSS of the Global Environmental Multi-scale model (GEM), the national model.
- P. 2, line 20: please delete « …in order… »
- P.3, line 6: please modify as follows: « …thermodynamics, as reported by Wiley… »
- P.5, line 6: 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.
- P.5, lines 5 through 34, 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.6, line 10: what does SA mean in SA-MESH?
- P.6, line 21: please replace «… the outlet of Lake Ontario.» by «..the Lake outlet. »

- P.7, line 10: please replace « … that it is higher than 1 m.. » by « … that it is greater than 1 m.. »
- P.7, line 20: is there any spin-up for the calibration period?
- P.7, equation (1): why presenting the PBIAS expression and not the NS...the latter being more complex than the former…
- P.8, line 3:
    - Please specify those GEM-Hydro-UH GRIP-O sub-basins that were locally calibrated out of all sub-basins?
        - What is the the percentage of the Lake Ontario basin that had local model calibration? Is it 88.5% (P.9, line 15) ?
- P.9, line 1: «…some subbasins in Fig. 1 have several gauge stations. »
    - It would help if these gauge stations could be displayed, but I assume it might not be feasible given the coarse resolution of this figure.
- P.10, lines 1-9: the equifinality problem still exists for the global calibration, please discuss?
- P.10, line 10: What does a unique implementation mean? It is not clear. I assume GR4J was first calibrated on each gauged sub-basin, then the global calibration took place and a single parameter set was found. Please define unique.
- P.10, line 16: What do you mean here: « …performances obtained with local Gr4J calibrations (Gaborit et al., in Press) were used when needed…».
    - Do you mean that for those sub-basins not modelled by GEM-Hydro-UH, the performances of GR4J were substituted in the computation of the objective function (Eq. 2)?
    - The hypothesis behind this approach must be clearly stated in the paper; that is it is assumed that GEM-Hydro-UH would have a similar performance, am I right?
- P.10, lines 15-17: « However, as GEM-Hydro-UH was not locally calibrated for all of the 14 GRIP-O subbasins, performances obtained with local GR4J calibrations (Gaborit et al., in Press) were used when needed (justifying the use of that model in this study). »
    - How was this done? Please provide a quick summary so the reader doesn't have to access the reference.
- P.10, line 21: it is not arbitrary if it is based on prior work!
- P.10, line 32 & P.11, lines 1 & 12: Watroute should be written with capital letters (WATROUTE).
- P.11, line 23: replace « …which…» by « …whose…»
- P.13, line 8: Please be consistent and replace watershed by basin.
- P.15, lines 20-21: « 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.

- P.16, lines 4-5: 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.
- P.16, line 17: please replace No. XXXX
- P. 17- 20: In the References section, there are several references with « …, », please fill them in.

Figures

- Figure1
  - The word « areas » should be replaced by sub-bassins, drainage areas or basins.
  - In the figure caption, replace sub-catchment by sub-basin, please be consistent.
- Figure 2
  - Moira river (CA) should be replaced by Moira River (CAN). CA usually stands for California.
  - Please remind the reader that the Moira River basin is sub-basin 11.
- Figure 3
  - Correct me if I am mistaken, but shouldn't the caption be as follows: Uncalibrated GEM-Hydro and GEM Hydro-UH performances…
  - Wouldn't be interesting to discuss the differences, at least for one or two sub-basins?
- Figure 4
  - Replace sub-catchment by sub-basin, please be consistent.
- Figure 5
  - Please use upper case letters for Mesh (MESH), Watflood (WATFLOOD), please be consistent.
- Figure 6
  - Why are not there any local calibration for sub-basins 13, 14 and 15
  - What are the default parameter values when compared to those resulting from the calibration procedure, local and global? Wouldn't be interesting to discuss the differences, at least for one or two sub-basins?
  - Please add the following precisions to the figure caption (at least that is my assumption): Results are presented as NSE √ (left) and PBIAS (right), for many GRIP-O sub-basins.
- Figure 8
  - Cumulative monthly NBS cannot by definition flow rate units, the units here should be cubic meters.
  - What are the numbers 7, 11 and 3 on the x-axis? Sub-basins number? I assume so as there are 14 tick marks between the occurrences of the number 7. Please provide this information in the figure caption.

Tables

- Table 4
  - The range for some parameter values defies the imagination, or any explanations?
- Table 6
  - To avoid any confusion please substitute CA for CAN, which is more often used - otherwise CA usually refers to California

Answer to traditional questions

Is the paper free of errors in logic?

- Yes

Do the conclusions follow from the evidence?

- Yes and no – see general comments.

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?

- No, see the above list of general comments.

Is previous work and current understanding cited and represented correctly?

- No, see the above list of general comments about local and global calibration strategy.

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

- Yes and no – see general comment about local and global calibration strategy

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

- Yes and no – see aforementioned comments