Hydrol. Earth Syst. Sci. Discuss., 3, S1548–S1552, 2006
www.hydrol-earth-syst-sci-discuss.net/3/S1548/2006/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Using the MESH modelling system for hydrological ensemble forecasting of the Laurentian Great Lakes at the regional scale" *by* A. Pietroniro et al.

A. Pietroniro et al.

Received and published: 22 November 2006

> 1. The title of the manuscript suggests that the paper emphasizes the methodology and results of the forecasts, but detailed description of the system, the great lake basin and the atmospheric forcing took too much space and it could distract the readers from the main focus. I suggest that these materials be condensed and some details omited. On the other hand, some key points of the system is missing.

The title has been changed to more accurately reflect the purpose of the paper. The authors recognize that the paper focuses on the development of our Canadian Hydrological Ensemble systems

> 2. For example, it is not clearly stated whether the SWAT model and the hydrological model are coupled in 1 way or 2 way mode, although they are said to be "tightly integrated" (p2478 line 7 from bottom). Although "atmospheric forcing" from models is described in length in section 4, "meterological forcing was derived from observed synoptic stations with in the basin" in streamflow simulation (section 6.2) without discussion.

Original text was changed to address the concerns:

Original text “As shown by Fig. 1, the land-surface and hydrological models are tightly integrated, running on the same grid. The land-surface model can then be coupled to an atmospheric model through the coupler (the online mode), or MEC can read atmospheric forcings in from files (the offline mode).”

Modified text “As shown by Fig.1 , the land-surface and hydrological models are tightly integrated, running on the same grid. The land-surface model can then be coupled to an atmospheric model through the coupler (the online mode), or MEC can read atmospheric forcings in from files (the offline mode). Our streamflow and SWE simulations were performed offline using gridded hourly precipitation and temperature data.

In section 6.2 we change meteorological forcing to be atmospheric forcing since in our paper they were used interchangeably. We checked the entire text to ensure we were more consistent with our terminology.

> Discussion about the stream flow simulation and forecast results seems too brief. For instance, fig. 8 is only mentioned once in the text without comments. While the general agreement between simulation and observation can be noted, the large discrepancy in streamflow for a number of periods at some stations should be addressed.

The authors agree and the following text was added to that paragraph to at least elucidate our current thinking on this.

We added the text “It is clear that overall simulations look quite reasonable however

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there are certainly peak flows that are not captured in the individual gauge simulations. Also evident in figure are the spring flows in 2001 where simulated peaks are higher than what was observed. This may be due to the fact that small control structures are not accounted for when we operate the model at the scale and resolution we have developed in this study. Of course there are many other issues such as uncertainties in observed precipitation and model uncertainty that need to be examined. What is encouraging is that with minimum calibration, we were able to achieve reasonable hydrograph simulation at a regional scale for a number of locations. Further investigations are still required and will be attempted in a systematic way as we further develop the system. What is interesting is that these large scale simulations performed at at a reasonable level of resolution allow us to determine specific geographic areas of concern where the system is failing."

> In Fig.9, everybody agrees that "the general patterns and seasonalities of the lake levels for all five Great Lakes are well simulated (P2943, line 1)", the almost exact match for Lake Erie and the substantial under-estimation in the simulations at Lakes Superior and Ontario should be mentioned even if they can not be explained at this stage.

The authors agree and the following text was added.

Clearly, though some issues require resolution, the general patterns and seasonality of the lake levels for all five Great Lakes are well simulated. It should be noted that lake evaporation is based on climatological estimates derived from GLERL, and that precipitation over lakes is based solely upon the synoptic observations interpolated for the MESH domain. This may explain some of the variations observed in the model as compared to observations. It should be also noted that Lake Ontario has some measure of regulation that has not been accounted for in this model. The discrepancies in Lake Superior are more problematic and will require further attention.

> The caption for Fig.10 is too simple for a reader to understand the plottings. In

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addition, each panel of the figure can be reformatted to focus on the period and range of water level so the difference among ensemble members and that between forecast and observation acn be clearly displayed.

The caption in Figure 10 was changed and the figure was divided into 2 separate figures. The changes are noted below.

Figure 10 - Deterministic lake level estimates from the WATFLOOD simulation for August 2003. The grey solid lines represent measured lake levels, the dashed grey line is the 5-day moving average. The black line is the WATFLOOD simulation

Figure 11 - Ensemble lake level estimates from the WATFLOOD simulation for August 10, 2003. The grey solid lines represent measured lake levels, the dashed grey line is the 5-day moving average. The black lines are the WATFLOOD ensemble simulation

Figure 12 - Ensemble lake level estimates from the WATFLOOD simulation for August 14, 2003. The grey solid lines represent measured lake levels, the dashed grey line is the 5-day moving average. The black lines are the WATFLOOD ensemble simulation

> The conclusion section of the manuscript seems out of the topic, without summarizing the success and/or weakness of the system, and approaches to improve the simulation and forecasts. This is partially due to insufficient discussions of the results in section 6 and, partly to the deviation of emphasize from "forecast using the MESH system" to MESH itself.

The authors agree, however, the tone of the paper has been changed to better reflect our intent. There are likely a plethora of issues to deal with as we move to operationalize this system for use, however, we feel that our early results were quite encouraging given the relatively small manual calibration and the complexity of the system we are dealing with. The conclusions were slightly modified to better reflect this reality.

> In addition to the minor modifications suggested and type errors pointed out by other readers, please also note the following:

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> 1) p2488, line 17, the meaning of "densities" is not clear to me.

- CORRECTED - should have read and land-cover.

> 2) P2489 line 10 , An more

- CORRECTED

> 3) P2492, line 9, “The could”

- CORRECTED

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 2473, 2006.